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HERBERT TABOR

Transcript of an Interview
Conducted by

James J. Bohning

at

National Institutes of Health, Bethesda, Maryland

on

3 April 1993

(With Subsequent Corrections and Additions)

CHEMICAL HERITAGE FOUNDATION
Center for Oral History
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HERBERT TABOR

1918 Born in New York City on 28 November

Education

1937 A.B., biochemical sciences, Harvard University
1941 M.D., Harvard University

Professional Experience

Harvard University
1941-1942 Researcher, Department of Biological Chemistry

Yale Medical School
1942-1943 Intern in medicine, New Haven Hospital

United States Public Health Service
1943-1983 Commissioned Officer
1943-1943 U.S. Marine Hospital and U.S. Coast Guard

Laboratory of Biochemical Pharmacology, National Institute of Diabetes and Digestive and Kidney Diseases, National Institutes of Health
1943 Staff Member
1962- Chief

Journal of Biological Chemistry
1961-1966 Editorial Board
1968-1971 Associate editor
1971- Editor-in-Chief

Honors

1956 Ninth Annual Arthur S. Flemming Award
1970 Meritorious Service Medal, U.S. Public Health Service
1971 Fellow, American Academy of Arts and Sciences
1977 Member, National Academy of Science
1986 Co-recipient, Hillebrand Prize, American Chemical Society
1994 Co-recipient, Rose Award of the American Society of Biochemistry and Molecular Biology

ABSTRACT

Herbert Tabor begins this interview with a discussion of his family and childhood. He grew up during the Depression in Manhattan, New York, and attended local public schools before becoming at student at City College in 1933. After spending two years at City College, he transferred to Harvard University, where he graduated with an A.B. in biochemical science in 1937 and his M.D. in 1941. While at Harvard, Tabor was influenced by several of his professors to pursue biochemistry rather than move into a clinical discipline. In 1942, Tabor began an internship at New Haven Hospital, where he was exposed to aspects of both clinical and biochemical medicine. After his internship at New Haven Hospital ended in 1943, Tabor entered the Public Health Service of the National Institutes of Health (NIH) and worked closely with Dr. Sanford M. Rosenthal, then head of pharmacology at the NIH. Tabor and Dr. Rosenthal studied electrolyte changes in burns and shock and determined how to treat burn and shock victims using saline instead of plasma. This research proved extremely important during World War II, when there was little or no plasma available. While with the Public Health Service, Tabor was assigned to be a medical officer of one of the U.S. Coast Guard cutters. He made three round trips to Scotland and North Africa providing medical care. In 1961, Tabor joined the editorial board of the *Journal of Biological Chemistry (JBC).* Later, he advanced to Associate Editor before becoming Editor in Chief in 1971, a position he still holds today. While with the *JBC*, Tabor developed the *Minireview Compendium*, which is a yearly compilation of all short reviews published in the *JBC* for a particular year. Tabor discusses the importance of computer technology in advancing the usage and availability of the *JBC* in today's world. Tabor concludes the interview with a discussion on the future of the *JBC* and electronic journal availability.

INTERVIEWER

James J. Bohning is currently a professor of at Lehigh University. He has served as 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 written for the American Chemical Society News Service, and 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.

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INTERVIEWER: James J. Bohning

LOCATION: National Institutes of Health
Bethesda, Maryland

DATE: 3 April 1993

BOHNING: I know you were born in New York City on November 28, 1918. Could you tell me something about your parents and your family background?

TABOR: My father owned a small wholesale business in New York, in downtown Manhattan.

BOHNING: Did you live in Manhattan?

TABOR: I lived in Manhattan, and I went to elementary school in New York City. I went to Townsend Harris High School, which was part of City College and had an accelerated program. The whole education system was accelerated then, so you did four years of high school in three, and the three years of junior high in two years. Many people also accelerated one year in the elementary school. I graduated from school at just fourteen and a half, but it wasn't that unusual for the period.

BOHNING: I didn't realize it was that common to do that. Do you have any brothers or sisters?

TABOR: I had two sisters, both of whom died.

BOHNING: What elementary school was it?

TABOR: It was number 169. It was right near where Columbia College of Physicians and Surgeons [P & S] is located now. That's not completely irrelevant, because it was being built while I was walking back and forth to the elementary school. I don't know what influence that had, but it was rather exciting to see it go up.

BOHNING: What was it like growing up in New York in those days?

TABOR: In many ways, it was a lot different than now. That area was probably a little more like what we would call a suburban area now. That far uptown Manhattan was relatively unbuilt. It was along the river and very pleasant. The place where Columbia P & S is now located was—I am told—the major New York baseball field around 1918 and 1919.

BOHNING: Where did your interest in science develop?

TABOR: I've been thinking about this, knowing that you were coming down, and that's very hard to pinpoint. I think it occurred a little later than the period we are talking about. I think many people in my generation were influenced by some of the classic books, like *Microbe Hunters* (1), *Rats, Lice and History* (2) and *Arrowsmith* (3). It was quite clear to anybody that there were big developments in science and medicine.

I think that at that time I wasn't sophisticated enough to realize the difference, to the extent that there are differences between medicine and science. At that time the development of medicine became more dependent on and associated with the developments in biological sciences. At that time, many, although not all, of the top scientific people in the medically related sciences had been trained as M.D.s, but there were, of course, a fair number of Ph.D.s in these areas.

It's pretty hard to put my finger on when my interest in science started. I was in elementary school beginning in 1924, and that was really just when medicine became more scientific. Vaccinations were developing, and that was exciting.

BOHNING: What about in elementary or high school? Were there any teachers that had any influence on you?

TABOR: I wouldn't think so from that period. Perhaps it's my naivete, but my recollection is that they were all pretty good. They were all enthusiastic, but there was no one person in particular.

BOHNING: It wasn't unusual at that time to have Ph.D.s teaching in high school. Did you have any?

TABOR: I would doubt it. I just don't know. I probably wasn't sophisticated enough at that time to register whether they were or weren't. The high school was a particularly interesting

one, because you took three years for the four-year course. If you came from the junior high school system, you did the three years in two years, so it didn't take much time. It was not part of the regular city system. We had to take an exam to get in, and even though a lot of people took the exam, it was still a small school, part of the City College system. The teachers were not from the regular school system. I have no data on it, but if you were to go over its graduates, they probably all have done quite well. It was really very special.

This was just at the beginning of the Depression. This was a school that was in downtown Manhattan, on the East Side. We went by subway through the city and walked across from west to east. At a relatively young age, you became quite aware of the problems of the big city and of the Depression.

BOHNING: Did the Depression have any effect on your family?

TABOR: Not really. Potentially, yes, because there was a certain insecurity if you were in business. Somehow we were sheltered. We didn't have expensive tastes. We lived in a modest apartment and did not have a car. I would say that except in terms of the implications of the Depression for everybody, it didn't have any personal effect. I don't think my life was any different because of it. I was quite fortunate in the sense that my parents had been quite conservative and frugal, and so we managed without any real inconvenience.

BOHNING: Was there any particular reason you selected this school?

TABOR: It was a challenge because it was a special school. It might be a little pompous to say, but probably I selected it because it was a school that had a better reputation, and as I mentioned earlier, you had to pass an exam to get in. The fact that you got through faster might have seemed relevant at that time, although at my age now, a year, more or less, hardly seems very important. [laughter]

BOHNING: The reason I asked is that I think there are other schools in New York that were also competitive and had entrance exams.

TABOR: Not at that time. You are thinking of schools like the Bronx High School of Science.

BOHNING: Yes.

TABOR: Those were all later. This was the only one at the time. I don't remember the size, but I doubt if the whole school was over a thousand, probably less. Compared to what I remember, the enrollment for many other high schools was around fifteen thousand, so this represented a very real difference. I would assume it was the challenge of a better school with smaller classes and so forth.

BOHNING: Did your parents have any influence on these decisions?

TABOR: I assume they did, but I don't remember. They certainly went along with me and supported me very much.

BOHNING: What was the curriculum like, and what kind of courses did you take?

TABOR: Technically, and in an historic sense, the school was set up as a preparatory school for City College. It was set up in 1846, if I have my dates right, and I think that's where the name Townsend Harris came from. I don't know why they named it after him. The atmosphere was such that it was clearly a pre-college program, very academically oriented.

Arthur Kornberg came from a somewhat similar a background—but a different high school—and you know his book (4).

BOHNING: Yes.

TABOR: Science and medicine and the intellectual approach were the popular thing in our Jewish community in that period. You asked about my parents supporting me. They completely supported me because it was a thing that they, without any specific plans or knowledge, thought was important. It was much more important than, say, going into business. In this day and age, I think that might be a little bit different. [laughter]

BOHNING: Were your parents immigrants, or were they born here?

TABOR: My father came over at the age of three, and my mother was born here. But the whole community was one that was in favor of this kind of thing. If one was interested in it, that was the thing to do.

BOHNING: Were there many books in your home? Was there an exposure to music and reading?

TABOR: Yes, there was, but just in the usual way. Anything intellectual in the way of reading and music was something that both my family and the community were in favor of.

Coming back to the high school, there were small classes and it was really very good. The problem with the school was one that I think was probably true of all schools in those days. While you can't dissociate it from one's own lack of maturity, the education was, even if extensive, somewhat rote. I think it was much better than the other schools, but it didn't have the give-and-take of thinking, the broad thinking that our children receive in the schools these days. But the classes were small, and it was quite a good background.

BOHNING: Was there any emphasis on science, and were there sufficient humanities as well?

TABOR: No, there was no emphasis on science. They had a science course. I think there was only one, but maybe there were more. I don't remember laboratory work of any kind.

BOHNING: As you were going through this school, did you give any thought as to what you wanted to do once you graduated? You were graduating at a very young age.

TABOR: As I said above, I was always interested in both science and medicine. The combination of science and medicine, with perhaps a little more emphasis on the medical aspect, was intriguing to me (although I am sure that I was not entirely clear on what this meant). The classical function of a doctor helping patients was certainly a positive factor in my thinking at that time. I should add, however, that I cannot be sure that these interests were not partly stimulated by the fact that science and medicine were considered very highly in the community, and that both family and the general environment encouraged this approach.

BOHNING: Had you thought about going to City College?

TABOR: I went to City College for two years. Partly, it was a routine thing to do from this high school. In addition, I was fourteen, and it was during the Depression. For whatever reason, I just didn't give it too much thought. I went to City College for two years and then transferred to Harvard. I was in the same class at City College, by the way, as Arthur Kornberg.

BOHNING: Your paths crossed again down here.

TABOR: Yes. We may have met once prior to NIH [National Institutes of Health], but I don't remember really knowing him until we came here. When we compared notes we were a little unclear on that.

BOHNING: You started at City College in 1933.

TABOR: Yes. This has some relevance, because this was in the middle of the Depression.

BOHNING: In those days, City College was certainly one of the outstanding institutions.

TABOR: That's right. That was another reason why I went. There wasn't any question that it was a very good place to go to.

BOHNING: It was also, as I understand it, a site of political unrest. Were you caught up in any of that?

TABOR: No, not in any defined sense of joining any organizations. There's no question that all of the political discussion et cetera that went on gave you much more of an awareness of the problems of the Depression, the problems of political unrest in Europe, and a general social awareness. I think that's not as unusual now at colleges. This would not have been true at all universities in that period, but it certainly was true at City College.

BOHNING: What did you select as a major?

TABOR: I was in the sciences. I don't know what it was technically called, but I was definitely in the pre-med category.

BOHNING: What kind of courses did you take in those two years?

TABOR: I certainly took qualitative and perhaps quantitative analysis, and biology. The way the curriculum was set up, for the most part the first two years were all relatively general courses. I did not take organic chemistry or physical chemistry there. I remember that in general biology there was a fair amount of invertebrate work.

BOHNING: This is where you really had your first laboratory experience?

TABOR: That's right. I think I also took a biology course one summer, because if I'd stayed at City College, I would have finished in three years instead of four.

[END OF TAPE, SIDE 1]

BOHNING: Did you make any comparison between chemistry and biology, as to which interested you more?

TABOR: No, I don't think so. I probably wasn't sophisticated enough to make those distinctions. I also think that it wasn't as clear to us at that time as it is now that all biology is really chemistry (as Arthur Kornberg continually emphasizes). The two disciplines were much more separate then, and I found both subjects very interesting.

BOHNING: You spent two years there before you went to Harvard. When you started, had you planned to finish your four years at City College?

TABOR: I think I had planned to finish the four years there. I was thinking before you came about why I shifted. It was on the advice of a very influential assistant dean, Dr. Gottschalk. He recommended that I should shift. I think that for the record one has to explain why.

I can give two different reasons. One is, City College was extremely good. The reason is that they had very good, bright, eager students and a dedicated faculty. I think they all were very good. The classes, though, were relatively large. It was perhaps my fault, but unless you were terribly sophisticated, the courses were largely rote. If you were very good, you could get well beyond that. I'm talking now about the first two years. I think the later two years were quite different.

As you know, City College had a very impressive history of turning out good people. In fact, I think there were two or three Nobel Prize winners just from my class alone, Kornberg and one or two others. I think it would have been very good to stay, and, intellectually, it would have been very stimulating. Nevertheless, because of its size, you had a little less of a challenge. Harvard was quite a different kind of school in terms of the intellectual environment—smaller groups, more time for discussion, and so forth.

The other aspect—which was, I think, not irrelevant to the assistant dean's recommendation and relates to something that Arthur Kornberg mentioned in his book (4)—is important in any kind of history of the period, which is that if you wanted to go to medical school, and wanted to approach science and medicine from that point of view, it was almost impossible to go to medical school from City College. Despite the very good record that City College had, out of two to three hundred premed graduates each year, usually only three or four got into medical school. I might be off by a few, but not by much. It was just about impossible. Columbia had a scholarship that was set up around 1920 for students from City College going to Columbia Medical School [College of Physicians and Surgeons]. I was told that they only awarded the scholarship two or three times in fifteen or twenty years, because they said they couldn't find suitable students.

There was no question that if you wanted to go to medical school you would do better to go from another school. It was very nice of the assistant dean, who was obviously unhappy about this situation, to not try to hide this fact but to say, "If that's what you want to do, we have to be realistic about it." I think that was probably the specific inducement to move. As it worked out, I think it was also intellectually very beneficial. This inability of City College graduates to be admitted to medical school is a very interesting footnote on the times and on the prejudices of the period. Arthur Kornberg, as I said, goes into this in his book (4) in considerable detail.

As far as my family goes, they were quite supportive of the transfer to Harvard, in spite of the fact that that kind of tuition in the Depression represented a very sizeable amount of money.

BOHNING: City College was free, wasn't it?

TABOR: City College was free, yes. Thinking back on it, that was probably, at least psychologically, one reason why many people either went there or had to go there. Most of the people did not have the funds. Even if they did, it was sort of an inducement, because then you would have that tuition for graduate school or medical school. My family was quite supportive.

BOHNING: What was the tuition at Harvard?

TABOR: Tuition was four hundred dollars per year. Correcting for the relative cost of living at that time, it was quite a lot. Parenthetically, it was rather interesting that Harvard was comparatively cheap; the tuition at most other schools was around six hundred dollars.

I went to Harvard when I was sixteen-and-a-half, just going on seventeen. I was fortunate to be able to transfer into the junior year because they usually didn't do that. In that

respect, I'm still a little sorry. I would have liked to have had some more time up there. They were two very good years.

BOHNING: Was this your first time out of New York City and away from home, or had you traveled before that?

TABOR: No, I had not. That was a very interesting sort of experience.

BOHNING: Returning to the question of the problem of admission to medical school from City College, I remember Jerome Karle mentioning something similar (5).

TABOR: He was in the same class at City [College].

BOHNING: He had wanted to go to medical school and couldn't get in when he graduated from City.

TABOR: Yes. I think you will find that this was the story with a significant number of our very best scientists. Many of the people who did not get in to medical school went on to graduate school with the idea—which some of them pursued and some didn't—that after they did well in a master's program at some other school, they would then reapply to medical school. Some got in at that time. Others, perhaps fortunately for science, stayed in whatever graduate program they were going to and did fantastically well. I can think of a number of very notable people who probably got into real science at an earlier stage because of this.

BOHNING: Who would that have been?

TABOR: I can't be sure, because I don't know their backgrounds with certainty. You mentioned, for example, that Karle mentioned it to you. I hadn't known that. You had to be quite sophisticated in this environment to realize that if you wanted to do science, you could do it as well—in some ways better—by getting a Ph.D. in science than by going to medical school. I think most of the people I knew were not sophisticated enough or knowledgeable enough to know that.

Plus there was the economic thing. In those days, as I'm sure you know, salaries and support in the sciences were minimal. Medicine was a way that many people thought they could get into science. But a number of people, for the reasons I've mentioned, went into science

directly. I think this was very good for science. It's a very important historic comment, and that's why Arthur Kornberg mentioned this so much in his book (4).

BOHNING: If I remember correctly, I think Karle started that route that you mentioned and went to Harvard and got a master's degree, still hoping to get into medical school.

TABOR: I didn't know that, but that would be consistent.

BOHNING: He still wasn't successful in getting into medical school, and I think that's when he went out to Michigan.

TABOR: I would not be surprised that if you delved into the past of some of our very distinguished scientists, you would find this was relevant. It doesn't have to be as clear-cut as this. It might have been that these individuals were considering both alternatives. However, if it weren't for this inability to get into medical school, they would have gone to medical school. It would have been a real loss for science if some of these superb people had gone into the practice of medicine at the local level, although I am sure that they would have been excellent physicians.

