CHEMICAL HERITAGE FOUNDATION

PAUL M. DOTY

Transcript of an Interview
Conducted by

Raymond C. Ferguson

at

Harvard University

on

17 November 1986

(With Subsequent Additions and Deletions)

THE BECKMAN CENTER FOR THE HISTORY OF CHEMISTRY

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(Date) Sept. 5, 1994

(Revised 24 February 1988)

Chemical Heritage Foundation
Oral History Program
315 Chestnut Street
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PAUL M. DOTY

1920 Born in Charleston, West Virginia, on 1 June

Education

1941 B.S., chemistry, Pennsylvania State College
1944 Ph.D., chemistry, Columbia University

Professional Experience

Polytechnic Institute of Brooklyn
1943-1945 Instructor, Research Associate and Co-Director of Quartermaster Projects
1945-1946 Assistant Professor of Physical Chemistry

1947-1948 University of Notre Dame
Assistant Professor of Chemistry

Harvard University
1948-1950 Assistant Professor of Chemistry
1950-1956 Associate Professor of Chemistry
1956-1968 Professor of Chemistry
1967-1970 Chairman, Department of Biochemistry and Molecular Biology

1968- Mallinckrodt Professor of Biochemistry

Honors

1946-1947 Rockefeller Fellow, Cambridge University, England
1950-1951 Guggenheim Fellow, held in 1958, Cambridge University
1950 Fellow, American Academy of Arts and Sciences
1955 Priestly Lecturer, Pennsylvania State University
1956 Award in Pure Chemistry, American Chemical Society
1956 Edgar Fahs Smith Lecturer, University of Pennsylvania
1957 Elected Member, National Academy of Sciences
1959 Harrison Howe Lecturer, University of Rochester

1960	Harvey Lecturer
1961-1965	Member, President's Science Advisory Committee
1963	Senior Fellow, Society of Fellows, Harvard University
1966	D.Sc., University of Chicago
1967	Gold Medal Award, City College Chemistry Alumni Association
1970	Fellow, American Philosophical Society
1971	Robertson Memorial Lecturer, National Academy of Sciences
1972	Dedication Lecture, Mitsubishi-Kasei Institute of Life Sciences, Tokyo
1973	25th Anniversary Lecture, Brandeis University
1973	J. T. Donald Lecture in Chemistry, McGill University
1975	Foreign Member, Serbian Academy of Sciences and Arts

ABSTRACT

Paul Doty begins by describing his family's background and his early education in Western Pennsylvania. He also recalls attending the ACS national meeting while he was still a teenager. He describes his impressions of Pennsylvania State College under Frank Whitmore, and the influence of John G. [Jack] Aston. Examining his selection of Columbia University for graduate studies, Doty describes the famous scientists there at that time and the effects of World War II; next he discusses how thesis research in physical chemistry led to work on light scattering and polymers. He remembers his coworkers, including Bruno Zimm and Turner Alfrey, and his postdoc in Eric Rideal's laboratory at Cambridge University, where he was first drawn to research in biopolymers. Doty recounts his early research at Harvard University, including protein denaturation and renaturation, and describes his colleagues. He continues the interview with an account of the development of biochemistry at Harvard and his involvement in public service and activism in nuclear and international issues. Finally, Paul Doty reflects on national characteristics in academic policy.

INTERVIEWER

Raymond C. Ferguson obtained his degrees in chemistry from Iowa State University (B.S., M.S.) and Harvard University (Ph.D.). He worked in research divisions of the Organic Chemicals, Elastomer Chemicals, and Central Research Departments of DuPont, principally in molecular spectroscopy, organic structure analysis, and polymer characterization. Currently he is affiliated with CONDUX, Inc., a consulting association of former DuPont professionals.

TABLE OF CONTENTS

INTERVIEWEE: Paul M. Doty

INTERVIEWER: Raymond C. Ferguson

LOCATION: Harvard University

DATE: 17 November 1986

FERGUSON: Paul. I want to first start with your father's occupation; what did he do?

DOTY: He was in the oil business, first in West Virginia and then in my father's home town in Western Pennsylvania. I was about seven when we moved from Charleston, West Virginia. Early in his career he had been the telegraph operator in our town, and then worked in the oil business. He managed an office which collated the production of a lot of small companies in Western Pennsylvania. We were relatively poor. The depression hit us rather hard, and we operated on a small budget.

FERGUSON: What was the town in Western Pennsylvania?

DOTY: It was called Chicora. It was about fifty miles north of Pittsburgh.

FERGUSON: What was your mother's background?

DOTY: Neither of them had higher education. She came from this region of Pennsylvania, Butler County, and had a Scottish-English background. My father's lineage was half English, half German. I grew up from seven in this town of about a thousand people until I went to college. I was the only child and had quite a bit of time to myself. There were no distractions like television and so forth. My father bought me a chemistry set after Christmas, when I was nine. It was marked down, because it had been frozen and was somewhat inoperative, but that certainly got me going. I had a little laboratory in an outhouse—washing house—at home and did a lot of grass cutting to buy chemicals for it. That was quite a major preoccupation.

When I came to high school, the chemistry teacher was also the basketball coach, and somewhat more interested in basketball. After a time he let me teach the class, and that was a great gift. I remember so well that in 1934, when I was 14, there was the dedication of the Mellon Institute in Pittsburgh, and in 1936 there was the meeting of the American Chemical

Society. I saved up my money and went to both. I was very inspired, particularly at the Mellon Institute where Harold Urey and others spoke, and I really ate that up.

The meeting at the American Chemical Society was more differentiated. Jack [John G.] Kirkwood got the American Chemical Society Award in Pure Chemistry there. I had a great awe of theoretical matters, with no understanding. I followed him about—he was a great role model—until I saw him go into the bar, and have a drink. That was the first year after prohibition, and I had been brought up very strictly, and I thought this was inconsistent. Then I heard some papers and so forth. Then came the presidential address at the end. I didn't have money for that, so I sat in the balcony while everyone ate their chicken. We heard the Presidential Address, which followed the dinner. It was by a man whose name was Professor [Edward] Bartow from Iowa. He spoke on "Fifty Years of Sewage Disposal," which rather took the edge off my keen anticipation.

I tell that only because it comes up later. When I was 35, I got the same prize [the ACS Award in Pure Chemistry]. I went to Dallas to the American Chemical Society [meeting] to receive it. I saw somewhat later in the day I was to speak at the dinner that evening, where they would award it. So I thought I would embellish this story of my first ACS meeting. The dinner progressed, and I got up to speak. I just got launched when I looked down and saw at the nearby table an old man whom I recognized as Professor Bartow, so I had to backtrack all together. I couldn't tell the story; the whole structure of my speech was gone. So I gave a very poor speech, but I was glad that I'd seen him, because I saw in *Chemical and Engineering News* that he'd died a month or two later. I felt that I hadn't been guilty of that—a joke about him in his last days.

FERGUSON: What books did you read in high school that prompted you or helped you teach chemistry?

DOTY: Well, I was quite a reader, although I'm dyslexic and a slow reader. I read a great deal, and I think the life of Pasteur, for example, by Vallery-Radot (1), was very influential. Also, the life of Madame Curie (2). There was also a four volume series on science and life by an Englishman—I think his name was Thompson—and somewhat later De Kruif's *Microbe Hunters* (3) and Sinclair's *Arrowsmith* (4).

FERGUSON: I think you were starting to say Eddington?

DOTY: Yes, Eddington had a book (5) in those days, you know.

FERGUSON: Did you have a laboratory manual or a teaching manual?

DOTY: I got an old one from the Grove City College, on qualitative analysis, and worked my way through that, I remember. There was a chemist, Dr. Lofton, at a small petroleum products company a few miles away; I did get to be with him some. What stands out mostly are my visits to Pittsburgh.

FERGUSON: How many were in your graduating class from high school?

DOTY: There were 22; it was the largest that had ever graduated. This was a wooden school house with a coal stove in each room. I did quite well in school. Languages were difficult because of my dyslexia, but I did well. We all did chemistry, physics, biology, and general science—all four courses, which is not often the case today.