On the other hand, even though in many cases this worked out well in the long run, this does not excuse the situation. It would be of interest to know more details from the admissions committees on what kind of discussions went on during their deliberations, but very few of the members of these committees are still alive. The overall pattern, including what you have just told me about Karle and the material in Kornberg's book (4), unfortunately clearly indicates the pattern of discrimination and restriction.

There was one other factor that was somewhat relevant. I concentrated on Columbia because it was a local school. They did not want to take all of their students from New York. In the reverse, and this is true now too, but not to the same extent, medical schools in state universities in other states did not take out-of-state residents, or very few. For both these reasons, and particularly because of the discriminatory aspects that I have just discussed, it was very difficult for students from New York City to get into medical schools. In this connection it is important to point out that most of the students at City College in those days were Jewish, and the role of anti-Semitism in medical school admissions at that time vas very clear.

BOHNING: NYU had a medical school. Wasn't the Cornell medical center in New York, too?

TABOR: Yes, but the same factors applied. As I mentioned it is perhaps not unreasonable for a school to take only a certain percentage of people from the local area or from any one area if it

wants to be broadly based. An average medical school, which only had an enrollment one hundred to two hundred students in a class, might not want to take too many students from New York City. However, it is clear that the restrictions were much greater than could be explained on a geographic basis. The geographic distribution was used as an excuse for the discriminatory restrictions. Be that as it may, this was a fact of life in the community, and I think is relevant in terms of what I was saying about stimulating a lot of good people to go into science instead of medicine.

Arthur Kornberg was an outstanding student with an outstanding record, and he was accepted at the University of Rochester Medical School. He was only one of five in his City College class who was accepted at any medical school at the time of graduation. I think that he points this out in his book (4). Maybe there were a few more later who were admitted after receiving their master's degree.

BOHNING: Were there any others like yourself who were advised or assisted to leave after two years?

TABOR: Not that I know of. I know a few people who left. A good friend of mine who was in geology, and with whom I later roomed for a year at Harvard, moved. There again, you get into the other aspect that I was talking about. In any particular specialty, you had much more intellectual opportunity at Harvard than you did at City, which is no criticism of City College's education. It's just that, if you were at a school with an extensive graduate program, that rubbed off on the undergraduates. Certainly, that was true in my friend's case in geology and in my case. The fact does remain that if you're at a big university that has an extremely good chemistry program, it rubs off on the undergraduates. You have the tutorial system at Harvard, which was a very real advantage that you couldn't match in a metropolitan undergraduate school. Nevertheless, City College did awfully well, and they have a good record of people who came out of it at the time.

BOHNING: When you got to Harvard did you continue both chemistry and biology?

TABOR: Yes. There was a so-called premedical type curriculum, which was in the biochemical sciences. At that time Harvard University, the Cambridge part, surprisingly had no laboratory course in biochemistry. There was a non-laboratory biochemistry course, Chemistry 15, in the chemistry department. There were, of course, an extremely good chemistry department and a very good biology department, but there was no biochemistry department in Cambridge.

The medical school, of course, had a good biochemistry department, but the medical school and the undergraduate schools were fairly separate at that time. The undergraduate biochemistry tutorial program was really quite good. John T. Edsall was the chairman of the

Board of Tutors in the Biochemical Sciences, and each student met weekly with his tutor. As part of the house plan, you had lunch with different people, including the tutors. It was just at the period in 1935 when biochemistry was really exploding, primarily in Europe. I think the explosion had not yet really occurred in biochemistry in this country.

My tutor was Bob [Robert E.] Johnson, who later became chairman of physiology at Illinois. He had just come back from Oxford in Britain, where he had been a Rhodes Scholar in physiology with [John B. S.] Haldane and the whole group at Oxford that was doing such exciting work in physiology. He already had his Ph.D. It was really a very exciting period in physiology. My reading was in that area, and it was really very stimulating. Later he was in my class in medical school. He applied to medical school at the same time I did, which is a little amusing. He had a very good background in modern physiology, which was very nice for me.

The subject of my senior thesis was the metabolism of carotene. In carotenemia, people who can't handle carotene well get a jaundiced-looking appearance. This work was done under the guidance of Bob Johnson. He was at the Fatigue lab, which was at the Harvard Business School, right across the river and in the basement. I did the work there. This laboratory was one of the very top-notch laboratories in physiology. They had visitors, people like Roughton (who was famous later for his work on carbonic anhydrase) and others. I was only there for two years, but it was a very exciting period.

BOHNING: Did you have a chance to select the topic of your senior thesis or was it assigned to you?

TABOR: I think you could select it from whatever was around. In this case, I hadn't thought of it myself; it was quite specialized. It must have been that my tutor heard about this individual and discussed the topic with me, because I can't believe I would have spontaneously chosen it. I wouldn't have known that there was a case of carotenemia. It wasn't much of a contribution, and it wasn't perhaps even a very exciting subject, but anything of that sort was exciting to me by definition. (On the basis of this work I received my Bachelors degree with *Magna cum Laude* distinction.

In addition to my tutorial readings related to physiology, other tutorial readings and the Chemistry 15 course emphasized hemoglobin and the various components of blood and their interactions. In this country this was one of the most popular research areas. This was the area mainly covered in the lectures by Lawrence J. Henderson who was the Professor in charge of this course. Henderson was well known for his classic studies on the blood system, and had written a classic book in 1913, *The Fitness of the Environment* (6).

Jesse Greenstein (who later was at the National Cancer Institute) was the section man in charge of the course and covered a variety of other biochemical subjects.

[END OF TAPE, SIDE 2]

BOHNING: Did you take a specific physical chemistry course as an undergraduate?

TABOR: Yes, I took the elementary physical chemistry course. Actually, I'm glad you brought that up. The physical chemistry course was given by E. Bright Wilson, Jr., who had been appointed not too long before. He was a full professor at twenty-nine. He was vibrant and exciting. This course tied in very nicely with another course I took the same year. In 1936, I took a course in atomic physics with Oldenberg. This course gave me a good background for understanding the discussions of nuclear power and of the atomic bomb nine years later.

In another course that I took at the same time—in philosophy—the section man was someone who had obtained his degree or had worked with Eddington, and so the combination of these different disciplines was really very exciting. I also had organic chemistry with Louis Fieser. That was very, very special. He was sparkling as a teacher. On the other hand, I think that it was a little unfortunate that I took chemistry at a time just before the teaching was based on electronic structures and the resultant mechanisms. Even though the teaching was exciting and done well, it was still old-fashioned organic chemistry. I've tried hard to make up for this, but it is hard to make up for not having this more modern background.

BOHNING: I think that it is important to go back and look at this background in some detail, to look at these windows on curriculum and departments, and what people were like, such as Wilson and Fieser, whom you've just mentioned.

TABOR: You know better than I do. These things perhaps all add up. Even if it's not relevant to me and what we are talking about, some of it might be relevant to the general picture.

BOHNING: What about mathematics?

TABOR: I just had the routine kind, and only at City College. I didn't have any mathematics at Harvard. I went through the usual calculus and differential equations. That was a real loss too, because while I enjoyed the courses, unless you have a little more mathematics, you don't have a solid enough feeling for it. This lack of background makes it very difficult for me to really understand articles based largely on mathematical treatments. In the seminars at NIH, we once had a series on quantum mechanics, and I found that if you don't have a good background in math it's impossible to really follow the material. I wish I had taken more math. On the other hand, I wish I had taken more literature and so forth, too. [laughter] Unfortunately, there wasn't enough time.

BOHNING: You mentioned that you roomed with a friend from City who was in geology. Were there any other relationships you developed while you were there as an undergraduate?

TABOR: Oh, quite a few. Many of them were people who went on in science or medicine, and I've kept up with them over the years. Harvard was a very good place from that point of view.

BOHNING: Would you say that John Edsall was influential at that point in terms of your future career? You mentioned his involvement in biochemistry.

TABOR: No, I think it was just the biochemistry. John wasn't my personal tutor; he was chairman of the Board of Tutors in the Biochemical Sciences. I found all of the people who were in this field very exciting. However, for the most part I had my mind made up about what I really wanted to do. I can't say one person or another was particularly influential.

I do want to emphasize at this point that John Edsall was (and is) a very special individual. In a very real sense, he does represent the ideal approach to science.

BOHNING: I guess what I'm referring to is the track of using an M.D. as a practicing physician as opposed to the track of doing science and whether you had thought about those two tracks.

TABOR: Going back to what I said, I think that from reading the books that everybody in that period read, I didn't realize the distinction. I planned to go into medicine from the scientific point of view. I don't think I intended to stay out of the practice of medicine completely, especially with the attraction of curing patients, but I certainly did not intend to do local medical practice.

The other thing I should mention for the historical record is something that is true about City College and to a lesser extent about Harvard too. It's still a real defect in premedical training, and I'm sure you're familiar with it. There's still a perceived difficulty in getting into medical school which induces the undergraduates to not take as much advantage as they should of the other aspects of the university. I feel fortunate that I did try to take advantage of the general educational aspects of the college, despite the time that the laboratory courses took and the general pressures of getting into medical school. I can't underestimate this negative aspect of pre-medical education, and it's still true, I'm afraid, from what I hear. I don't know the answer, but it's unfortunate.

BOHNING: I taught chemistry for thirty years in a liberal arts school where there were a number of premeds.

TABOR: You understand the problem.

BOHNING: Yes. [laughter] I understand what you're saying.

TABOR: I guess for the most part the medical schools, provided the students are good, prefer someone from the small school, or didn't you find that?

BOHNING: We had some very good successes. Several of our students went to Harvard and Penn [University of Pennsylvania]. If they were good, they were pretty successful. It's that attitude that you see, especially early on. In the first two years, there's a particular attitude in the students that is almost anti-intellectual.

TABOR: That's right. It's really too bad.

BOHNING: They're not learning for the fun of learning but worrying about a long-term goal instead.

TABOR: Although it wasn't completely the case, when I was speaking of the excitement, there was enough of that atmosphere of learning and of the intellectual goals of other people who were in other disciplines, including the graduate schools, to affect the pressures of the premedical curriculum. But you can't underestimate the fact that there was pressure.

BOHNING: It must have been pretty intense at City then, if you had this large number of students who had that goal who virtually knew that their success rate was going to be very low.

TABOR: Yes, but not quite the way you're saying it, because everyone realized that that was the way it was. I think people did the best they could, but I never got the feeling that there was any cutthroat competition of any kind. Everyone tried to do what he or she did best. Everyone worked very hard in this group, but I never thought of it as competition. As with a lot of these things, at the time you're doing it, you don't think of what the overall picture is. You just did the best that you could.

BOHNING: Do you think that people at that time were perhaps more accepting of the status quo or the conditions that were around them, as opposed to fighting those conditions to change them?

TABOR: I think it varied a lot with the people. It certainly was not true, in terms of what you were asking before, of those people who were aware of the socioeconomic aspects of the times. Perhaps people were more adjusted to social barriers and limitations. Certainly the groups at City College and at Harvard were not happy with the economic inequities in society. Perhaps at Harvard it was a less personal thing; it was in many cases a more conceptual thing, the rights and wrongs of society.

BOHNING: Would you classify Harvard as being more conservative than City at that time?

TABOR: Certainly across the board it would be, because there was a fair percentage of conservative and wealthy people at Harvard. On the other hand, of the people who were, shall we say liberal, I would say probably not. Obviously, the percentage was much smaller, and it was a much more intellectual liberalism. The thing I do find a little surprising, thinking back on it and comparing it to nowadays, is that there was very little that I remember of real activism on the part of most of the students. People were very opinionated, in terms of, for example, presidential elections and being pro-Roosevelt or pro-New Deal, but I don't remember them working for the Democratic Party or what have you. I am excluding the small number that might have worked for left-wing parties. In general, there was a real interest in intellectual discussion, but one of the things I find a little bit amazing is that I don't remember people taking political action in a presidential campaign. But perhaps I just didn't know what was going on.

BOHNING: This is as opposed to City where I would expect there would have been more activism.

TABOR: Even there I don't remember people participating as part of the regular electoral process at all. Again, I was only in each place for two years, so I may be giving the wrong impression.

BOHNING: Had you planned on staying on at Harvard for your M.D., or had you thought about other places?

TABOR: That was where I wanted to go, of course. If I hadn't gotten in, I would have thought of other places. But fortunately, I was accepted, which was nice.

BOHNING: That was 1937?

TABOR: Yes. I started in 1937.

BOHNING: There was a four-year period during which you were working on your M.D. I'd like to talk somewhat about those experiences, but I would also like to look at the larger changing political scene. The situation in Europe was deteriorating at that time. Were you aware of that or did that influence you?

TABOR: Everybody was obviously terribly aware of what was happening in Europe. Frankly, the thing I found remarkable was how much people went about their education, knowing what was going on in Europe and being upset by it, but just going ahead with their regular activities.

I think at City College there was much more of an obvious awareness because people were more personally involved in the economic depression and the enormous impact that had on this country, as well as the fact that things were obviously unraveling fast in Europe. In addition, the plight of the Jews in Germany had a particularly strong and personal effect on the Jewish students at City College.

At the medical school I think it was much less apparent, except to a small group. This was partly because in the medical school there were more people from across the country than in Harvard College, although I don't know the figures. In Harvard College you had a very large percentage of people from the eastern seaboard. At the medical school there were many more people from across the country, and it was a small school. I would say that the medical school had much more of a conservative environment within the student body.

BOHNING: What was Harvard Medical School like in 1937?

TABOR: It was a terrific place. Except for a few departments (such as Biochemistry and Physiology), it was perhaps less scientific than it is now. Now it's a center of basic scientific research. At that time, I think Cambridge was more involved in basic research.

Fortunately, the biochemistry course was one of the best. It was given by [A.] Baird Hastings. I was very close to him and worked in that department over the years. He had a very strong influence on me. He was very young, and he had lectured at Harvard College in the Chemistry 15 course, which was where I remember first hearing him lecture. He was about twenty-nine when he became the chairman of the department. He took an extreme interest in teaching and in the individual student. (As an example of his personal interest in each student,

if one of the students had a minor accident in the laboratory, he would call him up at night to make sure he was all right.) I found his course very, very stimulating.

Hastings' course was excellent. I could appreciate his course a little more than many of the students because of the background that I had at Harvard College in the sciences. I think that some of the medical students, who did not have this kind of background and who were really only interested in the practice of medicine, could not understand why nomograms of the interaction of hemoglobin, CO_2, and oxygen were really relevant to them.

In retrospect, this was very silly. I have often pointed out that over the years, many areas of biochemistry that seemed somewhat esoteric at the time have gradually become the cornerstones of important aspects of clinical medicine twenty years later. What we learned about acid-base equilibrium in the Hastings biochemistry course, for example, is now so well accepted in clinical medicine that you usually find it covered now in clinical textbooks rather than in biochemistry textbooks.

We also had a very good professor in physiology. He was Walter B. Cannon, who was one of the classic names in physiology. He was chairman of the department and gave us many lectures. Those two courses were particularly good.

BOHNING: Did you use a text in biochemistry?

TABOR: In general, we did not use the textbooks, but depended on the lectures. It was rather striking that the only texts available at the time were one by Meyer Bodansky (7) that was already several years old, and one by [Philip B.] Hawk and [Olaf] Bergheim (8).

There was a very nice laboratory in both biochemistry and physiology, in which the students actually did real problems, such as studies on exercise and on water balance. We used the Haldane apparatus in Physiology. In Hastings' course, he organized very unusual sessions in which small groups of the medical students used the Van Slyke manometric apparatus for real experiments. Hastings was personally very involved in these experiments, and participated personally with the students in these experiments. It was a very exceptional course, and I think that it was much better from what I heard than biochemistry courses anywhere else in this country.

We also, of course, had a course in gross anatomy, and surprisingly I did well enough in that to be a prosector the next year. Gross anatomy was what you would expect. It was very dry but interesting. We had histology, and again this was before all the new techniques such as electron microscopy were available. Still, the course was presented well. All in all, it was a very good year.

BOHNING: If I recall correctly, Bodansky's book (7) was titled *Physiological Chemistry* rather than *Biochemistry*.

TABOR: I think that American biochemistry was considered "physiological." It is a semantic question, but I think that this terminology did indicate this point of view. The books did not reflect in any way all of the work that was being done in Europe on enzymes, such as the work of [Otto] Warburg and others. It wasn't until some years later that a number of comprehensive books in biochemistry became available.

We had a number of very good people lecturing in the course. [Yellapragada] Subbarow, who later went to Lederle, and [Oliver Howe] Lowry, who later was chairman of pharmacology at Washington University. John [F.] Taylor also lectured as did Cyrus [H.] Fiske, who with Subbarow first discovered phosphocreatine in muscles.

[END OF TAPE, SIDE 3]

BOHNING: The biochemistry department was in the medical school, which I believe was not unusual at that time.

TABOR: That's right. There had been a biochemical course in previous years at the university, so-called Bio 3, but they hadn't given it in several years. The biochemistry course was at the medical school. Hastings himself was a Ph.D. He obtained his degree from Columbia around 1920. He was in the Public Health Service [PHS] as an "assistant sanitary chemist," and during World Was II, he was assigned to Columbia. He was really the first "pre-doctoral fellow" of the Public Health Service, which became a much bigger program after 1946. Then he went to Rockefeller and to Chicago, and then he came to Harvard. He was very interested in the teaching of the medical students, with the idea that they would be using biochemistry in their thinking and in the care of patients.

BOHNING: What was the second year like?

TABOR: The second year is a little more vague in my mind. It was very interesting in its own right, but not in relation to things we're talking about. We had a very good pathology course, both microscopic and gross, and we started physical diagnosis, elementary use of the stethoscope and things of that sort. I had a pharmacology course given by Otto Krayer, which was quite good. It was much better than pharmacology almost anywhere else in this country, because it was much more dynamic instead of the old *materia medica* kind of thing. We had heart-lung preparations and so forth, and a minimum amount of attention to how you make pills. That was very good and really in many ways a physiology course.

Krayer was a most unusual person. He had been in Feldberg's laboratory in Germany. The story that I was told and I assume is correct, is that when Feldberg was dismissed by the German government because he was Jewish, they offered Krayer the job—or Krayer was next in line. Krayer, who was not Jewish, left the country instead of taking the job. He was a most righteous person in everything he did. I got to know him a bit. He was either unmarried at the time or his wife hadn't come over yet, and he used to eat with me in the dining room at the medical school dormitory (Vanderbilt Hall).

Another interesting thing was that at Harvard Medical School in the second year you had two afternoons a week free for voluntary courses. I took one in physical chemistry. That department was in the medical school, and like the biochemistry department, it was a very high-level scientific department. It was unusual in that it had no teaching interactions, so most of the students didn't even know it was there. Because of my background at Harvard College, I knew about it. This was the department that John Edsall was in. Edwin J. Cohn was the chairman. There were a number of really superb physical chemists there. In addition to John Edsall and Cohn, there were John Ferry, J. L. [John Lawrence] Oncley, and several others. They were all superb people. They gave a course for this relatively small group of medical students, probably a half dozen at most who were interested. They went over all the new things that were being developed in protein chemistry, particularly the developments abroad. That was really very exciting. I'm sorry I didn't remember more of it.

At John Edsall's ninetieth birthday some speakers pointed out that, except in the hemoglobin area, much that was considered valid and important in those days in protein chemistry was wrong. [laughter] That's rather interesting, but that's the way science is. Even some of the very definitive theories that were presented are almost ridiculous from our point of view now. Nevertheless, the ideas and the approaches were important. The work of John Edsall is still classic. His book with Cohn is still a classic, and is still used as a reference book in the laboratory today.

BOHNING: What was the status of the knowledge of enzymes at that time?

TABOR: This was 1938-1939, and I think it would depend on what part of the world you were in. In none of the courses did we particularly go over enzymes. On the other hand, obviously, you had the whole group in Germany, Warburg and others, developing the field. Perhaps I'm showing my lack of recollection about what was covered, but I think in the physical chemical group the discussion was about proteins as proteins. I don't remember any discussion of enzymes or catalytic centers or anything of that sort. It would have been hard to do if you didn't have the know-how we have now about the structures.

As I mentioned the voluntary course was mainly concerned with the physical chemistry of proteins, particularly hemoglobin and other purified proteins. The medical school biochemistry course emphasized two areas:

One, physical chemistry at a relatively elementary level for medical students; i.e. the interactions of blood components, acid-base balance, etc.; and two, intermediary metabolism. The latter was largely classic biochemistry, mainly what compounds go to and what compounds get excreted and their function where known. (I should mention that this was the laboratory that [Otto] Folin had been head of, and he was very well known for his analytical methods.).

Talking about hemoglobin brings me back to Chemistry 15 at Harvard College and L. J. Henderson. There was one very picturesque classroom demonstration he had that I have referred to again and again in all kinds of connections. To show the interactions within blood, he brought in a wooden frame, with nails around it, and he had interlocking rubber bands. He plucked one and all the others moved or vibrated to show how when you change one component in the blood or in the red-cell system, you change all kinds of other things. You can't just change one without considering interactions. I've used that example again and again because, from what we know now in proteins, you can't change one amino acid without changing the whole secondary and tertiary structure. It's a concept that is fairly obvious, but often neglected.

I also took a very good bacteriology course that Hans Zinsser gave; he was a very picturesque lecturer. (I have already mentioned his book: *Rats, Lice, and History*) (2). John Enders and Hans Mueller taught a part of it, [Charles A.] Janeway was a section man. Again, this was before the development of the use of bacteria as a tool for studying metabolism, at least in this country. I think Mueller was just starting his toxin work. In general, the course presented a classical type of bacteriology, which, of course, was very important for medical students.

Apropos what I said earlier with regard to interactions in blood, at that time there was a different way of thinking in bacteriology from that in some other fields. In bacteriology the concept was that one thing leads directly to another. You get an infection and you get a disease. It's a different way of thinking than the concept that one change causes changes in all kinds of parameters. At that time most bacteriologists were not concerned with the complicated interactions of metabolic systems. Now, of course, bacteriology is an integral part of everything else we're doing, and I and so many biochemists have used the tools and concepts of bacteriology in many of our biochemical research projects. Bacteriology is in the forefront of everything now.

BOHNING: You are saying that bacteriology and physiology and all of these different branches were pretty separated. Is that true?

TABOR: That's right. They were very separated at the time. There were just these little glimpses of their getting together, but it was before they really got together. Of course, with a bias, you would say that it's biochemistry that brings them together.

I remember I also took a volunteer course in genetics, about which, I'm very vague. It was very unsatisfying because at that time so little was known. Maybe that's inaccurate because obviously there was a lot known, going back to [Gregor] Mendel. But in terms of thinking molecularly there was just nothing.

BOHNING: Wasn't genetics at that time more a study in statistics?

TABOR: Yes, very much so. I don't remember much about it, but mainly it was just that. How different genetics is now! In discussing the genetics and bacteriology courses, in retrospect, in some of the areas that we now know are important, more was known than was taught. For example, bacteriophage was around, and maybe we were told something about bacteriophages in our course, but I certainly do not remember it. In retrospect, this was an omission of an important topic. Again, however, these courses were in the medical school, and I think despite the critical approach that I am taking talking to you because of my present interests, I think Harvard did very well in balancing both the pragmatic and the basic scientific aspects of the material being taught. The fact is that in medical school most of the students are going to go out and practice medicine, so there was much more of an emphasis on that in these courses. I must add that I enjoyed these practical aspects too.

BOHNING: When did the clinical studies start?

TABOR: A little bit in the second half of the second year, but it was mostly in the third and fourth years when you go into the clinics. Before I come to that, though, in the summer after my first or second year I worked part of the time. When I say work, in those days you did not get paid for working. It was still the Depression period, and there were no funds for students; you just volunteered. I worked with Elmer Stotz, who had opened a laboratory over in Waltham. It's probably part of the MGH [Massachusetts General Hospital] now; I don't know if it was then. Later he was professor of biochemistry at Rochester. I did some work on cytochromes. It was nothing publishable, but it at least provided familiarity with some of the techniques. He was also a very stimulating person. He had also taught in Hastings' course.

To go back to your question, in the third and fourth year it was completely clinical. Frankly, I enjoyed that.

BOHNING: That must have been quite a change with all your interests in the more scientific side, such as taking physical chemistry. Did you see any immediate connection between the kind of science exposure you had and the clinical aspects?

TABOR: No. It would be somewhat strained to make that connection at that time. We would analyze urine, and that's a connection, but only in a very narrow sense. I don't want to underestimate the attractiveness of clinical medicine at this level as a student. It was very exciting. It's a problem. You have a patient, and it's a black box. You have to get the symptoms and figure out what's going on inside, what the mechanisms are.

This aspect of clinical medicine is an important one. It is a big challenge in much of internal medicine; you have a problem and how do you figure out what's going on. There's a lot of satisfaction in making a good diagnosis. There's a lot of satisfaction in curing people. There's no question about that.

I frequently point out that a clinical problem is often more difficult than the usual laboratory experiments. In a biochemical or physiological experiment in the laboratory, even though you can control some of your variables, you still have trouble figuring out what's going on. I still reluctantly write to authors, (and we'll come to this later), "You have a very interesting observation, but it still doesn't contribute enough to our understanding of the basic mechanisms involved."

BOHNING: You've solved the problem if you've made a good diagnosis.

TABOR: That's right. Even more than that, you solved the problem and there's a very personal aspect. You can't help but be pleased when you make somebody feel better. When you're interning, you're doing that, but the students do have a fair part in the diagnosis in the hospital in almost all schools. A bright, energetic student can pick up things that even the more experienced person doesn't. They spend more time. I don't want to underestimate my interest or the interest that one should have in clinical medicine.

Harvard was particularly good from this point of view because they had certain departments, like the Thorndike Laboratory at the Boston City Hospital, which were very high-grade scientific departments. This was true of most of the Harvard hospitals, but at that point, most medical schools in the country did not have this approach. I think they were much more practical schools. Now many medical schools have excellent scientifically oriented departments, partly because of grants. At that time, there were certain people who even encouraged this difference and said that the science should only be done at the university. Fortunately, there were certain schools, Columbia, Harvard, Yale, that pushed the scientific approach to medicine. I think that made a big difference. I think that's one reason why so many of my classmates went into academic work, although very few of them left clinical medicine completely. I don't have the figures handy, but I think over eighty percent of my class had academic appointments along with their clinical practice. There were quite a few deans and professors of medicine. I think about half of my class were in that category.

Just as an anecdote, and to show that I'm still pleased with the satisfaction you get with a good diagnosis, I tell this story. If someone asks me why I went into biochemistry and not into

clinical medicine, I tell them the following: When I was a third year medical student at Boston City Hospital, William [B.] Castle, was the senior attending physician. The interesting thing in medicine, parenthetically, is that you still remember many of your cases fifty years later. A man came in with a mass in his abdomen. As usual, a medical student takes a history and writes up a provisional diagnosis. I went to the library and, in my naivete, I made a very rare and esoteric diagnosis. In retrospect, I was justified. I said it could be a pseudo-cyst of the pancreas due to occlusion of the Foramen of Winslow. Ordinarily, you would think that the mass was a pancreatic cyst, but in this case it was actually due to the fact that the Foramen of Winslow, the exit from that part of the peritoneal cavity, was blocked.

The junior attending, when I presented this diagnosis, took me to the window. They still had outside electric and telephone wires near the Boston City Hospital, and he said, "See that bird over there? It might be a Roman eagle, but it's probably a sparrow." When they operated it turned out it was a Roman eagle and I was right. [laughter] Dr. Castle was obviously impressed by the diagnosis, and he took me aside and said, "Look, you've had it. You made one of the diagnoses of a lifetime!" So what I like to say now is that, "Well if I've made my diagnosis of a lifetime, there's no use going on in clinical medicine, so I went into biochemistry!" [laughter] Of course, this is not why I went into biochemistry.

[END OF TAPE, SIDE 4]

TABOR: As I said, none of this is relevant to what I was supposed to be talking about with you, but it is fun to talk about.

BOHNING: Oh, no. It is quite relevant.

TABOR: I can't say I don't miss the challenge of clinical medicine. I think clinical medicine would have been fun. On the other hand, I think one of the sad things about medical practice—and from what I gather from people I know, it's more so now than then—is that you can't imitate the excitement you get when you're a student and an intern. Later on, there are too many other complicating factors in medical practice, especially nowadays.

In the third and fourth years we had obstetrics (as well as other clinical courses). In historical terms, our arrangement in obstetrics in the third year was very interesting. We went out and lived in a settlement house in the community for two weeks. In my case, it was in the Roxbury-Dorchester area. We went out to the homes to deliver the babies. The mothers were usually people who had been checked in the clinic. The fact is, however, you'd never know what you would run into. Most of the time the student would go out alone. That was quite an experience, because you would go out in the middle of the night to some economically deprived area in Roxbury, Dorchester, or Allston. We might or might not have a public health nurse meet us, and we would deliver a baby. That was also quite an interesting experience in seeing the

communities, seeing people in the community and so forth. It was also rather dramatic, because that was the period when the German armies overran Belgium. I remember it particularly vividly.

As I mentioned earlier, I lived in New York near Columbia. Between my second and third year, I spent the summer attending rounds and looking at pathology specimens at Columbia. I think the rounds were mostly at the Neurological Institute, and these were very interesting. Between my third and fourth year I took a one-month course in medicine at Columbia on an exchange arrangement between Columbia and Harvard.

Harvard had a system where you could finish in January of your senior year, although formally you didn't get your degree until May. I combined the remaining six months plus an additional six months to work in Dr. Hastings' lab in biochemistry. In that period you were able to start your internship in January. The starting dates were staggered in those days. I started my internship in January of 1942. We're now talking about January of 1941 to January of 1942. Thus I was in Dr. Hastings' laboratory from January of 1941 until January of 1942

We entered the War [World War II] in December. Dr. Hastings was usually down in Washington two days a week, because he was on the Committee of Medical Research of the OSRD [Office of Scientific Research and Development]. My problem was a physical chemical problem, for which I had very little background, but this made it particularly interesting to me. It was the ionization constant of secondary magnesium phosphate. As you know, you have inorganic magnesium and phosphate as well as phosphorylated organic compounds in biological materials. Dr. Hastings was very interested in what the free ion concentrations were. The idea was to find the ionization constant of magnesium phosphate. In those days, compared to now, there were literally no extra funds for materials and support. I made a conductivity meter with the help of one of my classmates, Ivan Frantz, who was an amateur radio ham. We made an amplifier to use on a type K potentiometer that was in the lab, and I determined how much the conductivity decreases when you mix magnesium and phosphate because of the complexing, and calculated the dissociation constant. It was fun to learn a new area. I did a little bit of work with ATP [adenosine triphosphate] and phosphocreatine.

I wish I had done more work with ATP and related compounds because the binding was tighter with these compounds and they have considerable physiological significance. There were several reasons why I didn't. One was that the materials in pure form were hard to come by. The other was that, because there were multiple valences, it was much harder to write the equations, which goes back to what I was saying before about not having enough math. That was an interesting physical chemical study. I learned a certain amount of basic physical chemistry, and it was fun learning the Debye-Hückel equations and all that kind of thing. I didn't know much about this type of material, and still don't, but it was at least fun to get that background.

BOHNING: That resulted in your first publication (9).

TABOR: That's right. That was not published until 1943 and it was in the *JBC* [*The Journal of Biological Chemistry*]. The reprint is sort of interesting to look at because the pages were small, and there was quite a difference in size from the present size. Obviously, I was very pleased to have that publication. During that time, I was in a very nice laboratory in the same room with Ollie Lowry, who helped me quite a lot. I had no background, but he had both a Ph.D. and an M.D. and was extremely helpful. He had been in [K. V.] Linderstrom-Lang's lab. He was extremely good with glassblowing and other techniques, and was extremely helpful.

Dr. Hastings was quite stimulating and, as I mentioned before, interested in everyone. In the laboratory there were John Taylor; Jack Buchanan, who was a graduate student then; Bernie [Bernard J.] Jandorf; Chris [Christian B.] Anfinsen, who was also a graduate student; Birgit Vennesland; William Wallace and Otto Klemperer, who helped me with the polarographic equipment, and several other people. I think Dr. Fiske was still around, as well as Dr. Harry C. Trimble.

BOHNING: You had mentioned the question of support. I was going to ask how you supported yourself through medical school.

TABOR: As I said, we were not well off, but my family did support me completely.

BOHNING: That was really an amazing accomplishment in the Depression years.

TABOR: That's right. I certainly appreciated it. Especially in terms of taking time off, like the extra half-year in the biochemistry laboratory before my internship. I feel very lucky. To give you the economic and social background of the times, there weren't many jobs, even if one wanted to work. A number of my classmates did wait on tables and so forth, but it was very hard to get jobs at that time.

BOHNING: Did the entrance of the United States into the War in December of 1941 have any influence on how you ascertained your future career after your internship? Were you thinking of possible government service? Was there any change in your plans after the War started?

TABOR: I think to a certain extent it's hard to answer that, because things were out of one's personal control for the most part. The entrance into the War itself didn't *per se* have that much of an effect, because the draft had started earlier and perhaps it was obvious we would be getting into the War. I think one just assumed that after one's internship one would be in the military. There just wasn't any question. I don't think we necessarily realized how long the

War would last or how difficult it was going to be. I think it wasn't something one really thought much of, because it was just what you were going to do.

BOHNING: You started your internship in January of 1942?

TABOR: Yes. Fortunately the atmosphere at the New Haven Hospital was particularly good. There was a mixture of the clinical and a different kind of biochemical background. The chairman of medicine was Francis [Gilman] Blake. One of the full professors, a very fine person, was John [Punnett] Peters. He had written a classic two-volume book with [Donald D.] Van Slyke on quantitative clinical chemistry (10). He was a biochemical physician par excellence. He felt that unless you quantified things, you didn't know what you were doing. He set up a laboratory different from the hospital laboratory for very high-grade biochemical analysis, which was rather unusual at the time. He did a number of biochemical studies in his lab. Yet at the same time, when he was attending he was very concerned with the patient, whether the pillow was in the right position and so forth. He was a very fine person, both dedicated and intellectually honest. You may recall that later on he was the subject of a Supreme Court case, because on his last day on one of the NIH study sections, he was dismissed because of secret allegations over whether he was loyal. This decision was later reversed by the Supreme Court. He was the son of a minister and an extremely righteous individual who was very concerned with everybody's welfare, his patients, his students, and the interns. Peters represented a unique combination of an excellent biochemist and an excellent clinician.