FERGUSON: You mean in high school. Yes, that's unusual. Why Pennsylvania State College?

DOTY: Well, that was the advice I generally got. My parents would have liked to keep me closer to home, but they were rather agreeable to my ideas. That is what also made my upbringing beneficial, I think, because they would give me leave to do as I wanted if I had thought it through hard enough. So I went to Penn State, and that was a new world.

Frank Whitmore was Dean of Chemistry and Physics then, and his personality projected throughout the School of Chemistry and Physics in a big way, and that was influential. I remember that he gave a course one hour a week to all the freshman enrolled in chemistry or physics or chemical engineering. It was really good; it was dynamic and forward looking and had lots of moral philosophy. You know, "There's no such thing as a dishonest chemist." I ate that up; that was very good.

Then I had the good luck to become acquainted with John G. [Jack] Aston. I presume he's now dead, although I haven't seen it announced. He was a transplanted Englishman who had the Cyrogenics Laboratory at Penn State. He did heat capacities on various molecules, measuring the barriers to internal rotation and so forth. He took a liking to me and I took his courses even though they were in graduate school. That was very good; I did a lot of work both for pay and my senior thesis there. I think my first publication was when I was nineteen, on the rotation barriers of methylamine (6).

FERGUSON: I looked at that paper, and this was "Heat Capacity and Entropy of Methylamine from Spectroscopic Data Alone." Sounds like you were getting into Professor [Edgar] Bright Wilson's field.

DOTY: A little bit, yes.

FERGUSON: Was this Aston's interest?

DOTY: Yes, this was Aston.

FERGUSON: Was this the title of your senior thesis?

DOTY: Yes, that's right.

FERGUSON: That was an early paper. Did you take a straight chemistry curriculum?

DOTY: Yes, straight chemistry. There wasn't biochemistry in those days. It was mostly urine analysis, so I didn't come close to that. In fact I never had a course in biochemistry. I worked two or three summers at Penn State; that was great.

FERGUSON: How did you support yourself?

DOTY: Well, I did odd jobs; I waited on tables quite a bit. I lived at Alpha Chi Sigma, the chemistry fraternity, and that was good for me. I needed some socializing, and they were certainly supportive in chemistry as well, so that was positive. All in all it was quite good. I stuttered a great deal when I was young and the speech clinic there got me more or less straightened out on that.

I took a course in public speaking one summer school, I remember, so that was all to the good. Then I took a fair amount of physics. I took optics, I remember, from Dr. [David H.] Rank, and differential equations, and vector analysis and so forth. Rank was a physics professor I liked very much who was very good to me.

FERGUSON: Did you have minors?

DOTY: I don't think there were minors then, no.

FERGUSON: How about math?

DOTY: I remember the partial differential equations and vector analysis.

FERGUSON: Do you remember any particular texts that were used?

DOTY: Not very well.

FERGUSON: How large was your chemistry graduating class?

DOTY: I would guess it was a pretty good size, eighty or ninety. I was president of the student council in the School of Chemistry and Physics, but I don't recall it being a very demanding job.

So then I went off to Columbia. You know, I was somewhat precocious, so in the summer of 1940, before my senior year, I made the rounds. I visited Princeton and had a visit with Professor [Henry] Eyring, and came up here and visited Professor George Kistiakowsky, and went to Columbia and had a visit with Professor [Harold] Urey. I don't know whether there is the unrestricted ease of access these days to comparable people, but Aston wrote ahead for me and of course that helped. I remember visiting Professor Percy Bridgeman, over here in Physics [at Harvard]. I remember he had nailed his charter on the wall, not allowing anyone from Nazi countries to visit his laboratory. I really came away with the feeling the war was really on after that trip. It didn't quite seem that way back in Pennsylvania.

FERGUSON: From Bridgeman?

DOTY: From Bridgeman; Kistiakowsky was already talking about going off to the Explosives Laboratory in Pittsburgh. I really didn't think he would be at Harvard by the time I arrived. I got offers from all of them. They were all attractive, but I went to Columbia, I guess on the theory that there were more people there and therefore there might be some left, whatever the war did.

That was also a good experience, because I had a most remarkable set of people in that first fall term of 1941 before the war, specifically the Manhattan Project, really took over. I had Urey, [Enrico] Fermi, and I sat in on [Isidor I.] Rabi's lectures, Professor Joe [Joseph] Mayer, and Victor K. LaMer, who taught thermodynamics. Professor Edward Teller taught theoretical physics. That was quite exciting.

FERGUSON: It sounds like you were taking more physics than chemistry.

DOTY: Yes, it does. I think chemical physics wasn't defined as a subject area, but that's what I was really doing. I was not good in organic chemistry. My only C grade in college was in organic analysis, where a combination of phenol and whatever it was turned into Bakelite every time I heated it. I think, partly also, my memory wasn't all that good in organic chemistry, so that sort of pushed me off to the physical side, too.

FERGUSON: What was Teller teaching, spectroscopy?

DOTY: No, theoretical physics. He was quite a good teacher. I can remember the first day. He started off with, "What is a vector?" I was in the front row, and I held up my hand and gave the usual definition of scalar length and direction. He said, "How do you tell that from a chimney?" I just felt put down. No, he was a good teacher. Now, Fermi was terrific, too.

FERGUSON: Was Fermi's English good?

DOTY: Yes, his English was pretty good. He taught thermodynamics also. I guess I only sat in on his lectures, but he was very good. He was a great man, yes.

FERGUSON: Then did Urey leave?

DOTY: They began to disburse. They set up laboratories there at Columbia, and I guess by the spring term I was working part time on the Manhattan Project, mostly on heavy water work. It didn't come to anything in particular but was good laboratory experience.

I think by the second year, most people had left. There wasn't much to do in the form of courses, so I did this thesis with Joe Mayer. He was a theoretician and did not pretend to help in the laboratory part, but [Bruno] Zimm and Bill McMillan and I were together.

FERGUSON: Was Zimm at Columbia?

DOTY: Yes, we were in the same class. In fact, I got to know him first off because LaMer seated everyone alphabetically. I came in a little bit late, so I got put at the end of the row beside Zimm. That's how that started. Then we eventually married sisters. I later became divorced, but he didn't, so we had a long period there.

FERGUSON: These were girls you met at Columbia?

DOTY: Yes. The thesis itself was on electron affinities and some bond energies that one could calculate from it. I enjoyed the apparatus, vacuum tube work and so forth.

FERGUSON: These were the series of papers (7-9) on the hot filament experiments?

DOTY: Yes, that's right, nothing revolutionary but good practice, and also very modest requirements. I think I just bound the three papers together for my thesis of fourteen pages, and that was it. Somehow I got to know Herman Mark in about 1943 or 1944. I suppose Peter Debye came along to lecture.

FERGUSON: Were you still working on the Manhattan Project?

DOTY: I think that was fading, because it was moving all to Chicago and elsewhere. I think that probably phased out.

I can't remember how I first made contact with Herman; it must have been in a lecture of Debye's over at Brooklyn Poly. So by late 1943, light scattering came along. Zimm and I got interested in that and he built a little apparatus on the side at Columbia. I got my degree in 1944, didn't I, so I think I moved to Brooklyn [Polytechnic Institute] a few months before graduation in June 1944.

FERGUSON: At this point were you involved with the rubber project?

DOTY: That came later. I did get to know Mark, and started going over there on Saturdays. They used to have very good seminars.

FERGUSON: I have heard various versions from various persons. Burt [J. Burton] Nichols has told me quite a lot about the early light scattering work. This originated with Debye, his idea of light scattering from solutions?

DOTY: Yes, I think that he deserves the major credit for recognizing there was a quantitative tool there.

FERGUSON: You built an apparatus at Columbia; was yours the first?

DOTY: I think ours was the first around there. A man who had a long career in polymers, [Fred W.] Billmeyer at Cornell, built an apparatus there, so the experimental work went along about in parallel.

FERGUSON: So your first papers with Zimm (10, 11) came out of the work you did at Columbia?