While I interned, in the evenings I worked with Jim Hopper [Jr.] in Dr. Peters' department on a method for doing blood volumes using carbon monoxide, which was published in the *Journal of Clinical Investigations* (11). I enjoyed my internship very much for the reasons I mentioned before.

BOHNING: I have here that you were at Yale Medical School.

TABOR: The internship at the New Haven Hospital was an integral part of the Yale Medical School. You had a double appointment, but effectively it was just an internship. Yale was a little unusual at the time, and it may still be, in the sense that the classes were smaller and the students all wrote senior theses. Nonetheless, it didn't have quite the same end result. More of Yale graduates went into private practice than in the Harvard group. Still, it was a very high-grade group, and it was a lot of fun being there.

BOHNING: Whom did you interact with there?

TABOR: My fellow interns and resident staff, and when there was time, Dr. Peters and others in his laboratory. Also, I used to go over to the biochemistry seminars when I could get away. C. [Cyril] N. H. Long was the chairman there. Abe [Abraham] White was there, and several other people. Obviously, when you're interning, you don't have much time. However, I may have had a little more time than interns do nowadays at the big metropolitan hospitals. I did have time to go to the library and to look up material relevant to my cases. This was rather unusual for internships, and made the Yale appointment particularly nice. I don't think that I can say more from the biochemical point of view except for the work with Jim Hopper that I just mentioned. I recall that one kept very busy with the day-to-day care of the patients.

BOHNING: Were there any particular incidents during that time, clinically speaking?

TABOR: I remember a lot of the cases, but they are hardly of any relevance to anything we are discussing here.

BOHNING: When your internship was over, you entered the U.S. Public Health Service. How did that occur?

TABOR: I guess that is relevant background. I don't really remember what the exact story was, or I never knew. Bernard [D.] Davis, who was later the chairman of the bacteriology department at the Harvard Medical School, was a class ahead of me and was an old friend of mine. He was in the commissioned corps of the Public Health Service at NIH. He called me and said they were giving exams for the regular corps of the Public Health Service, of which the NIH was part. At that time, I had a double interest. One was in research, but the other was the broad area of preventive medicine. That appealed to me. I came down and took the exams.

Dr. Sanford [M.] Rosenthal, who was head of the pharmacology group at NIH, had shown the efficacy of saline in animals, especially mice, after a burn. This was very important potentially during the War, because of the question of the use of saline versus plasma in mass casualties. He had apparently asked the director if there was anyone who could work in that area. Dr. Hastings was on the Committee of Medical Research, and Dr. [Rolla] Dyer, who was the director of the NIH, was *ex-officio* representing the Public Health Service. I don't know the timing of it, but somewhere along the way the two things got together. I was applying for the Public Health Service, and also I had this background that would be particularly good for this project.

I applied for the Regular Corps. The Public Health Service was a very small Corps. When I came in, I was the five hundred thirty-fifth member of five hundred thirty-five members in the country. They only made two or three appointments a year, and fortunately I was appointed as a commissioned officer in the Corps. The appointment was for the Public Health Service; you had no commitment for a specific station. I was first assigned for five weeks to the

Marine Hospital in Boston, really just waiting for my Coast Guard cutter to arrive. Then I became a medical officer on one of the cutters. The Coast Guard cutters were manned by the Public Health Service. They were the convoy escort ships in the ocean. In peacetime, these cutters would normally go up to Alaska and do medical care and so forth. They had about two or three hundred men on them and one doctor. They were three hundred twenty-seven feet long. They had a sick bay and a pharmacist mate, and the PHS officer supplied all of the medical care for the people on the ship.

I was assigned to the Coast Guard Cutter Duane. I made one round trip to Scotland and two round trips to North Africa. They were about twenty-one days each time in each direction. I was on the cutter until September, when I was assigned to the NIH. I had no surgical training except at medical school. One morning the convoy commodore was transferred to my ship because of abdominal pain; he was brought on board by a breeches-buoy, because this was in the middle of the ocean with submarines. He came on board, and it looked like he had acute appendicitis. We were on a rolling ship. He asked me where I went to school, and I said Harvard. He said, "Oh well, then go ahead and operate, Doc." [laughter]

I decided to treat him conservatively. Of course, this was before antibiotics were available, except for some sulfonamides. Later the statistics proved that I was right, but everyone on the ship thought this was a terrible thing to do, not to operate. They had read about pharmacist mates in submarines doing operations. He was operated on successfully ten-days later when we reached port. Ten years later I found out that the convoy commodore was a retired commandant from Annapolis. I think I would have been even more scared if I had known that. Frankly, I was very ill at ease. For me to operate, never having done an appendix, on a rolling ship, would have been something! [laughter]

By the way, I do have one anecdote that I might mention to you from the New Haven period that really is of considerable interest, although my part in it was very trivial. When I was a junior intern, I gave the first dose of penicillin that was ever given in this country. It was very dramatic. As you know, there are books written on this (12), as well as a PBS documentary. The British had developed penicillin. They didn't have the technical, industrial capability, because of the War, to exploit it. They were trying to get American help to make the penicillin.

[END OF TAPE, SIDE 5]

TABOR: As I understand it, John [F.] Fulton, who was a well-known professor at Yale, had a lot of connections with the British group, and he arranged to get a test amount of penicillin. It wasn't sent over from Britain, but they arranged, presumably with Merck, to make a very small amount. The titer was extremely low then.

It was sent up to us in New Haven. I gave it to a woman who had a pelvic inflammatory disease and had been running a fever for months. It was absolutely miraculous! The second case was a man with a brain abscess, and he did very well. The third case was very sad. The

patient was a nice young fellow with bacterial endocarditis. His fever went down and he did beautifully. Then we ran out of the penicillin, and he died.

The rest of the story is from of an anecdotal point of view, and it is quite amusing and interesting. Ninety-five percent of the penicillin is excreted in urine. We saved the urine and sent it back to Merck to be re-extracted because it was so precious. Years later I found out the following connection. Gil [G. Gilbert] Ashwell, who is quite well known in biochemical circles and was head of the laboratory with which we had seminars for many years, ran into me in the supermarket. He had just come back from a celebration on the discovery of penicillin in England. It turned out that before he went to medical school he had been a technician at Merck. He had used the relatively new technique of lyophilization to prepare the penicillin that was sent up to New Haven for me to inject. When I sent the urine back, he re-extracted it. [laughter] We learned fifteen or twenty years later of this interaction. Anyway, it's quite an amusing story. It was a very dramatic thing. The amount of penicillin was absolutely trivial compared to what you can buy for probably ten cents now.

Returning to our previous discussions, I want to mention that even though there are people who are unique, most of us, although we hate to admit it, represent a type or reflect our environment. I guess whatever I'm saying holds for a lot of other people.

BOHNING: Well, that's true, but I've always been amazed at the interactions, at the networks that exist of people. You mentioned Kornberg. He was with you at City, and then you crossed paths with him again. It's not obvious in reading the literature that all of these connections exist.

TABOR: How long have you been doing these interviews?

BOHNING: I started doing this in 1985.

TABOR: I see. You've been doing this for quite a while. You had no personal historical training, except by doing this?

BOHNING: No, I have no direct training, although I've been doing work in the history of chemistry since about 1980.

TABOR: Of course, you go to meetings, and you interact with others. Do you know Stetten's [DeWitt Stetten, Jr.'s] book (13)?

BOHNING: Yes.

TABOR: At first I questioned how useful this book by non-historians—including my chapter (14)—would be to historians. The reason was that I had the feeling that a trained historian would have known the right questions to ask, and then would have been able to make something out of it. I thought that most of the chapters in the book were too specialized in terms of what the authors themselves were doing. One of our friends, however, Howard Schachman, made the point that there's so little background available, that even if a book or chapters are not expert presentations from a historian's point of view, they would still be good sources for historians from which to start. Incidentally, Dr. Schachman would be a good person for you to talk to for background on biochemistry in this area.

BOHNING: Oh, yes. Every bit of information can be very valuable.

TABOR: These autobiographies, except for your editing and so forth, are not used for anything other than source material for other people? As such, I wonder how valuable they will be?

BOHNING: You will have a final release form in which you can indicate who has access to the document. You have the control over the document. The final document will be indexed, which will facilitate people using it, and we're in the process of preparing a catalog.

TABOR: Frankly from that point of view, my opinions have changed on this question. My original idea was, why don't they put these on CD-ROM, and then people all over could get them. My objection to that now is that I think if you want source material, that kind of distribution would repress people in what they say. I think somewhere in between, probably, would be what I would vote for. In other words, anyone who really wants to see it should be able to see it. On the other hand, I would hate to think that it could be put out, say, as a supplement to the *JBC* as has been suggested.

BOHNING: One of the categories you will see on that release form is that anyone can look at it, but your permission is needed to quote or cite from it, that's one possibility. Or it may be that no one can look at it unless you agree to it, which is another possibility.

TABOR: My own feeling is that I would have no objection to people who really want to look at it, but I would hate to have it mailed to everybody. As a compliment to you, the little bit we saw of Harland [G.] Wood's biography (15) was so interesting that it would be nice to have it really freely available to everybody.

BOHNING: Did you see the Harland Wood transcript?

TABOR: It was only passed around at the council meeting. We all read certain parts that were particularly interesting to us. It's a compliment to you. On the other hand, I think it also might be hard to ruin what he did. [laughter]

BOHNING: Well, yes. With so many of the people I've interviewed I have my notes and I have questions, but it's the person who is responding that's making the document. I just try to keep things moving in a particular way.

TABOR: I think the relatively prosaic background that I've had and that we've been talking about this morning is typical, perhaps more prosaic than that of many others whom you've interviewed.

BOHNING: It is and it isn't. I think that, as I said earlier, you have a very unique path you've traveled. Even if I were to agree with you that it is typical, there aren't many of those that are going to be preserved in some fashion.

TABOR: It's rather interesting how different people react. Are you familiar with the yearbooks that places like Harvard put out (16)?

BOHNING: No, but I saw one on the shelf up there.

TABOR: My write-up is typical of what you would expect of me versus other people. Let's see if there's anyone in here in science.

Incidentally, I see an item on the shelf that is of interest even though it is not at all relevant to what we're talking about. This is the brief concerning a case decided by the Supreme Court in 1970 (17). We're one of the et. al.s in that case. NIH (and the quarters in which we live) is a federal reservation, and when our son went to register, the election officials wouldn't let him register because they said he wasn't in the State of Maryland. [laughter] We sort of precipitated further action by saying: "We've been voting, so how come he can't?" So they canceled all our voter registrations. Tillye Coleman (who also lives on the NIH grounds) is an M.D. physiotherapist who is in a wheelchair having been shot by a patient years ago at NYU. She was used to getting things done by phone, and she called up one of the prominent law firms in town who accepted the case. The case went to the Supreme Court (398 U.S.419, 90 S. Ct. 1752), who said that things are different now than when the Maryland Constitution was adopted

in 1865 or thereabouts. The decision indicated that voting rights cannot be denied for relatively trivial reasons.

BOHNING: That's amazing!

TABOR: Coming back to the periodic "Reunion books" published by the Harvard classes, I think that these would be a useful source for some background information that you might want on any of your interviewees or others who have graduated from Harvard College. Some of the people really write quite lengthy evaluations of what they've done in these books (16).

BOHNING: Yes. I wasn't aware of this. How often do they put these out?

TABOR: Every big reunion year.

BOHNING: Would it be possible for you to send me a photocopy of this?

TABOR: Oh, sure. That's the twenty-fifth. I'd be interested to see how I evaluate myself. Here's the fiftieth. The other people here really do very well in summarizing their work and thoughts. Apropos of what I was saying before, I find it very difficult to write anything frank and open in something like this.

BOHNING: I'm glad to know about that for future reference.

TABOR: The big ones are the twenty-fifth and fiftieth, but let me see, I have a fortieth, a thirty-fifth, a thirtieth.

BOHNING: It must be every five years.

TABOR: Something like that. We have one from the Medical School, too, but people here are much less talkative, or maybe the editors asked for less. The fifty-fifth gets smaller, I'm afraid. By that time most people are retired. There is someone in here (from my class) whom you know, whom I did not know at college. His name is certainly known in the history of sciences, I. Bernard Cohen.

BOHNING: Oh, yes.

TABOR: How about Saul [G.] Cohen? You don't know him, do you?

BOHNING: The chemist?

TABOR: Yes. He's in here; he's a classmate of mine.

BOHNING: Oh, sure. He was at Brandeis.

TABOR: He's actually an old friend of ours.

BOHNING: I don't think I've met him, but I know the name very well.

TABOR: Let's see, here is the material written by I. Bernard Cohen. He probably wrote more. Yes, this ought to be interesting. He wrote in the twenty-fifth, at least. He certainly made the History of Science a big field.

BOHNING: He talks about his professional things, and then he says that he "remains a photography addict, an amateur on the accordion, and a novice at figure skating."

TABOR: Isn't that amusing?

BOHNING: That's interesting, and I certainly am glad to know these volumes exist.

TABOR: I think these will probably be very useful to you.

BOHNING: Yes, very useful.

TABOR: I hope we haven't spent so much time in the earlier period that we neglect the part we are supposed to be talking about.

BOHNING: I think the earlier part is equally as important. We try to capture that early period in a person's life.

TABOR: The one thing I don't think I really covered adequately because it's so far back and I don't know enough of the background, is the influence of one's particular society and environment, the times and the economics of the times. That is quite unique, and I think it is interesting, not from a biochemistry point of view but from a social point of view. I think to get at that would require a lot of thinking and more background, and, perhaps, very forced and penetrating questions that I wouldn't even know how to either ask or answer at this point. One would really want to know the whole historic background of the times.

BOHNING: That was partly one of the reasons I asked whether your parents were immigrants or not, because sometimes first generation children have family pressures. I'm not saying just support, but sometimes pressures to succeed.

TABOR: Whether one is first generation or second generation is not necessarily as critical as whether you're in a group that is first generation or second generation. The community I was in reflected that. It really did not make much difference whether my father was three years old when he came to this country (as he was) or whether he was born here, or whether he was older when he immigrated. A whole variety of other factors also affect you; i.e. whether there is a Depression (as there was after 1929) or whether there was a long period of prosperity (as there was during the 1920s). How you respond to these factors is important. I should also mention that still other factors were significant in that period, such as one's religious and ethnic background. In particular being Jewish was certainly important, particularly with regard to the ethical values and the intellectual ethic of the community.

You talked about Konrad [E.] Bloch and this period. Obviously, the whole scientific community, as well as everyone else, was terribly affected by all the stories of the people from abroad and people coming over.

I am very concerned that what I have been discussing has the same disadvantage I was saying about the book on NIH, that it's really quite superficial from a historian's point of view.

[END OF TAPE, SIDE 6]

BOHNING: When I applied for my passport, I had to get a birth certificate from the state Bureau of Vital Statistics because my hospital birth certificate wasn't good enough any more. On the birth certificate form that came from the state, where it says my father's occupation, it says, "unemployed." I was born in 1934, and my father hadn't worked—I found out later—for

several years. They survived because he had an older brother who had a farm. It was very, very difficult, but growing up, I was never aware of that. Everybody else around me was virtually in the same position. When the War started, I was in first grade or second grade. To kids, rationing is rationing, and we didn't think anything of it.

TABOR: Some of the questions you asked me I think I really answered in that vein. Parents tend to protect children from this sort of thing. When I say that they supported me and paid the tuition, I wouldn't be surprised if this might have been very difficult in ways that I didn't know. I assume so, because, after all, everybody was affected very much by the Depression.

Then there's another aspect. It's true even nowadays, especially in the kinds of fields that you're in and I'm in. You do your job, and you do it conscientiously while all these other things go on. You're moved by them and wish you could do something about them, but you just go ahead. If someone asked you, what were you doing the weekend that [William Jefferson] Clinton was in Vancouver? Or, how could you work while everything is going on in Bosnia? I think that's really it. I was talking about the period when I was doing obstetrics. When you're in somebody's home delivering a baby, Germans might be invading Belgium, but you still go ahead and do what you are doing despite the worrisome news reports.

BOHNING: There is a famous picture from the London Blitz that shows a milkman delivering milk bottles over the rubble.

TABOR: That's right. One of the rationalizations I have for not doing a better job for you is that you can't tell what motivates anybody. After all, you can spend three years in a psychoanalytical program, and you still don't <u>really</u> know what motivates people. That's one of the more intriguing things about people.

BOHNING: I have noticed in talking to a number of people that the answer to the question I asked you earlier about development of the interest in science is in most cases <u>not</u> a light bulb going on all of a sudden like a revelation signaling that this is what you're going to do.

TABOR: That's right, yes. I think one of the problems you have—and it is unavoidable—is that people rationalize things, even in situations like this kind of questioning. I think what really motivates one is so unclear; you don't know what motivates one right now, let alone fifty years ago. I think one of the problems you have now which is very important for the biochemical community, is why there are less people going into science. The picture I was portraying for myself and for Arthur Kornberg was unique for the time. You wouldn't think it would be, but you don't have the same situation now. Very few of the M.D.s are going into basic science. That's a question that's really quite critical.

BOHNING: There was a point when the combined M.D.-Ph.D. program was very popular.

TABOR: That program is still very good. I don't know where the graduates of this program are going. I'm sure they're staying in some sort of science, but how applied that is and how clinical it is, I don't know.

There are all kinds of accidental things, too, which I think are relevant. When I think of the group here, I was probably more interested in not going into the practice of medicine than most of my associates. Being here, almost by definition, made a difference in directing us into basic science. For example, despite the interest I've mentioned that I have had in basic science, if instead of being assigned here I had been assigned to some clinical investigative laboratory or to some clinical program, it is possible that I might have stayed in these clinical areas.

BOHNING: Your coming here was not really your choice? I guess we haven't really gotten into that.

TABOR: It was a choice that I applied for the Public Health Service, but applying somewhere within the PHS was not a choice. This gets into a more involved issue if you want to go into it later.

One of the very fortunate things for the NIH was that they were able to attract excellent, well-trained people. A lot of those people, because of the scientific atmosphere at NIH, reversed their career plans, and spent their lives in science. Of course, you can argue perfectly well that if they were excellent people, like Kornberg, they would have done well no matter what they went into.

BOHNING: When you joined the Public Health Service, was there a commitment for a certain period of time?

TABOR: That's a difficult question, The Public Health Service is a Commissioned Corps, and I was in the Corps until about ten years ago. During the War it was like any other part of the military. You had no choice in assignments at that time. It was a small Corps, containing about five hundred thirty-five officers.

There probably weren't more than twenty commissioned officers at NIH. There was a sort of *esprit de corps,* and I think there was a feeling on both sides that you were there for life. If you did leave, there was a real feeling that you let people down. This has gone by the board completely, but that was at least the atmosphere at the time.

The NIH was an interesting place for a young scientist. The NIH administration took the approach that for the most part NIH did not sponsor big groups. An incoming commissioned officer was assigned to a scientific area, and almost by definition became an expert in the area. For example, if NIH assigned someone to work in leprosy or tuberculosis research, that person automatically became an expert and was allowed to proceed even without further training. Kornberg was unique in realizing the importance of additional outside training. After he had spent several years doing beautiful work in nutrition, he decided that he wanted to learn some good basic biochemistry. As I have just mentioned, the usual NIH policy was that you learned an area by working in it. Most of the people at NIH at the time, including the well-known names, were self-taught. Kornberg's viewpoint, which was of course correct, was that you go elsewhere and receive training with somebody like Carl Cori or Severo Ochoa and then come back to NIH. He was probably the first person at NIH to do this, and it is a credit to NIH that the administration permitted this assignment.

The NIH was and is an unusual operation, mostly in the positive sense, but with certain complications. The NIH, because of its unique status as a government laboratory, has many advantages and many disadvantages compared to the universities. I think the two types of institutions—NIH and universities—are much closer now because the universities have developed complications of their own. It is remarkable and dramatic how well the NIH has done and how well it has developed within the kind of restrictions imposed by being a federal institution.

I might mention again, in discussing these early days at NIH, how, without quite knowing how, one is very affected by one's local environment; i.e. by such factors as the composition of one's local associates and by the prejudices and economics of the period. As I mentioned this morning, one does reflect, without realizing it at the time, what the community does and favors. You think that you're independent, but there's the whole question of how much free will you really have and how much you think you have. We like to believe we do things in a rational way, but we do reflect our society.

BOHNING: Did you see any of that in your schooling, or was it outside of the school, in the broader community?

TABOR: Well, even though I talk about these factors now, I'm not saying that at that time I was aware of their effect. As I have said, at the time you just did things the way you thought you wanted to do them. If you're asking me what motivated me to do something, there is no question that there were other influences that I wasn't aware of then, that perhaps were from the community. As I said earlier, why don't more people go into science now? Why does it not have the excitement now for young people? Why do people want to go into business now? I think in my community going into business would have been considered something you wouldn't talk about openly.

BOHNING: At that time, business was what you did when you couldn't do anything else.

TABOR: Yes, that's exactly the point. I say now a little facetiously that you wouldn't be seen carrying a *Wall Street Journal* then. [laughter] I don't know how it is now at Harvard, but, certainly, it would have been unthinkable in my group then.

Now let's get back to the NIH. It was and is a unique institution, and it is remarkable how well it has been organized and run. I have repeatedly said that I cannot understand exactly why the people who ran it did so well. It was a small institution with a long history. The Public Health Service was started in 1798, mainly to be the medical arm that would take care of the infant Merchant Marine. It was like the Coast Guard; it was established for the same purpose, to support the new Merchant Marine. Marine hospitals were set up to care for sailors in the Merchant Marine. Then the NIH started around 1880 as the small Hygienic Laboratory in a small room in the Staten Island Marine Hospital.

BOHNING: Was it called NIH at that time?

TABOR: It was called the Hygienic Laboratory. You may or may not be familiar with some of the books on it. Here are two books: *A Profile of the United States Public Health Service, 1798-1948* by Bess Furman (18). The other one is: *The United States Public Health Service, 1798-1950* by Ralph C. Williams (19). I think that the latter one is a little more comprehensive. I want to emphasize that, in terms of the general history of science, that the NIH has been extremely important in the explosive development of biomedical sciences in this country and around the world. There was a period after the War when the NIH was the major support for science in many foreign countries in addition to the U.S. Very little has been written on the history and importance of this support of science abroad.

Interestingly there is a small booklet that was privately published by a lawyer in Washington (20). He was a member of the class of 1936 at Harvard College, and who, for his undergraduate college thesis, wrote a history of the Hygienic Laboratory. He probably wrote it in the sociology or government department.

It was a rather unique operation for the government to have a government-sponsored laboratory doing basic research. The Hygienic Laboratory was small and quiet, and until about 1940 it was located in downtown Washington, near the present location of the Kennedy Center. Along the way—sometime in the 1930s—the name was changed to the National Institutes of Health (NIH). (See appendix note i)

The Hygienic Laboratory (NIH) was run by a very fine group of people, who were commissioned officers in the PHS, and most of them had been trained in southern universities. A number were from the University of Virginia. In general, they were relatively more easygoing than the Harvard type of MD with whom I had been associated. Many of them had

joined the Public Health Service during the prosperity days of the 1920s when fewer people went into government service. They stayed in the government during the Depression, when, obviously, it was a very good place to be. It was a stable group, and on the whole they published very good work.

The scientists at NIH were a small group and they did not have much association with the universities and university staffs. Most university people didn't know of the Hygienic Laboratory or NIH. At the end of the War this group at PHS and at NIH was responsible for the expansion of NIH and for the development of the extramural program that became so important to American universities and science. Somehow they had the imagination to do that. Later I will discuss the importance of Baird Hastings in this development.

Before discussing Dr. Hastings' role I might mention another important factor in the development. The Hygienic Laboratory, as I mentioned, was downtown in Washington. At that time the site at which we are now located [Bethesda, Maryland] was an estate owned by Luke Wilson and his wife. He was a Chicago businessman; I was told that he was associated with the BVD manufacturing company. Mrs. Wilson was a member of either the Woodward or Lothrup families, who owned a large department store in Washington. I assume that they were rather wealthy. There are two stories that I have heard as to why they gave the land to NIH. The second story was told to me by his son, and is probably the correct one. On the other hand it is likely that both incidents contributed to their gift.

In 1933 the Wilsons watched the "Bonus Army" march down Rockville Pike to Washington. This is the episode that first made Douglas MacArthur famous. The Wilsons were so impressed by the poverty of these veterans that they decided to do something with their money. They were friendly with Franklin D. Roosevelt and Harold Ickes, and I think, mainly because of Ickes, they gave the land to the government for the expansion of the Hygienic Laboratory.

The explanation that the Wilson's son told me is that the senior Luke Wilson was interested in international cooperation, and that he realized that at that time such cooperation was most promising in the scientific community. To sponsor such cooperation he and his wife decided to give their estate to NIH for expansion of the laboratory. He certainly was correct, and today NIH is known throughout the world and has many scientists from all over the world visiting and working in its laboratories.

The move to Bethesda permitted the physical aspects of the expansion. It is important to note that despite the expansion the NIH continued to be set up in a remarkably good way, so that the investigators were largely allowed to do what they wanted to. It was not set up in the Geheimrat type of system that one might have expected in a government agency. I think that this approach has continued for the most part, and accounts for much of the excellence at NIH.

Even though I am very biased towards the intramural program, I think that the big contribution of NIH has been the system of extramural grants. Baird Hastings told me the story of how the NIH became involved in this program, and I am sure that the story is correct. Dr.

Hastings was chairman of the Department of Biological Chemistry of the Harvard Medical School, and my preceptor for my first paper in the *Journal of Biological Chemistry*. He was on the Committee for Medical Research during the War; the Committee of Medical Research was part of the Office of Scientific and Research Development. The Surgeon General for the Army and the Surgeon General for the Navy were *ex-officio* members, as was Dr. Rollo Dyer, who was the director of NIH and represented the Public Health Service on the Committee for Medical Research. At the end of the War the Committee distributed all their grants and contracts to the various agencies that wanted them. Dr. Hastings was very friendly with Dr. Dyer, and he urged Dr. Dyer, whenever the basic grants came up, to take them for the NIH. That was how the NIH became involved in an extramural program. (See appendix note ii)

However, the aspect that I think is really remarkable for a government agency, and I marvel at why this was the case, is that the NIH leaders set up the program as a grants program and not as a contract program. That's a very intriguing concept, and it has been very important. It would be perfectly obvious for a government agency to set up a contract system. In a contract you state that you want something done, and contract with someone to carry out this specific project. However, NIH set the program up as a grant program, which means that, even though you said what you planned to do, and were given the grant to do the specific project, there are no strings attached. Thus, if you find something interesting along the way, you are free to use the funds to study these new findings. It has been very exciting to watch this important aspect of NIH operations, even though, except for participation on a fellowship committee and a study section, I have not been directly involved in the extramural operations.

Another important and unusual policy was the decision to give the grant to the individual investigator rather than to the department chairman or the research director. This policy removed the distribution and use of the grant from local politics, et cetera and permitted the investigator, rather than the department chairman, to make the decisions concerning the direction of the research. Many of us feel that this policy is the reason that American science has flourished so much under the NIH grants system. (I should add a note of explanation: the grant is technically to the university, but all of the decisions on its use are in the hands of the investigator.)

Returning to a discussion of intramural NIH, as I have already mentioned, the intramural program was small compared to what it is now. It had, I would say, probably one hundred professional people, of whom twenty or thirty were probably members of the Commissioned Corps. At that time the members of the Commissioned Corps were all M.D.s.

BOHNING: When you talk about a Commissioned Corps, and it's just my ignorance in not understanding that part of the government arrangement, you were really like a military officer?

TABOR: That's essentially right.

BOHNING: Is there a rank involved?

TABOR: Exactly. It's very hard to explain, because it's very anachronistic. For the historic reasons I mentioned, the Public Health Service had a Commissioned Corps, which is exactly parallel to the Army and Navy, with exactly the same equivalent ranks. The ranks were named differently, but they were equivalent. For example, if you have two bands on the arm of your uniform, that would be equivalent to Lieutenant (Senior Grade) in the Navy (or Captain in the Army), but in the Public Health Service the rank was called "Passed Assistant Surgeon".

BOHNING: I was going to ask about that, because on your third paper (21) you are listed that way.

TABOR: It's a rather amusing designation. It means that you passed the grade of assistant surgeon by one grade. Four stripes are equivalent to Captain in the Navy (or Colonel in the Army), and this rank is called Medical Director in the Public Health Service. Then there is Assistant Surgeon General and Surgeon General. Even though this description is anachronistic, this aspect of the Public Health Service is somewhat relevant to the whole recruitment process over the years. It is a quasi-military organization in the sense, that, except during war when it is fully military, it doesn't come under the uniform military code. But everything else is very parallel. You have access to all of the U.S. Navy and Army hospital facilities, PX and commissaries, and the pay is exactly the same. All the legislation on pay and retirement is parallel with the Army and Navy. This has both positive and negative aspects.

This personnel system has been very important in recruitment. We like to believe, and I think that it is true, that the NIH has done well because it has attracted very good people. But the fact remains that there were periods when the people had the choice of either coming into the Public Health Service and the NIH or going into the Army or Navy without necessarily knowing where they would be assigned. This made the NIH appointments extremely attractive to many doctors—especially during the Korean and Vietnam Wars—many of whom had recently graduated from the best medical schools and had very good academic backgrounds. I say the Korean and Vietnam Wars, but this was also true during the second World War, except then the numbers were so much smaller that there was much less of an impact. Nevertheless, people like myself, Arthur Kornberg, Leon Heppel and several others came to NIH at that time. In the earlier period only M.D.s were commissioned, but later Ph.D.s were also commissioned and a number of Ph.Ds joined the NIH as PHS Commissioned Officers.

It was a somewhat awkward personnel situation, in the sense that there were two parallel personnel services. One was the Commissioned Corps, which at that time was in charge. This is no longer the case. The other was the Civil Service, and the two services had different salary scales. Parenthetically, this did lead to certain striking inequities. For example, M.D.s with no training would be assigned to work with Ph.D.s who were very good and who taught them everything, but whose salary was less because of the different personnel system. Nevertheless,

the Commissioned Officer system did attract very good people, and that was particularly important when the Clinical Center was opened. A very large number of people who are now professors of medicine in various parts of the country came to the NIH for two, three, or four years because of the Commissioned Corps arrangement.

Although the Commissioned Officer system was in general very beneficial for NIH, over the years there have been some problems with integrating the rules that the Commissioned Corps have had for the entire Public Health Service with the special needs of a research laboratory. For the most part, the directors of NIH have been strong enough to get changes made in these rules, and the Commissioned Corps administration has usually been cooperative enough to make the appropriate adjustments. One of the most important adjustments was that people are assigned to the NIH and stay here, whereas in the rest of the Corps they rotate through various Public Health Service activities.

Again, it is important to note that with this dual personnel system, NIH was able to use either system to attract all these bright young M.D.s and Ph.D.s.

BOHNING: I'm assuming that a lot of this happened after the War was over. During the War, was it relatively constant, or was it developing and growing rapidly at that time?

TABOR: They moved here from downtown in 1940.

BOHNING: Just before the War started.

TABOR: There was a little expansion at that time. Then there was a limited expansion during the War. For example, I was assigned to work on the role of electrolytes in the treatment of burns and shock with Sanford Rosenthal. I assume that represented a small expansion of that section. My assignment to work with Dr. Rosenthal on this project was based on my background with Dr. Hastings and Dr. Peters and the desire of the NIH to support this work. In general, however, new commissioned officers were allowed to go around and choose the department in which they wanted to work.

When I came to NIH, I had come off the Coast Guard ship, immediately after a transatlantic trip with a convoy, and I was very pleased to get this assignment. The procedure was for the new officers assigned to a station such as NIH to report to the commanding officer of the station, and this was the director of NIH. This is hard to believe now! [laughter] I went to Dr. Dyer's office, and told him that I was here. He wondered where I was staying that night. I hadn't made any arrangements, so he gave me the keys to a small cottage called, "Top Cottage" that the Wilsons had used as a guest cottage before they gave the land to NIH, and I stayed there until I could find a room. This shows how small and personalized the operation

was at the time, and how interested people like the director were in the young officers and in the details on what was going on at NIH.

BOHNING: Was there housing for personnel on site?

TABOR: There was the housing that you see here, but even then this was not enough housing for all of the Commissioned Officers at NIH. There are six duplexes; i.e. for just twelve families. In addition there is the one that used to be for the director of NIH and one for the associate director of NIH. Now the assistant secretary of HHS [Department of Health and Human Services] lives in one of these houses. Having housing was based on a historic anachronism when they built NIH. Technically, NIH was considered as only one station in the Public Health Service; like a Marine Hospital, and the other stations had housing. I think the plans for the houses were taken directly from those used for one of the Marine Hospitals.

I did not move onto the grounds until 1949. I came to NIH in September of 1943. There are two incidents that I might mention to indicate that, despite the fact that so much worked well in interactions between NIH and the Commissioned Corps, at times you could see the complications. Fortunately, due to the strength of the NIH directors, these complications did not usually interfere with the work in the laboratory.

One episode occurred six to eight months after I arrived, when the director of NIH received a request, probably from the Army, for me to be transferred on an assignment to evaluate some aspects of the scientific developments in Japan (i.e. to be accomplished immediately after the War ended). I was neither capable for that assignment, nor did I want to go, since I had just started my work on burns and shock with Sanford Rosenthal. Apparently Stanley Bennett, who had been one of my instructors when I was at the Harvard Medical School, was in this program in the Army and had asked that I be assigned to his group, thinking that I would be good for this project. Fortunately, the NIH director decided that I didn't have to go, although that would have been an interesting assignment.

The second incident turned out to be very interesting, but was again an indication of the occasional complications in the interactions of NIH and the PHS. At the end of 1945 at Christmas, I was the only unmarried Public Health Service officer in the Washington area. Parenthetically, this is an indication of how small the Public Health Service and the NIH was. The U.S. Government needed a ship to return to Russia the members of the Russian Purchasing Commission who had been in Washington during the War, and Russian military personnel who had been in the U.S. learning naval techniques to enter the War against Japan. Since the War in Japan had fortunately just ended, there was no need for the U.S. government to transfer to the Russians the military ships that they had planned to give to the Russians. Therefore the U.S. arranged to have an American transport ship transport the Russians back to Russia. Even though the Russians had their own doctors, since the transport ship was an American vessel, it was necessary to have an American doctor on board. I was assigned to this ship for this purpose. We went to Odessa, Constanza, and Burgas; the trip was, of course, interesting.

Although the duration of the trip was only two months, this temporary transfer from my laboratory work at NIH to another assignment by the Public Health Service does indicate how tenuous the interaction sometimes was between the Corps and the NIH.