DOTY: Yes, I think they probably came out of the gadgets we built at Columbia. I must have moved lock, stock, and barrel in 1944 [over to Brooklyn]. I must have started working on the Quartermaster Project at that time.

FERGUSON: I think your papers indicated somewhat how you compared the molecular weights by light scattering with the molecular weights as measured otherwise.

DOTY: Yes, osmotic pressure and so forth, but of course they measure different averages; therefore, it was a little sloppy.

FERGUSON: Did you recognize that at the time?

DOTY: Oh yes, it was pretty loose, and of course throughout all of its history the biasing effect of dirt in your solution was pretty serious. You had to go to great lengths to get rid of that.

FERGUSON: Nichols claimed that he sent some molecular weight standards that he had characterized by ultracentrifuge for someone to work with. Did you get to look at those?

DOTY: That's right, I think that's part of [Richard] Stein's thesis (12). Stein is the head of polymer group out at Amherst. I think he's known as my first student. He was a very bright young fellow. It was his senior thesis, actually.

FERGUSON: Did he stay on for graduate work in Brooklyn?

DOTY: I think he did, yes. [Stein did his graduate studies at Princeton: ed.]

FERGUSON: So you moved immediately into the faculty?

DOTY: Yes, I was an instructor. In fact, I taught nine hours a week, which is something I haven't done since. I had three courses that met three times a week, in the evenings. It stretched me.

FERGUSON: Did Zimm go to Brooklyn Poly with you?

DOTY: I'm a little bit vague on that. He was certainly there for a while, yes.

FERGUSON: He got the Zimm plot named after him, but I think you together made an important contribution in the analysis of effects of polydispersity (11, 13).

DOTY: Yes, we pulled that out and that was useful.

FERGUSON: Did you sense how important this was, or did it seem important to you at the time?

DOTY: Oh yes, it did seem fresh and new, and we weren't perhaps very self critical in those days. It all came wonderfully fast. Turner Alfrey was there. He was a great supporter, and I did have a paper or two with him, if I recall (14, 15).

FERGUSON: As a matter of fact, those were impressive papers, one specifying the properties of viscoelastic materials.

DOTY: Oh, yes, he was very good. I learned a lot doing that paper.

FERGUSON: Was he a graduate student working for you?

DOTY: I think he was a year or two older; he must have also been an instructor or an assistant professor.

FERGUSON: You had two co-authored papers. One was the viscoelasticity paper, and "Statistical Thermodynamics of High Polymer Solutions," was the earlier paper.

DOTY: We meant to follow that up in a series and never did. Yes, that's right.

FERGUSON: How about your involvement in polymer-liquid interaction by swelling measurements (16)? Was that connected to your general wartime research?

DOTY: No. I think that was on the side. I think probably a little bit of it came from the film work, but mostly we were measuring diffusion constants and transmission rates in films (17). It was on polyethylene, socks and so forth for people in the trenches and for rain protection.

FERGUSON: You were associated with Quartermaster Corps Project?

DOTY: Yes, that was on polymer films.

FERGUSON: Were you particularly associated with the rubber project?

DOTY: No. That was part of organic chemistry. So many of these things were on the side.

FERGUSON: You had some official title in the Quartermaster Project, didn't you?

DOTY: I might have been Co-Director with the army fellow.

FERGUSON: I saw that somewhere. You didn't stay long at Brooklyn, did you?

DOTY: No. I guess I wanted to see more of the world, for one thing, having not been in the war itself.

FERGUSON: How did you become a Rockefeller Fellow at Cambridge?

DOTY: Well, I suppose Isidor Fankuchen, who was in Mark's group and had been in Cambridge and London a lot, excited me by his tales. He had a rather good repertoire about the prewar X-ray business. Somehow, through him or Mark, I got introduced to Dr. [Gerard R.] Pomerat, who was running the Rockefeller Fellowships. I had a good interview with him and got the fellowship award for the spring of 1946. I decided I'd take my chances, resign from Poly hoping to find a job when I came back. Two or three weeks before I was to leave, I found in the fine print that the fellowship was contingent upon having an academic job to come back to. The first person I fell upon after that was Charlie [Charles C.] Price, who was going to Notre Dame, so he offered me a job there and I took it.

FERGUSON: Was he going to head the Chemistry Department at Notre Dame?

DOTY: That's right, yes. That was fortunate, and I went on [to Cambridge].

[END OF TAPE, SIDE 1]

FERGUSON: Now you can continue with your experiences at Cambridge.

DOTY: Before we go to Cambridge, I feel we should go back to Brooklyn and say that the liveliness of that scene which came from Mark and Fankuchen was pretty remarkable. Getting to know people like Debye, Paul Flory and Bill [William O.] Baker accelerated my career and my whole sense of professionalism enormously, and that's a very important part of it.

FERGUSON: Bill Baker was there?

DOTY: He came to the Saturday seminars. It was really a great gathering.

FERGUSON: Burt Nichols mentioned those Saturday seminars. He used to come up from Wilmington and thought that they were outstanding. They must have been to be worth a trip from Wilmington.

DOTY: In those days, yes. I'm sure there were a lot of others, but Debye and Flory and Bill Baker stand out, and the DuPont people.

FERGUSON: New York and Brooklyn are very strategically located for people coming in and out. I suppose you had a lot of people dropping in at Columbia just on their way through.

DOTY: Yes.

FERGUSON: Going to England in 1946 wouldn't have been my ideal choice.

DOTY: No, it was not very sybaritic then. It was pretty hard; in fact, 1946 was the worst winter. Coal ran out; the rivers overflowed and electricity was only on a few hours a day. I still remember the headlines in the paper in Cambridge, when they raised the amount of meat in sausage from five percent to ten percent. It was to get better later on.

FERGUSON: I suppose there was nothing comparable about World War shortages in Brooklyn.

DOTY: No, no.

FERGUSON: You were comfortable and had no problem?

DOTY: We had ration cards and there was not much meat, but it was pretty good compared to England.

FERGUSON: What did you work on?

DOTY: Well, England, Cambridge too, was just coming back together after the war. Mark's friend there was Eric Rideal. He came back as Professor of Colloid Science. That wasn't quite my cup of tea, but I didn't have any other contacts, and there wasn't so much going on either. He had a younger collaborator, [Jack H.] Schulman, so I worked some with him on surface balances and monolayers and so on (18). It wasn't very interesting work. There were other people that I got to know, like Gordon Sutherland, who was in infrared work.

FERGUSON: Oh, yes.

DOTY: At that time, [Robert B.] Woodward, here [at Harvard] was making the synthetic polypeptides and I carried them back and forth. That was quite intriguing, although we didn't have any publications.

FERGUSON: You mean you made trips back and forth?

DOTY: Well, yes. I think, somehow, I brought them together. I think I took samples over when I went, and Sutherland got the first spectra of polypeptide bonds in beta form. I named my son after Sutherland. Kenneth Bailey, a protein chemist, was there. He later died. I had long talks with him. I guess, most of all, I think of Max Perutz. He was already established working on hemoglobin. I think he was probably the most influential person.

FERGUSON: Was this where your interest in biopolymers developed?

DOTY: Yes. That seemed to fall together—that having worked with synthetic polymers, I should try to see to what extent the same principles applied to more complex molecules.

FERGUSON: Did it seem to you right away that light scattering would be applicable?

DOTY: Well, I thought I would give it a try. I was going back and forth between x-ray work, which seemed promising but which I didn't have much background in, and solution work, which I did have background in. Out of that came the attempt to use synthetic analogs of proteins and nucleic acids, and to go after them the way one had studied high polymers.

FERGUSON: You did a paper on light scattering studies of tobacco mosaic virus with Zimm (19). Was that after you got back or was that earlier?

DOTY: I think it was earlier.

FERGUSON: The paper was dated 1947.

DOTY: Yes, I think it was. It might have been; I just don't remember. Oster was in New York, and I'd been out at Notre Dame. I think he carried it on, and we wrote it up when I got back.