Those are the only two incidents, and in general I think the interactions with the Public Health Service in that period were very good for the NIH. Gradually, more of the top people at NIH became Civil Service employees and more people were hired in the Civil Service, so eventually there was much less difference between the two personnel systems.

BOHNING: You briefly mentioned the Journal Club before, and I wanted to come back to it. But before we do that, there were a number of changes of laboratory names and organizational structure. Was it originally the Department of Physiology that you were first assigned to?

TABOR: I think my answer to that is that none of the name changes were important. These were only semantic changes. For the most part, until we recently moved from one building to the other, I was in the same room on the same floor working with the same people all along, except that for various administrative reasons the names changed. For example, when I came, there was one "National Institute of Health". Later we developed all the separate institutes.

BOHNING: It was originally Experimental Biology.

TABOR: To show how little significance that has, I would have to look up the actual names.

Let's return to a discussion of my assignment to work with Dr. Rosenthal.

Dr. Rosenthal was a pharmacologist who had a very real interest in biochemistry. He was a very unusual person and a superb scientist. He was very interested in science, but not interested in his own personal recognition. He was born in the South and had an undergraduate degree and an M.D. from Vanderbilt, probably in the early 1920s. He interned at the Boston City Hospital and worked at Johns Hopkins with John Abel, who was a famous pharmacologist of the day, and who parenthetically was important in the founding of the *Journal of Biological Chemistry*. During that period, Dr. Rosenthal developed the bromosulfalein test, which was widely used over the years as a test for liver function.

In 1928 or thereabouts he came to the Hygienic Laboratory. He considered himself a pharmacologist and was a member of the American Society for Pharmacology and Experimental Therapeutics. In the 1930s he had carried out a large amount of excellent work on sulfhydryl compounds and on sulfhydryl reactions. Dr. Rosenthal had also developed one of the first treatments for mercury poisoning using formaldehyde sulfoxalate. He showed that this compound reduces the mercury ion to metallic mercury making it much less toxic. He also did

some of the earliest work on sulfanilamide related compounds with Dr. Hugo Bauer, who was an organic chemist at NIH.

Dr. Bauer was an excellent organic chemist who had received his Ph.D. with Dr. Wieland, and had taken courses with [Wilhelm Conrad] Roentgen and von Bayer. He worked with Paul Ehrlich from about 1906 to 1914. Because he was Jewish he had to leave Germany in 1935, and came to the Hygienic Laboratory. He worked in our laboratory until the age of eighty-three. He taught me most of the organic chemistry that I know, and was of particular help in teaching me preparative techniques.

I came to work with Dr. Rosenthal in 1943. He had recently carried out some rather revolutionary experiments in mice showing that that saline was very important in the treatment of burns. His conclusions contrasted with the standard view that burns and shock should be treated with plasma, and that saline was deleterious.

He and I then studied the electrolyte changes in burns and shock. During that period we were also studying the treatment of hemorrhage, and we developed some nice techniques for studying hemorrhagic shock in mice (21). One of his most important contributions, and he had many important contributions, was that, compared with most of the published work in burns and shock—which were based on one dog or two rabbits or a physician's observations on a few patients—he felt, as a pharmacologist, that one could only make meaningful conclusions with controlled studies on an adequate number of animals. His approach was to use large numbers of animals like mice to get a statistically significant result. We all know now that he was correct, but his approach was very controversial at the time. We developed methods for burns and hemorrhage and studied the electrolyte changes. This was extremely important as a war project, particularly since plasma would not be available in any mass disaster, and could not be administered on a large scale.

As I said, the Institute was very small and there were only a few younger people at NIH at the time. Consequently the younger investigators got to know each other very well. In the evenings, I did some experiments with Arthur Kornberg, who was working on folic acid with [William H.] Sebrell and [Floyd S.] Daft. We studied the effect of using bleeding to put a strain on the animal's red cell system to test the importance of folic acid in red-cell regeneration (22). Between that work and our personal friendship, we started having lunch together. After a while we decided—and this perhaps indicates our personalities and the times—why should we just sit and relax at lunch or argue about political subjects? Why don't we talk science?

We started to do that, and then we realized that you had to be a little more organized because, otherwise, more than one person would bring something to discuss. We arranged finally that we would go over some papers in rotation. We started the seminar group just before Arthur went to Cori and Ochoa and then continued at full blast when he come back.

In addition to myself, the seminar group consisted of Arthur Kornberg, Bernie [Bernard L.] Horecker, and Leon Heppel. Later Kornberg, Horecker, and Heppel all had outstanding scientific careers. Several years later Jay [J. Edwin] Seegmiller came to work with Bernie Horecker, and joined our seminar group.

We went through a lot of the classic papers, which had not been really gone over well (or at all) in most of the biochemistry courses. We went through all the papers of Warburg. In those days, you knew how to read German. Reviewing these older papers in detail was very instructive and exciting for all of us.

As I mentioned earlier, Arthur Kornberg had had his M.D. training at the University of Rochester, and his biochemistry training with Ochoa and Cori. Leon Heppel received his M.D. at Rochester and his Ph.D. before that at the University of California in Berkeley. Horecker was a real biochemist, having received his Ph.D. at the University of Chicago; his Ph. D. thesis was in enzymology on the "New Yellow Enzyme" with Haas and Hogness. I had no extensive biochemistry training, except for the short period with Hastings, and therefore these discussions were extremely valuable to me. Kornberg, Horecker, and Heppel were all extremely good, enthusiastic, smart people, and we went over these papers in a very thorough way with a lot of give-and-take discussions. We had superb interactions, and continually contributed ideas and discussion to each other.

It is rather surprising now, but most people were amazed that we would have a luncheon seminar. That was considered so unorthodox, because people worried that we would get ulcers and so forth. [laughter] As far as I know, none of us did. [laughter] This was the beginning of the seminar program that still continues in our laboratory. We were a little more intense then; we had seminars every day, except Christmas and New Year's. Now, I'm afraid, we only have it three times a week.

BOHNING: There's a story that may be apocryphal, about someone who didn't come on Christmas Day. Was it Kornberg who said he was very, very bright, but he didn't know if he was dedicated?

TABOR: It wasn't Christmas Day. I think we didn't meet on Christmas Day. The story probably applied to some other holiday, and it could well have been the case. [laughter]

I thought you were going to mention another story. At that time there was very little classical music to be heard in Washington. The Library of Congress had some superb chamber music. In fact it was the only real chamber music in the area. The Budapest String Quartet played in residence, and the tickets were only twenty-five cents. You had to go down Monday morning for next Friday's concert, and stand in line. Since it was the only chamber music concert in town, the line would get long at about 6:30 a.m. All of us would wait in line to get the tickets, and since we couldn't waste that time on those days, we would have our luncheon seminar on line. [laughter] That's the story I thought you were going to mention. It is really true, and quite typical of the intensity of the group.

As I said before, the seminar discussions were very valuable for all of us, and particularly for me. I was in another building and went over each noon to Building 3 for the

seminars. Even though Sanford Rosenthal was a superb scientist, and was very interested in biochemistry, he was basically a pharmacologist. Having access to the Kornberg-Horecker-Heppel group was terribly important to me, and I also imported what I learned there to the group in our building. In addition, we would exchange our own research manuscripts before publication, and we would review them very critically. It was just a superb arrangement.

[END OF TAPE, SIDE 8]

TABOR: What was particularly important was that this was the beginning, coincidentally, of the big explosion in the biomedical sciences at NIH and elsewhere. Horecker, Heppel, and Kornberg had new people come to their laboratory as postdocs, and their work was very exciting. All kinds of people from all over the world came to visit, and we would induce them to give seminars. It was just a fantastically exciting period. It gave me a lot of background.

BOHNING: What was the driving force behind the expansion at NIH? Was it just the discipline itself?

TABOR: That's very hard to say. I think it's mixed. Basically, it reflected the expansion of new techniques in the field, and the increased interest in biomedical research. You also have to give credit to the various directors, to Dr. Dyer, at the beginning, and Dr. Sebrell, and later Dr. [James A.] Shannon.

BOHNING: Did they have to go to Congress for funding?

TABOR: Oh, very much so, but Congress was very supportive. You have to give the directors credit for presenting the work in the right way. The interesting thing is that they never lost sight of the importance of allowing the individual investigator the freedom to do what he wants. I say this very advisedly, because things are not always the same now. I'm always amazed, thinking back on it, that instead of their saying, "You do this," or deciding that this program or that program should be pushed, they relied on the evaluation of the individual scientist. I don't think anybody ever told me or ever told Rosenthal what we should work on.

Like every other expansion there were a number of causative factors. It's true of all government, and it was true of a lot of the New Deal operations. People sometimes expand their programs because of the challenge of the expansion or because they want to have a bigger operation. If it is socially acceptable, the expansion occurs. Otherwise it might not.

When the top people asked for bigger budgets, I cannot say what their exact motivation was, but it was at the right time and in the right environment. When Dr. Sebrell and Dr.

Shannon asked for bigger budgets, I'm sure it was in part because they thought it would be good to have a bigger operation. If the environment had not been as good, it wouldn't have been successful. But it certainly was successful, and I think it was terribly important that it was. Congress was very supportive. I think all the people concerned deserve a lot of credit for this. This is true for both the intramural and extramural programs at NIH.

Perhaps we should take a little more time to discuss the factors involved in the expansion. There are obviously all kinds of factors to consider. One of these factors is that some of the expansion is autocatalytic. Also, some expansion is the result of better techniques. For example—and this is jumping ahead of ourselves—why do we have so many papers now? You can now press a button and in a few minutes get a spectrum across the whole range of wavelengths—UV and visible—that is better than the spectrum that one would have obtained in previous years after a month of work. These developments in techniques were certainly important factors in the expansion.

More than that, expansion occurs more easily if there are many good people working in an area. At this time there was a big influx of good people into science. There are any number of factors across the country that made the difference. One factor that has been pointed out to me is the GI Bill of Rights. It permitted many people to get an education, and some went into science. Another factor is the grants program. I mentioned before that Harvard and a few other schools were unique when I was in school in having good science departments. Now almost every medical school has a reasonably good biochemistry department, and each of them is turning out trained scientists. I think that is important.

Another aspect, which I think is not irrelevant, is the fact that, as far as I know, there are really no prejudices now affecting academic appointments or admission into graduate or medical schools. This applies particularly to Jewish students, and more recently to women and students with Asiatic backgrounds. This lack of prejudice has greatly increased the available pool of bright and qualified students. There are still very few black investigators, and we hope that this situation will change in the future.

I bring Arthur Kornberg's name up so much because he is such an important person in modern biochemistry. After all, if he had not been able to get into medical school and had not been able to get the appropriate positions, think of how much would have been lost to science and how many people that he has trained would not have been trained.

BOHNING: What about post-Sputnik? Did you see any change right after 1957, when Sputnik first went up?

TABOR: No, but I'm not sufficiently knowledgeable of discussions at the administrative or political levels. I must emphasize in terms of your evaluation of my statements, that I really have a double connection to science. All day I work at the bench in an almost old-fashioned kind of way, pipetting and so forth. In the evening I work with the *Journal* [*Journal of*

Biological Chemistry], and am more familiar with the broader aspects of biochemistry. On a day-to-day level, unless I hear about it in casual conversations, or read about it in the newspaper or in *Science*, I'm not involved with the ups and downs of the budgets. Within this limitation I would say that Sputnik *per se* made very little difference to biomedical science, at least intramurally at NIH.

Frankly, NIH laboratories, despite what people say now, have always been much more poorly funded than the good outside universities. We have much smaller groups, much less personnel, much less equipment. We are doing all right; I'm not complaining. Like any government organization, there's a certain stability in it, which is good and bad. In this sense I wouldn't have noticed any effect Sputnik had on funding for NIH.

BOHNING: The reason I used that as a benchmark is because it affected me personally. The summer before, I couldn't get any summer support for my graduate research. The summer after, there was plenty of money for my summer research.

TABOR: In that period compared to now, biomedical sciences were doing so well in funding that there wasn't the same pressure for more funding that there might have been in another field.

Just to indicate the change in funding for biomedical research, in the mid 1940s, the director of the Cancer Institute is said to have refused some small amount of money—perhaps two hundred thousand dollars for fellowships—because he said that there weren't enough people trained to take advantage of that money. [laughter] You can interpret that story in any number of ways. Either he was unimaginative, which I don't think was true, or he was right; i.e. that at that time there really were too few younger scientists in the area. This certainly would not be true now. You have to take all of this in context.

Extramurally, before the War there was essentially no money whatsoever from the government for any kind of funding. It is hard to believe, but essentially all funding for biomedical research was supplied by either local universities or private foundations, and there were very few of the latter. This situation made the whole development of government support for biomedical research enormously significant. There have not been, as far as I know, any good in-depth studies of this important development. One might ask such questions as why did the funding increase? Who made it increase? Why was the funding mechanism set up in the form of grants to individual researchers?

It was really a unique situation. To expand on that even more, we supported foreign laboratories just for completely altruistic reasons. The justification, which I think was valid, was that scientific and medical discoveries anywhere in the world would also benefit the U.S. just as much as if the work had been done in the U.S. Especially after the War this was the only research money that was available in certain areas like Israel or Italy or Spain or even England to a lesser extent. This support of biomedical research all over the world was extremely important.

Funding of foreign laboratories had another interesting and important effect. When this kind of no-strings-attached money went to people at some of these foreign institutions, based purely on who was good and on the quality of the work, it freed them from the hierarchy that had been so paralytic. Unfortunately in some laboratories the situation later reverted to the earlier hierarchical setup.

This positive effect of the grants programs was also true in this country. The grants program meant that a young person who was bright and was doing well could get support and do what he or she wanted to do, without being frozen by the department chairman. This effect was most important.

Since my work at NIH has been in the intramural program, I am not intimately familiar with the extramural program except that occasionally I have been on grants and fellowship committees.

BOHNING: In many respects, you have an academic setting without students.

TABOR: That's right. Very much so. There has been a lot of talk, and there still is, about whether or not a school should be set up here. Dr. Stetten was particularly in favor of establishing such a school. There are pros and cons.

BOHNING: You mean like the Rockefeller? Graduate students only?

TABOR: I think that the proposals have taken various forms. We've tried very hard to give the NIH an academic environment. I think the various directors were very important in creating the atmosphere of an academic environment. In my own case, Sanford Rosenthal, who was the chief of the laboratory, was extremely important in establishing such an atmosphere. Henry Sebrell, who was director of our institute and later of NIH, was also very influential in this regard. He supported me, he supported Arthur and Bernie and Leon, and permitted us to work on whatever problems interested us. We've had a number of other extremely good directors from this point of view.

BOHNING: Who selects the director?

TABOR: The secretary of HHS, but I'm sure it has to go through the president, too.

BOHNING: Is it a political appointment then?

TABOR: You're asking a very timely question. In the early days the directors of NIH and of the Hygienic Laboratory were career commissioned officers in the Public Health Service. The directorship wasn't a political appointment until about twenty or twenty-five years ago, when it became a so-called Schedule C appointment, which makes it technically much more political. On the surface, it isn't political. However, there has been considerable controversy, as you know, with the current [1993] director [Bernadine P. Healy].

Over the years, the NIH director has not really been a political appointment. I might mention here that NIH has always been at an administratively low level in the government. It is a part of the Public Health Service, which is part of HHS. The director and all the administrative people have to conform to all the established administrative guidelines and policies. Usually, except on a personal basis, they have very little access, if any, to the president. What you're asking, however, is a most important question.

Being in the PHS and in HHS had certain advantages for NIH in that you have people in the department who are politically astute in helping with appropriations. It has the disadvantage that the NIH director and staff need to go through another level of bureaucracy. In terms of reflecting the particular policy of the administration in power at any time, there are growing indications that now NIH is in the political arena. In general now when there is a change in the administration, the director leaves. On the other hand, you're dealing with a very big operation, and it's not surprising that it should reflect the political process. Nevertheless, most of us would prefer that NIH not be involved in politics.

This involves another question, which operationally gets more difficult as NIH gets bigger and bigger. The director has enormous responsibilities for both intramural and extramural operations. On a percentage basis, the extramural program has a much larger budget. The director also has to deal with the question of whether NIH is just going to support basic research, or whether NIH is going to take on the responsibility for various aspects of health care, all of which are terribly expensive. From the point of view of the operation of the NIH, these are terribly important questions, and they affect science and the country. Even though you could list a dozen programs we personally favor, such as preventive medicine, if there's a limited amount of money available, money going to such programs would effectively decrease the amount of money available for the so-called RO1 grants, investigator originated basic studies. These are so important for the universities, and as I have already stated several times, are so important for the development of science.

Intramurally, a comparable question arises. Do you continue the small investigator/ small laboratory arrangement, which has worked so well for NIH, or are you going to establish and fund big programs like the human genome project? How much money should go to clinical research and how much to laboratory research? All in all, the NIH has been extremely supportive of real basic research, and of programs that are conceived and chosen by the individual investigator. It has been a remarkable place.

BOHNING: What has been the change in the percentage of NIH grants awarded out of the applications coming in?

TABOR: I cannot answer that exactly since as I have already mentioned I am not associated with the extramural program. However, it is clear that the percentage of grants that are approved is much lower now. I am very concerned about this change in the percentage of grants approved because of the importance of these grants to science.