FERGUSON: I have a copy of the paper here.

DOTY: I think that that was a nice test of things, because that virus is long enough to have first order diffraction effects with visible light, so we could get its dimensions quite nicely.

FERGUSON: Oster was at Rockefeller Institute?

DOTY: I think he was then, yes.

FERGUSON: I wonder if that was the first paper on light scattering from biopolymers.

DOTY: Yes, it might well be. Again, running over the scene in Cambridge, there was also Bill [William D.] Astbury who was at Leeds. He came to Cambridge often. Before the war he had the best x-ray fiber diagrams of both protein and nucleic acids. Although very poor by present

standards, you could begin to see that there was very interesting order there. He was a very lively fellow and wrote a lot of letters back and forth when he didn't come to visit.

FERGUSON: Did your wife go over with you?

DOTY: Yes, she did some of the work with Schulman.

FERGUSON: She was a chemist?

DOTY: She'd not been trained as a chemist. She was a laboratory technician, learned as she went.

Because there were so few American scientists around, I got invited to a number of places. I spent a week or so in Sweden with [Thè] Svedberg and some time with [Charles] Sadron in Strasbourg.

FERGUSON: Please continue about the European contacts.

DOTY: I remember best the trip to Sweden and trips to France and Holland.

FERGUSON: Svedberg had been doing ultracentrifuge work on biopolymers.

DOTY: Yes, that's right. That was a very interesting contact. He was old but still very lively. Sadron was a physicist who had turned to macromolecules. He had difficult war-time experiences, but he was setting up shop in Strasbourg. I was there several times. Well, one of his earliest collaborators, Henri Benoit, then came over and spent a couple of years with me.

FERGUSON: At Harvard?

DOTY: Yes, at Harvard, after I got settled. That was a good collaboration. I got to know [John D.] Bernal, and the Joliot-Curies, and some of the polymer people in Holland—J. J. Hermans.

FERGUSON: Synthetic polymers, as well as biological?

DOTY: Yes. In England in 1947, I went down and spent a couple of months in London at the Royal Institution, where Rideal had moved. I built a light scattering apparatus there. I started to work on the disassociation of insulin as studied by light scattering. I came back to the U.S. in the summer of 1948. I talked about it when I came through here [Harvard]. I no sooner got settled at Notre Dame than I got the offer to come here. It was difficult because I didn't like to leave Notre Dame so quickly yet didn't feel these kinds of opportunities came along very often. So I tread water for a year and came here.

FERGUSON: Were you able to do much at Notre Dame?

DOTY: No, there wasn't any point in building anything.

FERGUSON: Were light scattering apparatus at that time all home built?

DOTY: Yes.

FERGUSON: There were no commercial units?

DOTY: I think they became commercial about 1950.

FERGUSON: Did you design yours?

DOTY: Yes, it was all sort of self design. I think Phoenix Instrument Company built the first commercial one.

FERGUSON: Was Father Julius Nieuwland still alive at Notre Dame when you were there?

DOTY: I think not; I can't remember him. It was a good place and has continued to be since. There was this laboratory for germ-free life which was going on there. I didn't understand much

about it, but it was very impressive—some of the techniques and so forth. So there was a little biology there too.

FERGUSON: What did you teach?

DOTY: I taught physical chemistry and polymer chemistry.

FERGUSON: Well, when you finally got to Harvard, what did you embark on?

DOTY: I got here in 1948, and I had three graduate students straight off. By that time we were able to prepare nucleic acid fairly well. We started from thymus glands, which we got at the local slaughter house in those days. We started out making as pure samples as we could of the nucleic acid. It was in the native double stranded form, although we didn't know that then. Also all of the samples then were degraded and therefore had a molecular weight distribution, not unlike [synthetic] polymers themselves. We didn't yet have the samples with identical molecular weights from phages and so forth, and viruses. Barbara Bunce did molecular weight measurements (20). That was a good start just to establish the molecular weight range of these somewhat degraded samples. That was a great step forward.

Then there was a very bright fellow, Stuart Rice, who is now head of the Materials Science Department at the University of Chicago. He is a good fellow. He was very inventive and got interested in the denaturation of nucleic acid DNA. He did a lot of the very first work on that in his thesis. The fact is that if molecules are stiff and very long, solutions are quite viscous; when you go above the critical temperature, the DNA melts out, concentrating into flexible single polymeric chains: it becomes very non-viscous (21). So, using viscosity measurements, the transition from the native DNA to the denatured form was very easy to follow.

FERGUSON: I see that during this period up to the 1950s, you were doing papers on synthetic polymers, though.

DOTY: Yes, but this was old work that I was bringing up to date. I didn't have anyone here doing polymer research. The third fellow worked on proteins, but that didn't give us very much. But that's how we got started. For about ten years—they were probably the most exciting ten years—it was a matter of going after synthetic polypeptides as models of proteins. We were seeing if we could get those in helical form in solutions—the Pauling alpha helix. We succeeded in doing that. Once you have an ordered single helix, again it's solution is very

viscous, but then if you raise the temperature beyond which it cannot sustain itself, it melts out very quickly and becomes much less viscous. You can do all that with polypeptides.

FERGUSON: Were the transitions reversible?

DOTY: Reversible, yes: very much like one dimensional melting. Then with DNA we could do the same thing with the two strands. We worked very hard to try to show that the molecular weight fell in half when the DNA melted. That would be consistent with the Watson and Crick model of the two-stranded helix. That was difficult to establish because the extrapolations of the Zimm plot were a little imprecise, but I think we did establish that by 1954 or so.

The woman who became my wife, Helga Boedtker, was a graduate student then. She did light scattering studies on gels, using gelatin and degraded collagen. That was our attempt at gelatin and collagen, and it was an interesting paper (22), but we wanted to get on to collagen itself, feeling that it was a three-stranded helix. So by 1956, when I gave this talk at the National Academy of Sciences (23), we had the parallelism between the helix coil transition for the single-stranded helix and the polypeptide, the double-stranded helix of DNA and triple-stranded helix of collagen. That was a nice unifying point of view. It showed that phase-like transitions could occur, in single-, double- and triple-stranded biological molecules. That was the first stage of the work at Harvard, although other things had gone on.

FERGUSON: Was the Harvard Chemistry faculty being far-sighted in hiring you? Were they planning in terms of biochemistry?

DOTY: Oh, I think they probably were. They seemed quite interested in the little lecture I gave on insulin dissociation. There was no biochemistry in those days, except what Edwin J. Cohn was doing over at the Medical School on protein fractionation. There was no one who knew anything about tobacco mosaic virus, or any kind of virus; there was really an open field.

FERGUSON: I think what you're saying is that at this point the really fundamental physical studies of biopolymers in solution were just taking off.

DOTY: Yes, I think they really got established by these configurational studies. Then people never thought about high polymers having this endowment to take on these helical forms, but then along came the stereospecific polymerization work of Natta and others, and there you were—although they didn't, of course, exist in solution but only in crystalline form. That was sort of lucky for me too, because I was consulting for Union Carbide and going back to

Charleston, West Virginia, where I'd been born. So these came along at that time. I was in a unique position because I knew about helical macromolecules.

FERGUSON: Let's see, you got involved as an expert witness in the polypropylene patent interference. Did you actually work with polypropylene?

DOTY: No, I only talked about it and read the papers and so forth. I also had gotten infrared work with Elkan Blout on the polypeptides we had studied. I had a very good understanding of the infrared side, and the x-ray side was very easy. I was on that patent case for nineteen years; I guess that was the longest interference that came down the pike.

FERGUSON: That's going to continue on appeal. There is so much money there it will be fought on forever. That's an interesting story, because of the people involved in the politics.

DOTY: I knew only a small fraction of it.

FERGUSON: Did you ever get any patents or get involved in anything on your own?

DOTY: No.

FERGUSON: You just mentioned consulting, so could we digress a little bit about whom you consulted for and what some of your experiences have been.

DOTY: I did Union Carbide maybe six or seven years. Then with the interference case where I went to Chicago a fair amount for Standard Oil of Ohio.