When I was on one of the study sections about 1960, I would say that at least forty to sixty percent of the applications were approved and were funded. In fact, when I was on the study section, if we felt an application was so poor that it shouldn't be funded, we had to disapprove it. We couldn't have approved it because there might have been enough money to fund it. Now, my understanding is that the figure is around ten to twelve percent, depending on the field. Very good people are not being funded. Young people are not getting grants. More importantly, young people coming up are not going into science because they hear these stories. Another aspect that isn't usually spoken of is that investigators, correctly or incorrectly, imagine that they have to go into more applied projects or more fashionable projects to get funded. Also they choose the biological system that they use on the basis of a real or perceived preference of the study sections. Thus they may decide not to use a non-mammalian system if they are afraid that such studies would not get funded, even if the use of such systems might lead to more significant results. Some of this might be imaginary, but not all of it. It doesn't matter if it's true or not, if this is the perception among many younger scientists. I think that this is a very serious problem.

BOHNING: Has that change been a steady decline? Has there been any reversal of the percentage?

TABOR: I don't keep up with the figures that carefully, but I would say it's been a steady decline. I don't remember any <u>real</u> reversal. There have been some temporary changes, but these are hard to evaluate since the duration of awarded grants are from four to ten years, usually four or five years. Therefore, any change in the percentage approved in any one year does not cause a striking effect on the total statistics. In addition, if the number funded is higher in one year, this may make the percent funded poorer the next year because of the carryover of the commitments made the previous year. All in all, the percentage approved has been going down. This has been a gradual decrease, and it is very serious.

BOHNING: Is that because of—and I know there's probably no simple answer to it—decreased federal spending or increased budgets for projects because of increased costs?

TABOR: I don't think I can really tell you in detail. I think many factors are involved. The most important thing is that there is a lot of good science and many good people. This brings up a conceptual question: How do you predict how many people can be absorbed by the scientific community, academic, government, and industry, or should be? There's no answer to this.

If any one good professor has two or three graduate students a year—a low number—he has trained a large number of good scientists over his lifetime. Each of those students will eventually train more students. I don't know where you draw the line or if one should do so. I should add here that this increase in the number of good scientists is reflected in the very large increase in the size of the *Journal of Biological Chemistry*, and of other journals over the past fifty years. There is a very large amount of good science, and the question is how much the country wants or can afford an increasing amount of good science. In my opinion regardless of how expensive support of research may seem to be, the real cost is very small compared to the benefits. It is cheap. You can calculate that the savings from any one of the therapies that have developed from this research are enormous compared to the cost of the original research that led to the therapy.

We like to say—and I think we all feel this way—that we're doing the research for the excitement of science. That's the motivation. But there isn't anyone I know who wouldn't be happy to find a cure for cancer. One can think of many recent clinical developments that were derived initially from basic laboratory studies, such as new vaccines or therapeutic products made with recombinant DNA, and realize the savings to the community.

[END OF TAPE, SIDE 9]

TABOR: Just imagine how much we will save if they find a cure for AIDS because of basic research in virology. Not if, but when they find a cure for AIDS. Even though there is now a resurgence of tuberculosis, if you think back to the earlier period, the cost of sanitaria, of early deaths and of incapacity from tuberculosis was enormous. The cost of polio was enormous, too. If you calculate these costs, our whole science budget is trivial. Obviously, I have a bias, but I think that it is terribly important for the public and for Congress to realize the practical results that have been derived from basic biomedical studies. I think the budget has not kept up with the amount of science being produced and with the number of good investigators available. In addition, as in medical care, the equipment to do research has become more expensive.

BOHNING: Speaking of equipment, I wanted to ask you about Horecker, because you've commented that he recognized the advantage of the spectrophotometer very early. Evidently he had built a spectrophotometer, although I wasn't clear whether it was infrared or UV.

TABOR: It was UV. Let's go back a little further to show how things developed. When I came to NIH, in one half of one of the rooms in the department there was a big Bausch and Lomb spectrometer, a great big thing. I don't know if you've ever seen one.

BOHNING: I've seen pictures.

TABOR: It was very expensive for its day. I have been told (although I do not know if the story is correct) that when Beckman developed his spectrophotometer he went to Bausch and Lomb to manufacture and sell it. They refused because they said they had all their money in the spectrometer and that was all they were going to do. When the Beckman spectrophotometer came out it was an important development, and at first there were very few of them available. They were only available at that time for military projects.

Bernie Horecker had access to one. It was in Building 2 in the Industrial Hygiene Laboratory. I would assume that he had used the spectrophotometer or a comparable instrument in his work on the yellow enzyme in Chicago. Perhaps it was an instrument that he or others in his laboratory had built.

Dr. Horecker gained access to the Beckman spectrophotometer that they had in Building 2 and used it very actively. That was the first time that the Beckman spectrophotometer was used extensively at NIH. This would probably have been about 1943 or 1944. By the time Kornberg came back from Cori and Ochoa in 1948 or 1949, the Beckman spectrophotometer was well established and available. The Beckman spectrophotometer made an enormous difference for enzymatic work, because you could carry out reaction rates quickly. Some investigators like Horecker, Kornberg, Ochoa and the Coris were very imaginative in converting all of their enzyme reactions to a form, often by coupling the reactions to other reactions involving NAD, which could be measured spectrophotometrically. The reduction of NAD [nicotinamide adenine dinucleotide or DPN] was conveniently followed in the spectrophotometer by following the absorption at 340 nanometers.

BOHNING: That means just following one wavelength as a function of time?

TABOR: That's right. You could measure the reactions in a few minutes. The speed of the assay also avoided the problem of denaturation or proteolytic breakdown of the enzymes that often occurred during longer incubations. That was a big development. Another big development, over the years, was that the substrates and other biochemical reagents and equipment became commercially available.

You just mentioned the possibility that Horecker probably used a homemade spectrophotometer in Chicago. I also know that Hans Stetten, when he was a student, put together a mass spectrometer. The time it took must have been enormous. Sanford Rosenthal, who was very imaginative and expert in so many ways, built a Warburg apparatus for the

measurement of enzymatic reactions manometrically. All that took time. Now you can buy the necessary apparatus, and use the time to obtain results rather than in making the equipment. I think that this development made a big difference but, of course, it increased the cost of the research.

BOHNING: Do you attribute some of the growth of the *Journal* to the fact that the results are in quicker?

TABOR: Without any question you are correct. More personnel is also a factor, but also results are obtained more quickly. More recently the use of the computer has been very important in obtaining and processing large amounts of data. The end result of all of these developments is more and better research. I occasionally point out facetiously that not only can you write the paper faster, but if you want to send it to another journal, the new programs allow you to change the way the citations are written automatically and send it right off. [laughter] A major development in recent years is the enormous amount of information obtained by DNA sequencing, and the use of computers to store and interpret these data. None of this would have been possible without the availability of computers and the appropriate computer software.

BOHNING: You made a comment very similar to what Konrad Bloch said when we were talking about some of his very first papers (23). I think you had commented on this earlier. When you look back at those papers now you see they are on a small-sized page with wide margins. [laughter] Konrad Bloch said the very same thing about looking back on those early papers.

TABOR: He was on the editorial board way back.

BOHNING: You've made the comment that there was no direction given when you came here, and yet you started work with Rosenthal. How did that come about?

TABOR: For most people, they were able to choose where they wanted to work. In my case, even though I didn't quite realize it at the time, I was chosen because the director and Dr. Rosenthal thought this would be a good place for me, where I could really make a contribution because of my background with Hastings and Peters, especially since Rosenthal had already shown the importance of electrolytes in the treatment of burns and shock. It seemed very reasonable to me. In that sense, I wasn't actively given the choice. On the other hand, I think that if I didn't want to work on this project, I might have been assigned to something else. I cannot be sure of that however.

BOHNING: At the same time you said that you and Kornberg worked on a folic acid problem in the evenings.

TABOR: The atmosphere was such that I could do that. The NIH was that way. Also, and this is a compliment to Rosenthal, it just never occurred to me, nor I think to him, that this would not be perfectly reasonable. We were using this apparatus (i.e. the apparatus that Rosenthal had developed for the study of hemorrhage in mice) for a different project. That was one of the nice aspects of the institution.

BOHNING: I'm thinking of the old saying, "If you're on any campus and you want to find the chemistry building, look for the building that has the lights on at night." This was not a nine-to-five kind of situation.

TABOR: I think at the time you were dealing with two different groups. I would assume that the older group, the people who had been here and of whom I'm so complimentary, for the most part did not work in the evenings. The newer group that came with the War and after was completely different. You had this big influx of a group that did work at night.

BOHNING: There was a greater intensity. How did the older group respond to that? You said you gave Rosenthal credit for his attitude, but in general how did the older group react? Did any of them come to your Journal Club, for example?

TABOR: No, but I don't think this is significant. In the first place, the place was small, and most of the other people were not working in biochemistry, with the exception of Jesse Greenstein who had an active group in the National Cancer Institute. The older people did have their own groups, and we were always invited and attended. When I first came to NIH the commissioned officers had an evening meeting once a month where papers were given. Much of these presentations were on the more classical infectious diseases. There was a very nice academic atmosphere. I don't remember any resentment of our small luncheon seminar group.

Earlier I have mentioned that after the War there was an important change in the attitudes of the universities and scientific departments concerning prejudices based on such considerations as religion or gender or geographical origin. The same change occurred intramurally at NIH. One of the reasons for this change might be that if one is going to be given grants or credit for the quality of the work that's done in a department or an institution, then you become more interested in getting the best people to do the job. This is a compliment, again, to the people who ran the NIH, i.e. that they were very interested in having productive work going on. They respected people like Kornberg because of the quality of his work. As I said, Sebrell gave me every support possible. I think this is true now, too. The directors are all under pressure, although perhaps of a different sort now, to show productivity. When site visitors

come to evaluate the various laboratories, the directors of the institutes are under both real pressure and psychological pressure to show that their institutes have been productive. Therefore, they're interested in getting the best people.

Returning to our discussion of our seminars, the only objection I heard was from some of the chemists in the building, who—as I mentioned earlier—worried that we would get ulcers. [laughter]

Another difference between our group and the chemists was in the speed with which we wanted to obtain data, and in markedly modifying our experimental approaches wherever indicated. Chemists were trained, and maybe they still are, to set up their equipment just right. In general, the chemists in our building would set up the equipment a day or two before they did the reaction. In addition, they always wanted to obtain clean crystals and a perfect elementary analysis. It was very hard for some of the chemists to realize that you could come in and get your answer in a few hours. You could get a spectrum without ever having a crystal. [laughter] You could do a distillation without caring whether the equipment was set up perfectly. [laughter] I do want to emphasize, however, that the chemists were all very helpful to us, and I personally learned all of my chemistry from them. Their help in my synthetic studies was invaluable. Their location on the same floor in Building 4 was very fortunate for me.

BOHNING: That's great. Yes. In that early period here you had seven papers published in a year and a half.

TABOR: That seems a little high to me.

BOHNING: From 1944 to 1945, papers numbers three to nine on your list.

TABOR: That's amazing! The reason I say it's amazing is that usually I'm quite careful not to do too much publishing. That number probably reflects the fact that I applied the thinking and analyses on the electrolytes to problems that Rosenthal was doing, and that he was very gracious about including me as a co-author. The one in 1943 was with Hastings on work done in 1941 (9). The 1944 one was from Yale on work done in 1942 when I was interning (11). The next one was the development and use of a technique for hemorrhage in the mouse with Rosenthal (21). Then the next one was the one with Kornberg (22). The next one was also with Rosenthal (24). [Ralph D.] Lillie, a co-author of another paper, was a pathologist, who examined the tissues from our experiments. The next one, again, was based on work I did with Kornberg in the evening (25). We were all set up, since he had the pyridoxine-deficient and folic-deficient animals and I had the hemorrhagic technique that Rosenthal had developed. The next one with Rosenthal was in two parts (26). That was my basic work, and it really had a lot of detailed work. I was analyzing the sodium and fluid changes in shock. The next one, again, was with Kornberg (27).

I'm really quite amazed that we had that many papers. I think that indicates that if you have a technique going, you can formulate experiments and get results relatively quickly. Dr. Rosenthal always pointed out that a very important aspect of any experimental problem is the development of dependable methods. Preferably these methods should be simple and, when working with animals, suitable for using statistically significant numbers of animals.

The experiments with Kornberg depended on the hemorrhage technique that Dr. Rosenthal had developed. This involved cutting the tips of the tails of mice, and letting them bleed into a citrate solution. Dr. Rosenthal had developed this technique to test the effects of various therapies, particularly since this was an important war project. Kornberg and I adapted this technique to study the effect of vitamin deficiencies on blood regeneration in rats. It was a very simple experiment.

The next one was a review that Rosenthal was nice enough to include me as a co-author (28). By paper number ten (29), it was pretty much the end of those studies. From then on, my studies were more biochemical. It was after the War, and Rosenthal had started to work on histamine.

BOHNING: That's what I was going to ask you, how you moved into that area.

TABOR: Histamine is a classic pharmacologic agent, and Dr. Rosenthal was interested from a pharmacologic point of view. Like Peters, he felt that it is most important to develop good quantitative techniques, and we worked very closely together on that. From there, because of the influence of Kornberg and Horecker and their work on enzymes, I started to work on diamine oxidase, an enzyme that oxidizes histamine and aliphatic diamines (30).

BOHNING: What at this point was the situation in enzymology and how had things progressed? This would be around 1947 and 1948.

TABOR: It depends on where you were and what groups and era you're discussing. For the most part, enzymology in the 1930s was based on taking a tissue extract, putting something in, and getting something out. The reactions were long, and the enzymes would be inactivated. Relatively little was done on the purification of enzymes in this country. Obviously there were a number of notable exceptions, such as the purification and crystallization of catalase in 1926.

Due to the group with Warburg, Otto Meyerhof, and some other investigators in Europe, and then Kornberg and his whole group, and very good people like Harland Wood and Ochoa and the Coris, new techniques were developed in the late 1930s and 1940s. First with the Warburg apparatus and then especially with the spectrophotometer, you were able to get your reactions done much more quickly. Refrigerated centrifuges had just come in, and Kornberg

obtained the first one that I know of at NIH. You kept the preparations cold, you carried out the reactions quickly, and you purified both the substrates and the enzymes. The concept that you didn't do much work on an enzyme system until you at least partially purified the components was a big development. The big change in the 1940s and the early 1950s was to do all you could to get purified enzymes, and to do the reactions under conditions where you did not get inactivation either by denaturation or by proteolytic digestion. The bigger development is much more recent with the advent of cloning, permitting the availability of large amounts of pure proteins.

BOHNING: Papers numbers eleven, twelve, and thirteen were all on histamine (31).

TABOR: Just as a background to history and a lesson, you never know where projects can lead. For example, I purified diamine oxidase and that was a very nice piece of work for that period, but then I dropped it. There were certain obscure things about the cofactor, but with the techniques available I knew we couldn't do much about it. Now, forty years later, papers are coming out showing that in diamine oxidase and some of the other amine oxidases there is a very intriguing kind of cofactor involved whose structure isn't entirely clear yet. The nature of this cofactor would have been wonderful to elucidate, especially at that time. The presence of this cofactor in diamine oxidase is really very dramatic because diamine oxidase is a somewhat esoteric enzyme, and yet it has a cofactor that is very basic and fundamental.

From histamine I became interested in histidine, and that was another change in terms of using different techniques. For example, I did paper number sixteen (32) with [Osamu] Hayaishi. I don't know if you know him, but he's one of the most prominent Japanese biochemists. He had come as a postdoc to work with Kornberg and had already developed the use of adaptive bacterial enzymes to study metabolic reactions. We used *Pseudomonas* preparations to study the metabolism of histidine. This was a really important conceptual contribution to all of us—using bacteria to get a much more active enzyme preparation to study a reaction of interest.

In terms of what you were asking me before, another change in enzymology was to try to choose a system that gave you the best enzymes, using bacteria and induced enzymes. Now recombinant DNA is very important for this purpose.

[END OF TAPE, SIDE 10]

TABOR: In paper number seventeen (33), I was using various kinds of techniques, including mass spectrometry, to follow the pathway of histidine metabolism. At that time it was thought, at least by the European group, that histidine was degraded by splitting the ring. In contrast, the Japanese group had shown years before that urocanic acid might be an intermediate. In fact, the original work was done in 1874 when urocanic acid was isolated from the urine of a dog. It was

thought that urocanic acid was an esoteric product, but it turns out that it is the intermediate in the breakdown of histidine. With mass spectrometry, we were able to show which of the nitrogen molecules in histidine was the source of the nitrogen in the glutamic acid formed as a product of histidine degradation.

BOHNING: Was that your first use of labeling? You used nitrogen-15 and carbon-14.

TABOR: Yes.

BOHNING: I think Harland Wood was already doing his labeling work by then.

TABOR: Yes, he was ahead of everybody. Carbon-14 came in right after the War. Before that, when I was up in Boston, Hastings and his group were doing carbon-11 work, but that has a twenty-minute half-life, so it was very difficult to use carbon-11 for metabolic experiments. The techniques we used were well established. None of these papers were that special or imaginative.

BOHNING: When did mass spectrometry come to NIH? This was 1952.

TABOR: It had been there for a while. Julius White was the man who introduced it at NIH. I don't know what they were using it for, but they were using it for quite a while. It was a well-established technique, but it was nothing compared to what is done now. You needed a lot of material, and there wasn't much you could do with it. You could tell whether you had nitrogen-15 in your product or not, but there wasn't that much more. You could tell the structure a little bit, but not like now.

BOHNING: One of the things that I was curious about is that a lot of your papers are in *JBC*, but yet number twenty-four (34) was in *JACS* [*Journal of the American Chemical Society*]. I'm just curious as to why you would pick something like that for *JACS*, when everything else was in *JBC*?