FERGUSON: Were you a regular consultant for Standard?

DOTY: I only went there for the interference work. I went to Gillette a few times, but nothing much came of that.

FERGUSON: Was this consulting on polymer work?

DOTY: Yes. Then by 1955 or 1956, the enzyme which can make synthetic polyribonucleic acids was discovered. [Severo] Ochoa got the Nobel prize for that in 1959. One of the people that worked for him, [Jacques R.] Fresco, came up here, so we started a project of making synthetic polynucleotides. Having gone through the phase of polypeptides, which I mentioned, we did a lot of optical rotation studies then that were very good. In fact, two papers from that period—one with Urnes (24) and one with Holzwarth (25)—have been recorded by that Scientific Information Institute in Philadelphia as among the 250 most quoted papers at that time. The paper by Urnes was a review in *Reviews of Protein Chemistry*.

FERGUSON: I was looking at one of the articles on asymmetry in optical rotary dispersion work; is that part of polarized optical rotary dispersion?

DOTY: Yes, part of the same thing. We put in a good slug on that and that was good.

FERGUSON: Did you introduce the use of the optical rotary dispersion method?

DOTY: I think in terms of its application to proteins, yes.

FERGUSON: Was that with commercial equipment?

DOTY: It was commercial, but it was pretty modest. The students at my 65th birthday recalled how they went blind.

FERGUSON: It required direct observation?

DOTY: Yes, it required direct observation in matching hemispheres.

FERGUSON: Wasn't recording optical rotary dispersion equipment developed?

DOTY: I think it came along not much later, yes. It was much better. Anyway, we did polarized optical rotation and were able to get, for example, optical rotary dispersion and the

infrared spectra of the three configurations of the polypeptide chain: the random coil, the beta form and the alpha helix. From that, one could get a rough estimate of the extent of these three configurations in proteins. That was quite helpful, particularly in selecting proteins for x-ray determination and so on. Elkan Blout, who is now a professor at the medical school, was a great collaborator in this period.

Then we went on to the poly-nucleotides in the middle and late fifties and that went quite well too. You could get DNA-like double-stranded helices out of them, and even triple-stranded helices, so that was a useful period, too. During this time my wife had shifted to studying ribonucleic acids. We were demonstrating, for the first time, the kind of localized structure that can develop by base pairing in single-stranded ribonucleic acids and how one could use various denaturing agents and remove that localized structure. That put all the physical chemistry of ribonucleic acids in a very satisfactory form, and she was largely responsible for that.

FERGUSON: Did Helga continue working when the children came along?

DOTY: Yes, I think one time she took off six months, and the other time just a couple. She was a good worker; we had the benefit of buying a house just a hundred yards away, at 4 Kirkland Place, and I think that saved the situation, in many ways.

FERGUSON: Is the house gone now?

DOTY: Oh, no. It's an old Cambridge fortress. It'll be there forever, I think.

[END OF TAPE, SIDE 2]

FERGUSON: Let's continue with this work that your wife was doing.

DOTY: She continued on the ribonucleic acid side, and she was really a pioneer in that. No one knew quite what to make of those molecules in those days, partly because this local structure was not recognized. It could be overcome by denaturing conditions which eliminated it but still did not degrade the molecule.

In the late 1950s, we came up to our best work. It was the denaturation of DNA and its renaturation. Ever since Rice's work that I described earlier, we had the general idea that the strands would come apart under denaturing conditions in a rather phase-like melting out. If so,

they should go back together if they could find the right mesh, because it was not like the synthetic polymers, where they come back together anywhere. It would have to be just in register the way it was originally done. Julius Marmur came to the lab then, as a postdoctoral fellow. He had worked with [R. D.] Hotchkiss at the Rockefeller Institute and had a good background in bacteria. We concentrated mainly on bacterial DNA. He made DNA from a number of different bacteria.

We found rather early on some nice straightforward physical chemistry. There are two kinds of base pairing in the Watson-Crick model: guanidine-cystine, GC; and adenine-thiamine, AT. These DNAs from various bacteria covered a range of composition so that we had maybe one-third of the percentage scale—from thirty to sixty percent of, for example, GC in different DNAs. So we had DNA molecules from different bacteria with different relative amounts of these two base pairs.

We quickly found that the melting temperature in standard saline solution was quite linearly dependent upon the compositions: the more GC, the higher the melting temperature. That was a nice, clean-cut breakthrough with lots of fallout over the next ten or twenty years. It gave the compositional consequence in terms of the physical property. Then, we looked at the buoyant density in the ultracentrifuge and found a similar relation there. We could put in DNA from a half a dozen different bacteria and band them in the ultracentrifuge. They would come out in six bands located in the gradient at points corresponding to their different densities. That was a nice physico-chemical side of it.

On the biological side, we just kept searching for a means of restoring the double helical form. We had the idea that it would be just like crystallization of organic molecules from the supernatant—that if you lower the temperature below the melting point twenty-five degrees or so and give it time, it will nucleate and ultimately form crystals. We had a bit of serendipity relating to another physical matter which we exploited a great deal. When the DNA denatures and comes apart, the stacking of the bases disappears and becomes randomized, and the intensity of the ultraviolet peak at 2600Å falls about thirty-five percent. Therefore, by varying the temperature, you could follow with very small samples, in the Beckman spectrophotometer, the course of the denaturation and the renaturation, if it occurs.

We went through one of these melting curves one morning and went out to lunch. When we came back, the housing of the spectrophotometer had cooled. We expected, nevertheless, that the sample would give nearly the same high absorbance as we had found with calf thymus, because it was denatured; but indeed the absorbance had come back down. So we recognized that we had had a bit of luck, and renaturation was indeed taking place. This was something that we would have not discovered with more complex DNAs, because they have a much longer message in them. The probability at these concentrations of those formerly joined sequences finding each other again in solution is very low. It might take a month or so for those to come back. So, it was lucky that we were having bacterial DNAs which were rather simple, plus this technique, plus looking for it. So we did find that renaturation occurred (26). This was

probably the best discovery that we ever made. I don't think I would have been primed to recognize it or begin searching for it without the background of rubber elasticity in polymers. There, that which you really believe in your heart—that Brownian motion is going on all the time, so you have a mechanism for [chains] feeling out each other and finding points of registration.

I remember that soon after discovering this idea, I gave a seminar at Caltech. Max Delbrük was there. He was a great biophysicist, but no chemist. He just claimed that what I was showing them was absolutely impossible; molecules would never find the points of registration where they could start generating orderly growth again. We bet twenty dollars, which was quite a bit in those days. [Robert L.] Sinsheimer held the bet. Delbrük didn't give in for another few years, but he finally did and Sinsheimer informed me that he had sent the forty dollars to Lyndon Johnson's campaign. So, it was not an obvious idea for a person who did not understand rubber elasticity or micro-Brownian movement of macromolecules. That was a nice synthesis of the past.

Then we were able to show that you could grow some bacteria in heavy nitrogen or deuterium and get the molecules labeled with a different density (26). We could bring one unlabeled molecule from one source and a heavy labeled molecule from another, put them together, denature them, renature them, and see how much of a common hybrid was formed with an intermediate density. That would tell you how much of the code from one was also common with the code for the other. We went through a lot of bacterial species and now this has become a standard method in the taxonomy of determining relatedness among species.

FERGUSON: Now, were you doing all of this without doing any sequencing?

DOTY: That's right. But if the sequence was common to both, then they would renature with a fairly standard rate which we could predict from seeing what the two things did independently. So we did a great deal of hybridization work then.

FERGUSON: The bacteriology part of this was done here?

DOTY: Yes. Of course, viruses have an even simpler code in them, so they renature extremely rapidly. The synthetic polynucleotides which we had by then renatured so fast we could hardly get it. You have a whole spectrum of synthetic polynucleotides which have poly-U on one side and poly-A on the other. They just go together; there's no registration problem. Then viruses, then bacteria, and on up into new life forms and up to humans. It takes about a month for the human DNA to renature, and it does it only in part. This, of course, has gone into big business now. It's sort of the first step in recombinant DNA work that you can get renaturation and move things about this way. So that was good. Then during the rest of the 1960s we exploited this—

not as much as we should have, looking back on it, but it disseminated like all other good ideas. Many other people contributed.

It was only in the very early 1960s that the idea was emerging that the code was read off the DNA onto the RNA (ribonucleic acid), and that message went into the cell and produced the protein in a two-step operation. We were able to show that RNA freshly made in the cell could be hybridized with the DNA and therefore was indeed carrying the encoded message from the DNA to the RNA for the blueprinting of the protein synthesis. We did some other interesting work in the late 1960s and early 1970s on very short synthetic polynucleotides with a designed sequence, so that we could see how much one mismatch would affect the melting point. There emerged the whole picture of how much defects cost in terms of stability of the two-stranded helix (27).

FERGUSON: You have your own people here in the department who make these things for you?

DOTY: We did all of that ourselves in those days. Of course, they weren't nearly as complex as you would make them now. Now they are done by technicians.

FERGUSON: You took an incoming graduate student and put him in the lab and told him to make this?

DOTY: Yes. That was the way it worked, with a little coaching. One of the fellows who did best in this was [Olke C.] Ulhenbeck, the son of George E. Ulhenbeck, the electron spin man. He's just now moved from the University of Illinois to Colorado. He was very good. He still is. All in all, I think there are about five of these people who are now members of the National Academy of Sciences. I'm sure there are some more to come.

FERGUSON: It doesn't seem consistent with your lack of enthusiasm for organic chemistry.

DOTY: Well, once it didn't involve too many complex reactions [laughter] it was helpful. I suppose what propelled me into biological molecules from polymers was the feeling that one wanted to stay ahead of the game: that one wanted to apply what one knew to the more complex cases, hoping that it would work. In the same sense, once one got the biological polymers understood and organized into a framework, the next goal was to go on to the synthesis of these molecules to see what one could do there with physical chemical attacks. So, there were two successive challenges. One was to get on with the protein synthesis and see how the RNA

actually governed that synthesis and how the code was read out. The other was to see if you could go beyond that to the next step and look at gene structure in higher organisms. Those were the two last phases of our work. On the protein synthesis side, we got in a little too late to be in a terribly important role.

Bob [Robert E.] Thach was a graduate student who then became an assistant professor here. He did the best work. He's now at Washington University. We did actually identify a couple of the elements of the genetic code for a couple of the amino acids here, but we were six months or a year behind in that. Then by about 1970, we had to decide whether to try to go on. I think the protein synthesis was becoming pretty well understood. The physical chemistry of biopolymers was pretty well understood. These were very qualitative judgments; there's gold in those hills yet. The question was what to do. We decided to go back to the structure of the genes and decided to pick on the collagen gene—a little bit sentimental because that was what Helga had worked on for her first post-doctoral job. I guess I didn't mention that, but after her thesis, which was in 1954, her first postdoctorate job was on the structure of collagen in solution. She did a great piece of work on that, getting the dimensions very nicely and following the denaturation of this triple-stranded molecule.

FERGUSON: She stayed at Harvard?

DOTY: Yes. So that was under her belt. We had a certain attachment to collagen, I guess, by that time. Collagen is a molecule that is sort of at the other end of the protein spectrum from simple enzymes, which have just one active spot and do one kind of catalysis. Collagen, again, going back to the polymer period, is the major structural protein of the body. About thirty percent of the protein of the body is collagen—nails, skin, blood vessels, and so on. It's a versatile building material. Also, if you go to the ultimate stage of molecular biology, the molecular basis of growth and differentiation from the embryo on, collagen is the matrix in which all of that takes place. Although it won't be in our lifetime, we felt that the ultimate molecular biology of embryonic growth and growth all the way to maturation is largely a problem of the reallocation of collagen molecules. One had better get the basic things straight in how it is read out, i.e., the control of synthesis and liberation of collagen at just the right time. The fact that we have similar facial features from parent to offspring reflects an extremely finely controlled evolution of collagen to give the face the form that it has.

This was the inspiration of our last phase which has gone on for a dozen years or so. We set to work; more and more of the work came jointly with Helga and then more her alone as I phased out of sorting out the collagen gene. There are now about ten collagen genes, but one of them can make about ninety percent of the collagen that's in the body. So we tended to concentrate on that one first. It turned out that we knew it would be a pretty long gene, because the protein is 100,000 molecular weight. That was the minimum size. As we got into it, the gene was larger and larger. Ultimately, when we got it all figured out, it was about twelve times

as long as the encoded message. The encoded message is broken up into 53 pieces and separated by large regions which do not carry any information. It was certainly, in its day, and is still almost the most complex gene whose structure has been determined. We have now determined all of the encoding parts and more than half of the non-encoding parts.

FERGUSON: When you say determined—

DOTY: By sequencing.

FERGUSON: So, the total sequence has been determined?

DOTY: Yes. It's thirty-nine thousand bases long. About twenty-five thousand of that is known and that includes all of the regions that contain the coded message.

FERGUSON: How did you get this information?

DOTY: This became possible due to the revolution in recombinant DNA. We could cut the gene at known places, analyze small parts of it, and put it back on the map.

FERGUSON: But you're not having to approach anything like specific sequencing?

DOTY: No. Of course, the way of getting this gene out of the enormous library that's in the human DNA—we actually work with chicken—is to go to a point in the embryonic cycle where collagen is the major protein being made, and extract the RNA from there. That RNA is largely encoding collagen. We use that to hybridize with the nucleic acid of the whole organism. That hybridization picks out the collagen gene. Then recombinant DNA technology takes over. So that's been done, and we, of course, are working on other more minor collagens. In this phase, we are trying to study how the cell shifts from making one kind of collagen to another. In tissue culture, we are trying to understand the means of molecular control which prompt it to go one way or the other. For example, one kind of arthritis involves this shift from one kind of collagen to another. The development of the embryo involves going from one to another. This is a fairly basic step and we're just trying to untangle that now. It's a little way from polymer chemistry, but if you would look carefully you can see some pathway.

FERGUSON: How many disciplines are there, or is it all chemistry, basically?

DOTY: Well, it's certainly all macromolecules, and I suppose that the earliest forms of life must have been much less specific than anything we know now and perhaps, therefore, much more polymer-like with just little islands of order emerging within a much larger, high molecular matrix, probably the RNA. In fact, we just had a very interesting lecture here last week by Tom Cech, from the University of Colorado, who has found an RNA can actually act as a catalyst, like a protein does. It had always been thought that only proteins would catalyze biological reactions, but RNA can catalyze itself by cutting out pieces of itself and reuniting the strands and so on. So, it was probably RNA that was the first self-producing molecule capable of some self surgery. If I were young again, I would pull at my boots and continue what we are doing. In a century of so, collagen will be seen as the primary building material of all higher forms.

FERGUSON: Let's talk about your teaching career a bit. When I was here, you were teaching physical chemistry and polymer chemistry.

DOTY: Yes, I even did inorganic chemistry a couple of times.

FERGUSON: When was the biochemistry department formed as a separate entity?

DOTY: That was 1967 and I guess I could call myself the founder of that. I was the first chairman. I wrote up the charter for the department and got the faculty to approve it.

FERGUSON: Did [Frank E.] Westheimer come here then or before?

DOTY: Westheimer and [Konrad E.] Bloch came in 1954. [James D.] Watson came about 1955, so that was the nucleus. Before they came there was really nothing except what was going on in Edwin Cohn's shop, and he died in 1954. [John T.] Edsall moved over to Biolabs [Biological Laboratories, Harvard University] then. He and I did some things together, as you can see (28). To get a critical size, it took Bloch and Westheimer and Watson. Then we got [Matthew S.] Meselson.

FERGUSON: Do you offer degrees separate from the chemistry department?

DOTY: Yes. We are all separate now since 1967. Between 1957 and 1967, we had a committee on higher degrees in biochemistry, which is what one does in hybrid fields here. Then in 1967 we started our department. I was also the chairman of the building committee. We were looking hard for money to build a building here, but we failed. It wasn't until about ten years later, when Meselson was chairman, that we got the University to give us a lien on some Mallinckrodt money that was coming due in 1984, so we started planning and building and ultimately got enough gifts to pay it all back.

FERGUSON: You gave a beautiful summary, but I would like you to reiterate what you think your most important scientific contributions are.

DOTY: I suppose physical properties of macromolecules in solution was the rubric of the earlier phase. That included the light scattering and a number of other things, but it wasn't focused on a single event and it did not have the coherency that, for example, Flory has in his work. I think I admire his work most of all.

FERGUSON: Flory had a little interest in collagen.

DOTY: Yes, he did.

FERGUSON: One of his students, my colleague Bob [Robert R.] Garrett, couldn't talk about anything except for rat tail tendon for several years.

DOTY: Flory had the interesting contribution, which we had missed, that when collagen is denatured into separate stands and then renatures, you would think it would be a third-order reaction, but it's only a first-order reaction. The reason is that the limiting step is the assumption by a part of the individual chain of configuration it takes in the helix and not the frequency with which they come together (29). That was a nice neat point. Paul was a great contributor.

FERGUSON: He tackled this theoretically?

DOTY: No. He did experiments on it too. He didn't continue it, but for a single foray it was a nice result. Then I think, secondly, it was my getting the basic helical configurations in solution for all three forms: single-, double-, and triple-stranded molecules. Then, thirdly, I think the renaturation has had the greatest impact and usefulness.

FERGUSON: I guess the optical rotary dispersion work had a strong theoretical aspect.

DOTY: Yes, it did. That was helped about by a young Englishman [William] Moffitt, who came here in the mid-fifties and quickly came to be an associate professor. He died of a heart attack on a squash court in 1959. He was my closest friend at the time.

FERGUSON: His field was quantum mechanics. This work on molecular optics theory suited him well?

DOTY: That's right. Yes. He was very good at that.

FERGUSON: He was a brilliant man. I admired him very much.

[END OF TAPE, SIDE 3]

FERGUSON: I remember when I was here, you put in terrible hours. You were here night and day.

DOTY: Yes, that's right.

FERGUSON: Do you still?

DOTY: Well, I still work most of the time. That's true, but it's more sedentary now. I do a lot of traveling to conferences in my other life. Before we get to that, I guess I should go back and say that Mark and I founded the *Journal of Polymer Science* back in 1947 and that seems to continue. In 1957, Kendrew and I founded the *Journal of Microbiology* and that continues strongly. In 1977, I founded *International Security*, which has become the leading quarterly in its field. I've just stepped down last year from being chairman of the editorial board. So I have these three journals to my credit, more or less, but always with the help of other people. They are flourishing, but I never got around to writing a book. We have about three hundred fifty papers from the laboratory here, about two-thirds of them with my name.

FERGUSON: Do you feel there's a book there, or there's a book needed?

DOTY: I think not. The proper niches are being filled.

FERGUSON: Would you settle for the collected papers [of Paul Doty]?

DOTY: Well, no. I think they are a little too scattered. It takes a profession like this to pull them together. I think they'll serve their purposes as time marches on.

FERGUSON: Let's talk a little about your public service. You were elected to the National Research Council.

DOTY: Again, I think this is probably Mark's influence. I remember he got me to be secretary of the section of Macromolecules in the International Union of Pure and Applied Chemistry. That was my first toe in the water. I remember we had organized a number of international meetings in the 1950s.

FERGUSON: Does this involve mainly organizing meetings?

DOTY: Yes, and making sure there is funding and getting the right people there to make them interesting.

FERGUSON: Do they establish research policy?

DOTY: No. This is just reporting and internationalization, which was very important in the 1950s, after the war, to get science going again in Europe. I remember a meeting in 1957 in Prague where I spoke on biological polymers (30). I think there and again in Moscow in 1960, I had never enjoyed public lecturing as much. So many people knew nothing about this work. I think I filled the opera house in Prague.

FERGUSON: That must be exhilarating.

DOTY: Yes, it does give you an upper. So that was very good. In fact, my lecture there in 1957, which is amongst my reprints, summarizes this first phase. Then the 1960 Harvey Lecture did the same thing for the renaturation part (31). Those were great lectures. I enjoyed them very much.

Then the external part of my life began. Well, I was always infected with the nuclear problem from my early days in the Manhattan Project and all the galaxy that I met at Columbia. In 1957, when I was working hard in the lab, I was elected chairman for a year of the Federation of American Scientists, which is a group of scientists interested in nuclear matters. It was in a pretty low state then.

FERGUSON: Was Kistiakowsky perhaps influential?

DOTY: No. His interest in the area was important, but in those days he was still very much on the conservative side of things. I don't think he took this very kindly, although he certainly would later.

FERGUSON: I gather you were not a great admirer of Edward Teller?

DOTY: No. Our differences grew, but we get along in a friendly fashion.

FERGUSON: You were a member of the President's Science Advisory Committee in 1961 through 1965. What was involved with that?

DOTY: Yes, but to explain my going into that line separately, it goes back to being chairman of the Federation of American Scientists in 1957. Because I was chairman, I was asked to come to the first Pugwash Conference up in Pugwash, Nova Scotia, to meet with Russian scientists. As a result of that, I was invited to Moscow in 1958. Then, being one of the few scientists that had gotten to know some Russian scientists, when [Dwight D.] Eisenhower was preparing for his summit meeting with Khruschev in 1960, he wanted a committee to advise him on disarmament. [James] Jim Killian put me on that committee. I got clearances and have been in the game ever since. So that's what led to all of this, starting with this original meeting at Pugwash. Once I got that far, [John F.] Kennedy asked me to be on the Science Advisory Committee, and I've been advising one thing or another down there ever since.

FERGUSON: I see you also had two stints on the National Academy of Sciences Committee for Soviet and American Scientists.

DOTY: Yes. I got that going and oversaw the first years of that exchange program.

FERGUSON: What is the Council on Foreign Relations?

DOTY: That's a professional group out of New York City on foreign affairs. In fact, I'm chairing a meeting down there on Wednesday. But most of all, I've kept contact with the Russians; not that I don't fault them in many ways, but I really think we have to find ways of living with our differences. Knowing each other better and finding out how to compromise on living together are important matters. I was over there in May and I think it was my twenty-eighth trip or so. That's probably taken a year out of my life right there.

FERGUSON: Do you speak Russian?

DOTY: No, I don't. I took it for a while, but as I said before, I'm so poor at languages.

FERGUSON: What's your position on the Strategic Defense Initiative?

DOTY: I think that is a major technical error and that we should be putting our technical resources elsewhere, but it's very divisive and the chances of it working are just about zero.

FERGUSON: It seemed to me that it was a great pity. It was something that was worthless and expensive. They could have traded away—

DOTY: I hope they still will. Going back to 1952: because I made these rounds in Europe in the late 1940s and had gotten to know a few people, I was invited by the French to be a pall bearer at the State funeral for [Paul] Langevin and [Jean] Perrin. They had both died during the war. I went over and participated in this great ceremony, learning only then that they had both joined the Communist party in the last months of their lives. I don't think that would have altered it. Then when I came back and applied for a Fulbright fellowship in 1954, I learned that I was first on the list but was denied it. It turned out that Mr. [Senator Joseph] McCarthy had prevailed against me because of my participation in that State funeral. As a result of that, I was

put on the NIH blacklist for serving on committees and study sections. Although I got my first top secret clearance in 1957, and a lot more after that, it didn't become "pure" for the NIH until about 1975. By that time I had missed being on dozens of study sections and other very demanding work that most people have to do to make sure the grants get distributed. That was an amusing aside.

FERGUSON: You seem to have done well on grants nevertheless.

DOTY: Yes, it has been good. The NIH has been very good to me. We're in our 34th or 35th year. Even that scene is changing a lot now. It's gotten so big.

FERGUSON: That leads to another broad question I've been asking people. How do you feel about the changes from when you started to now, with respect to science funding, science policy, et cetera? Are you optimistic or gloomy?

DOTY: I think, on the one hand, that we probably do it better than any other nation and therefore we have a lot to show for it. So from that point of view, the cup is certainly half full.

FERGUSON: Do you think we're doing it better than the Japanese?

DOTY: Oh, yes, I think so. The Japanese do many things better, but not basic science.

FERGUSON: Are the Japanese doing well in your field of biology?

DOTY: Well, they're putting a lot of effort into it now. What they lack, and we do so well, is the backing of young people early on with almost unlimited possibilities if they do very well. But in the academic university system in Japan, they refuse to pass judgment on each other, so peer review will never work there. Consequently, every professor and every assistant professor gets the same amount of research funds. The only way you can grow out of that very limited system is to join some other industry or a nationwide laboratory. These are quite big. You can go there and while you get more equipment, you can't rise very rapidly because of the Japanese cultural scene. I don't think there is any place that matches the U.S. in opportunities that it offers people in their thirties, and that's much of our strength.

FERGUSON: What about the Soviet Union? Did you get much insight into them?

DOTY: Yes, in fact I still visit my biochemical friends and other scientists when I go there. I got to know most of their great physicists in the early years. I knew [Peter] Kapitza extremely well. He was a close personal friend. He died two years ago. I knew others quite well: Tamm, Sakharov, et cetera, but I think the story there is much what is widely understood now, that the whole cultural and social-political system does not reward inventiveness, nor does it allow it to flourish and demonstrate its effectiveness. There's almost no competence in the translation of basic findings into technical accomplishments. They try hard, but they end up having to buy it or steal it from the West.

You see almost nothing in Moscow that is home invented. They're very good in sports, dance, and music and they do organize things well. Their cities and subways are much better organized than ours. But in terms of making science work, theirs only really works well in space and of course, the military. Their space program is, in some respects, better than ours, but that goes back long before the revolution. They had their own space enthusiasts, as we had ours, in the early part of the century. The Russians and the Germans and Americans were the only ones who had that cultural background of wanting to do big space science which had to be state supported because it could not be privately supported.

FERGUSON: I had a friend who made several trips to the Soviet Union. He had the feeling that they had a two-tiered system there. The military had excellent science and engineering but the rest of the economy was sort of second best. Is that what you're saying?

DOTY: I think so, but, of course, the military gets a lot of special favors and an enormous amount of money. They're forced to compete with the West in the military sphere, whereas in the commercial sphere, as long as the people don't clamor for too much, they don't feel forced to produce and make the consumer operation efficient and the servant of the people. Therefore, they are twenty to thirty years behind the state of consumer development in the West.

But it's an interesting thing. The people I see there, particularly in biochemistry, make me think that but for the grace of God, I would be there. I think that to be there and decide that the ties to the homeland and family are strong and to stay there, then you have to do the best that you can. It's a challenge that I think none of us would pull off very gracefully. The isotope labeled biological compounds, which are so essential to the work I've been describing, are made by dozens of little companies here. There's nothing equivalent there. They have to wait six months to get their order in, and then it depends somewhat on how good a party member you are. They have almost no work in the labs at night. It comes from the Revolution, where the primary thing to propagate in the new Soviet man was not to be exploited. Therefore, nobody should work more than forty hours a week. The director of a large laboratory unit has to get

three different people to sign off if he wants to come back to work on the weekend. It's a really different environment.

FERGUSON: I think you've already answered one of the primary questions that I always ask—if you had it to do all over again, would you go the way you went?

DOTY: Well, I would have been a better scientist if I had stuck to my bench. I would have been a better politician if I had gone to Washington. I think people end up doing pretty much what they want when they have the choice, and I've had the choice. The University has been good to me. They did nothing but encourage me when I set up the Center for Science International Affairs. So I've been well treated.

FERGUSON: Has recruiting into biochemistry and molecular biology research been easy?

DOTY: Yes, I think it has been, compared to the rest of science. In the last five years it has become more difficult. I suppose it's a complex cultural thing we don't understand. A smaller fraction of our brighter people are going into science. There is a trend: off to better paying professions.

FERGUSON: Molecular biology has a really excellent press.

DOTY: Yes, and I think the growth of the biotechnology industries is at least offering a financially attractive future for people who want to go that way. I think it will sort of develop a fairly substantial industrial base like chemistry did back in the middle of the century.

FERGUSON: Where have most of your graduate students gone for jobs and later careers?

DOTY: They've almost all gone into academia, but in the last five years, they're going into biochemical industries more. The last three have gone into biochemical industries. That's an exciting phase of its own right now. Coming back to the other side of the half-empty part of science, funding, I think this is across the board in basic science. While the National Science Foundation figures show a continuing rise, it isn't as great as it appears. I think research is getting more expensive. The inflation index for research equipment goes up much faster than for the common consumer products. The willingness to go through the long hard hours that they need in their late twenties and early thirties, at very low salaries, is taking its toll.

I think the management of the scientific enterprise is in pretty poor shape. It shouldn't be overmanaged. But to return to funding, the fact is that the sort of rule of thumb established in the government was that, in addition to the National Science Foundation and the NIH, each of the agencies which use science should contribute to basic science in some proportion. Defense is the biggest and it has the biggest R&D budget ever now—sixty billion dollars—but it's contributing less to basic science than it ever did before. That's a very severe imbalance. It shouldn't be overmanaged, but there's almost no general management in Washington. The science advisor is almost nonexistent in policy terms. The allocation of resources isn't bad; indeed it's better than I expected, but it's far from optimal. So we could be doing much better, but I can't shed any false tears. It isn't too bad.

FERGUSON: We touched briefly on your own personal life. Your son was named Gordon Sutherland. What's he doing now?

DOTY: He's head of the West coast branch of Fuji Bicycle. He was always a wheel man, so he's quite happy in San Francisco in a large-scale bicycle business.

FERGUSON: Did he go to college?

DOTY: No. He went some to the State University. None of the children have gone to college until recently. Our middle daughter is a senior at Babson [College] and will graduate in June.

FERGUSON: They had no interest in college?

DOTY: Partly. They were extremely dyslexic, much more so than I. All of them had trouble reading.

FERGUSON: I see. Was that recognized early enough?

DOTY: No. That's it. We went through an awful lot of experiments, many of which were not successful. Now it is much more straightforward as to what should be done, but they came at a bad period.

FERGUSON: Is this a hereditary thing?

DOTY: It looks like it is.

FERGUSON: I've never realized that. Marcia is your eldest daughter?

DOTY: Yes. She was just married a month ago. We went to San Francisco for that. Rebecca is in Babson. Kate runs a travel business out in San Francisco.

FERGUSON: You mentioned a number of distinguished, outstanding people as you went along. I don't know if there are some others who might have impressed you over the years—either of your cohorts or your students?

DOTY: I think I mentioned the major ones. Walter Stockmayer was a big influence when I was young. He was an instructor at Columbia when I was a graduate student. He was a fine fellow. I spent my first twenty years here in the chemistry department. That made a great training ground for sober judgment and so on.

FERGUSON: It's interesting. Of the major professors here, Doty and Kistiakowsky never did a paper together and neither did Wilson. They were all close personal friends?

DOTY: That's right. It was a collection of little empires. I think Moffitt was a person who moved back and forth much more, partly because he didn't have many graduate students. He was a real theorist.

FERGUSON: It seemed to me that Harvard had some of the character of the European Institutes where each guy had his own empire.

DOTY: I think that's it, but it's different from the European Institute. Here you have to make your own show work and be judged by your peers and so on. What's wrong in Europe, particularly in the Max Planck Institutes, is that they award the directorship to a person who is in his early forties, and he's really a supreme boss. He has an enormous apparatus: not a research group, but a whole department. Yet there's no one else for him to compete against in his field. He often tends to choose a field for the rest of his life that isn't too competitive. He settles down

and does maybe first-class work on a second-class problem. I think that places a very heavy burden on German science. I guess I'll leave it at that.

[END OF TAPE, SIDE 4]

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INDEX