TABOR: I think this was a one-page note, and the *JBC* at that time did not take communications.

BOHNING: That's an interesting point. Let's talk about paper number twenty-five (35).

TABOR: There, again, you get the same story that I mentioned earlier. We purified this amine oxidase enzyme from beef plasma and developed spectrophotometric methods, but we didn't characterize the cofactor. It was always very mysterious. Again, this is one of those instances where the field has just recently exploded in this specialized area with very broad implications. Identification of the cofactor by others rather recently has involved very complicated chemistry, and we couldn't have done it with the techniques we had. However, I wonder whether, if we had kept working on the problem, we might have solved the structure of this interesting cofactor ourselves.

BOHNING: One of the other things I wanted to ask you about paper number twenty-five is that for the first time a co-author by the name of Celia White Tabor shows up.

TABOR: Yes, that's right.

BOHNING: You had mentioned her earlier in the context of being at Radcliffe, and I just wondered whether this would be a point to talk about that situation.

TABOR: Celia received her medical degree at the College of Physicians and Surgeons of Columbia University after graduating from Radcliffe. She graduated from Radcliffe in 1940 and received her medical degree from Columbia in December of 1943. Following her degree she did six months of pathology at Babies' Hospital in New York. Her next appointment was as an intern in medicine at the Massachusetts General Hospital. She was the first woman intern in medicine at the Mass General. Following this she was an assistant resident in medicine at Vanderbilt, just before coming here. We were married in 1946.

When she came to Washington she worked at George Washington University Medical School, and did some attending at what is now DC General. She also worked in the Pharmacology Department at GW Medical School, and at the Warwick Cancer Clinic of GW Hospital. In 1952 she came to work with Rosenthal on the project that Rosenthal was working on at the time; namely the kidney toxicity resulting from spermine injection. Celia and Sanford were particularly interested in the possible role of a serum amine oxidase in this toxicity in view of recent studies by James Hirsch of Rockefeller University. When you culture the tubercle bacillus, you usually add bovine serum or a purified factor (Factor V from bovine plasma) to the culture medium. Hirsch had shown that when spermine is added to this culture, the culture produces a product that is toxic to the tubercle bacillus. Hirsch showed that the toxicity is due to an oxidation product of spermine due to the action of an amine oxidase that is in beef plasma and is a contaminant of commercial Factor V.

Celia and Rosenthal were working on the toxicity of spermine for the kidney, and we decided to purify the enzyme. Here, again, we were influenced by the members of the seminar

group who were so interested in purifying enzymes. As Arthur said in his book (4), he has never seen a dull enzyme. [laughter] That's the background of our entrance into this field. Celia continued to work with Rosenthal, and then I joined them for some additional studies on the spermine problem. After Dr. Rosenthal retired, Celia and I continued working in this area of spermine and other polyamines.

It is interesting to mention how Dr. Rosenthal first became interested in spermine, since it is an indication of how he worked and his different way of looking at things. He was reading the older German literature, and especially, a rather classic book by [Markus] Guggenheim, *Die biogenen Amine* (36), and found descriptions of spermine and spermidine. These compounds had been known for a long time. They were present in large concentrations, but nobody knew what they were doing. He decided that this would be an interesting subject to study. He was interested in the biochemical and metabolic aspects of the compounds he studied, but as a pharmacologist he had an interesting approach. When he was studying a new compound, the first thing he would do would be to inject the compound into mice to see if the compound had any pharmacological effect. Because he was also very thorough and careful, he didn't discard the mice the first day, but kept them for a week or two to observe them. After five days, they died with an acute renal tubular toxicity. This renal toxicity is still very intriguing. We still do not know the mechanism of the toxicity, but his observations stimulated him, and subsequently Celia and me, to study these amines.

Not irrelevant to what we were talking about earlier, some of one's clinical background still sticks subconsciously. I was interested in histamine, which is a pharmacologic agent, and spermine, which has this nephrotoxic effect. On the whole, we used the approach that if you study such compounds—with pharmacologic effects—carefully, and study the enzymes involved in their biosynthesis, metabolism, or physiologic action, the total work would have broad physiologic significance. Some of my friends would, perhaps more correctly, take the approach that it would be preferable to study a subject or a system that you already knew had broad biologic significance. To a certain extent, they were a little critical of my interest in histamine at the time, because they felt that anything that was that specialized would not have the same broad significance. That is a valid point, but on the other hand, as I said earlier when discussing the cofactor for amine oxidase, some studies could end up having broad significance, even though on the surface they initially look like they are highly specialized. These are two different ways of looking at a research problem.

BOHNING: I was just curious about one more thing. You said you met Celia when you were at Harvard and she was at Radcliffe.

TABOR: Yes, she was at Radcliffe, but we were three years apart. I was introduced to her in Boston by a friend. My mother's home in New York was very close to Columbia Medical School, and I would visit her whenever I was home for a weekend.

BOHNING: When were you married?

TABOR: In 1946.

BOHNING: It was around 1950 when the name Experimental Biology and Medical Institute was changed to the National Institute of Arthritis and Metabolic Diseases. I know that you said before that this was just semantics.

TABOR: This change in name had no effect on our research, but it was part of the general expansion of NIH. At a certain point having a single institute became administratively unwieldy. Therefore the NIH was divided into several institutes.

At that time there was special interest in the community in the treatment of arthritis by cortisone. I assume that that was why the name "Arthritis" was incorporated into the name of our Institute—partly because of the interest in working on this subject from the clinical point of view, and partly, I assume, for political reasons. "Metabolic Diseases" was the wording that was added to the Institute's designation to permit a broad interpretation of the work to be done in the Institute. In other words, most of us were in the "metabolic disease" part of the Institute. Most of us didn't do any work on arthritis. The Institute was very good about that kind of arrangement.

The next group of studies listed in the bibliography were and still are of some interest because we showed that folic acid is involved in the degradation of histidine. In a folic-deficient animal, or after therapy with anti-folic drugs, we were able to isolate formiminoglutamic acid, a metabolite of histidine, from the urine (37). Measurement of the levels of formiminoglutamic acid was later used to follow the therapy of leukemia with anti-folic drugs. The involvement of a formimino group during the metabolism of histidine is an unusual and interesting reaction.

We had a paper with Howie [Howard H.] Hiatt on the urinary excretion of formiminoglutamic acid (38) in human subjects after antifolic therapy. That was an enjoyable collaboration, involving patients at the Beth Israel Hospital in Boston where Hiatt, who had been at NIH, was located after leaving NIH.

BOHNING: Then you get into spermine, starting with paper forty-nine (39).

TABOR: Yes. Actually, there is an earlier paper, number forty-five (40). But from paper number forty-nine on, Celia and I have been studying various aspects of spermine and other polyamines.

BOHNING: How did you make that change?

TABOR: As I mentioned earlier Sanford Rosenthal was interested in spermine from his reading of the older literature, and because it had this unexplained nephrotoxic effect. It became obvious that very little was known of the biosynthesis, metabolism or distribution of these amines.

Rosenthal was always open to new techniques and new ideas, and we decided to apply chromatographic and isotopic methods to our studies. I was particularly pleased to be able to learn and apply these techniques, even though they would have been considered trivial by experts in the field. You've recently interviewed Harland Wood and Konrad Bloch, and they would certainly not consider using these techniques as accomplishments. However, for someone with my limited background, to be able to synthesize a labeled compound like putrescine was quite a feat. It gave me a lot of satisfaction. In addition, we now had the isotopic amines that were not commercially available, and we were then able to use these labeled amines for a variety of studies.

That gave us the opportunity to study the metabolism of these amines, and eventually their biosynthesis. We realized how little was known about these amines, and indeed we still don't really know definitively what their functions are. These amines are present in all tissues and in almost all biological material and have many effects when studied in *in vitro* systems. We now have, as you can see from the papers, mutants that show that the polyamines are very important *in vivo* since growth is markedly decreased or stops in their absence. We also know that the polyamines stabilize DNA and certain organelles. We still don't know definitely what their specific *in vivo* functions are, and this is why we are still working on this subject. To a certain extent, and I'm thinking again of the influence of the seminar group on my approaches, we also used our interest in these compounds as a vehicle for studying interesting enzymatic reactions—enzymatic reactions of interest aside from their involvement in polyamine biosynthesis or metabolism. For example, this was true of our studies with spermidine dehydrogenase, diamine oxidase, serum amine oxidase and *S*-adenosylmethionine decarboxylase.

The next paper (41) reports work we did with Dale Kaiser during a three-week period at Stanford in 1961. We learned about phage DNA, and it was good fun learning about all these techniques.

BOHNING: You're not as concerned about titles as I was when I put my notes together. [laughter] But in 1962, in those early papers, you have the title of Passed Assistant Surgeon, which we've mentioned briefly.

TABOR: I didn't realize that designation was in the published papers. That must have been only in the Public Health Reports.

BOHNING: Yes, it was in the Public Health Reports.

TABOR: I don't think that designation would have been used in any of the other papers. Occasionally, I used to get mail addressed to "Passed Assistant Surgeon Herbert Tabor. Dear Mr. Surgeon…." [laughter]

BOHNING: By 1962 you were called Chief, Laboratory of Biochemical Pharmacology.

TABOR: This did not represent a change in location or my work. At that time, the Commissioned Corps had a rigid rule that you had to retire at sixty-four. Fortunately, the rule changed just before I was sixty-four. When Rosenthal retired, I became Chief of the Laboratory. This did not represent any significant change in my work, because Dr. Rosenthal and I had worked very closely together. The designation as Chief of the Laboratory did mean that I had more direct responsibility for the Laboratory. However, both Dr. Rosenthal and I had set up the Laboratory with independent people. Each one had his own little group. With that as the setup, being Chief of the Laboratory really didn't involve too much effort. Setting up a Laboratory or Department in this way is not the usual policy in most places now. There's much more of a tendency now towards bigger groups, here and elsewhere.

BOHNING: That means more administrative responsibilities. Is that right?

TABOR: With the larger groups there are more people working on the senior investigator's problems, and thus this represents a different way of doing things. One of the beauties of NIH, which is not completely unique, is that you can keep your administrative duties under control, so that you can still work like a postdoc or a graduate student if you want. You can literally work at the bench all day without too much interruption, which is what I do. That's pretty hard to do elsewhere, because you have students and faculty meetings. There is one big meeting a month of all of the laboratory chiefs in our Institute. Occasionally we have other meetings, but not very many, and we have our noon seminars. Otherwise, I can work at the bench. That's true of several people at NIH, and it's true of some people elsewhere. I think NIH is particularly good for those who wish to work in this way. People argue whether this approach is an efficient use of one's time, but I find this approach to be more fun.

BOHNING: I remember Konrad Bloch telling me about when he went to Europe on a sabbatical or something, and he was going to work in the lab. He had a project, and one of the

big-name German chemists said, "Well, who are you going to have do it?" Bloch said, "I'm going to do it myself." The German was flabbergasted! "How long have you had your degree?" "Fifteen years." "You shouldn't be working in the lab then." [laughter]

TABOR: There's an argument on both sides. I guess if you're going to direct a group, you have to be awfully confident that your directing is the right way. We've set our laboratory up differently, with independent investigators, and over the years it has worked very well. We've been able to recruit very good people who were attracted by this arrangement and they have added to the atmosphere and contributed new techniques. My approach is not completely altruistic because these investigators have added their input to our work, either in seminars or in personal conversations. Associating with people with other techniques and scientific backgrounds helps everybody. It's been a very good arrangement.

Unfortunately, with the cuts in funding and our inability to expand, it is now much more difficult to set up the laboratories in this way—with many independent or semi-independent investigators. There are too few positions open now and for budgetary reasons they have to be filled very rapidly. In addition, many of the younger people have relatively limited or more specialized backgrounds and need the assistance of some associates. I think this inability to permit younger investigators to develop their own programs is a real problem now. As opposed to the period when Kornberg and Heppel and Horecker came to the NIH, it is no longer feasible to recruit young investigators and to say, "Well, you look promising. You do what you want, and we'll support you." That's not true here or anywhere else now for the most part. That's another overall problem in the whole development of science.

Returning to our discussion about interesting enzymes, in paper number sixty-seven, with Reed [B.] Wickner, we showed that *S*-adenosylmethionine decarboxylase, which is an important enzyme in the biosynthesis of spermidine, has a covalently bound pyruvate that is essential for activity (42). A protein bound pyruvoyl group is a most unusual cofactor, and had only been described once before in histidine decarboxylase by Esmond Snell. Subsequently a small number of other examples of enzymes with this type of cofactor have been described. This is a very intriguing problem, and we still don't know why these few enzymes use this mechanism, rather than one involving pyridoxal phosphate, as is the case with most other decarboxylases. I should also point out that another unusual aspect of this protein is that the pyruvoyl group arises during the processing of a proenzyme. We are still working on this problem.

Wickner now has his own group in this laboratory and has been doing very nice work on yeast genetics. Having a group competent and knowledgeable in yeast genetics has been very useful to us in our current work.

BOHNING: In 1972 the Institute's name changed to Arthritis, Metabolism, and Digestive Diseases.

TABOR: I can't tell you why. I don't really know. I assume that there was some pressure at a political level to include digestive diseases. This change in name didn't affect our work at all. The major effect was at the clinical level. There are all kinds of interest groups that pressure Congress to set up a new institute or a new study, and I do not know much about these pressures or interest groups. One of the best ways to respond to these pressures has been to incorporate the subject matter into one of the existing institutes and perhaps have a section working in the area of interest.

BOHNING: In 1980 it then went to Arthritis, Diabetes, Digestive and Kidney Diseases.

TABOR: That change was presumably for the same reason.

BOHNING: It started out as just simply Experimental Biology. [laughter]

TABOR: Exactly, yes. Yet it hasn't really changed, except at the clinical level.

[END OF TAPE, SIDE 11]

BOHNING: You had some papers involving instrumentation.

TABOR: Nothing much.

BOHNING: There is paper number eighty-four (43), but that's not the one I'm looking for.

TABOR: You may have been looking at paper number eighty (44), but that really wasn't an instrumentation paper. This is really just a methodological paper, nothing very special from the instrumentation point of view.

BOHNING: I'm not clear when liquid chromatography became a standardized technique. Was it earlier than that?

TABOR: Yes. Chromatography was an old technique. Ion-exchange chromatography was used during the War for the separation of uranium. This type of chromatography was

introduced into biochemistry in the 1940s. At first the major use was for separating nucleotides. Herb [Herbert A.] Sober, who was one of our neighbors, was most important in applying ion-exchange chromatography to the separation of proteins. Together with Peterson he made a very important contribution by developing DEAE cellulose, an ion-exchange derivative of cellulose, that is still used extensively today for the purification of proteins. I think that their paper was in 1954 (45).

BOHNING: In paper number eighty-three (46), which is a review paper from 1976, you stated, "It is generally accepted that polyamine biosynthesis is intimately related with the synthesis of nucleic acids and proteins."

TABOR: I think that there are any number of observations suggesting such a relationship. For example, ornithine decarboxylase, which is the first step in the biosynthesis of putrescine, increases enormously when you stimulate growth. You can also stabilize ribosomes with polyamines. We have shown that amber mutations are affected by the presence or absence of polyamines, using a polyamine deficient mutant. There's a lot of suggestive work for this hypothesis, but no definitive proof yet. We have a lot of evidence for it, but not the clean kind of evidence that we really would like. This is conceptually very difficult.

The beauty of enzymatic work and how it has changed over the years is that, instead of doing experiments in the whole animal or in crude tissues, you focus on an *in vitro* system. Using such *in vitro* systems, investigators have obtained beautiful answers, which, despite initial criticisms, have on the whole proven to be valid for the whole animal or organisms, although there are certainly some exceptions.

In the polyamine area, the situation is more complicated because the polyamines, since they are strongly basic, bind strongly to polyacids. When you break up the cells, regardless of where the polyamines were in the whole cell, the polyamines bind to whatever polyacids are around. You may find them associated with nucleic acids or ribosomes in broken cell preparations, but you don't really know whether the polyamines were associated with these moieties in the whole cell. Once the polyamines bind to other components of a lysate or an enzyme system this binding may affect the behavior of these components. For example, in studying reactions involving nucleic acids, added polyamines may affect a nucleic acid substrate by binding the nucleic acids or precipitation of the nucleic acids or by inhibiting nucleases. One can get all kinds of *in vitro* effects, but, as opposed to most studies in other systems, it's hard to extrapolate from these *in vitro* effects to what is really happening *in vivo*. That's one of the reasons why we've been pushing so hard to get mutants in the biosynthetic pathway for polyamines, and to find what defects these mutations produce in the intact organism. Even this is a conceptually difficult approach because even with good mutants the effects one sees may not be the primary ones. Thus again we go back to the rubber band story that I mentioned earlier in which a primary effect leads to many secondary effects. This is the challenge—to try to simplify the system, and to find what the polyamines are really doing.

BOHNING: You retired from the Corps in 1983. Your title was "Medical Director". Is that a rank?

TABOR: Yes. That's like a captain in the Navy or a colonel in the Army. I then shifted to the Civil Service, although this change did not represent any significant change in my work or in my administrative responsibilities. The motivation was a very local one. Dr. [C. Everett] Koop was the Surgeon General at the time, and Congress had changed the retirement law to permit him to be appointed, since he was sixty-five when he came in and sixty-four had been the obligatory retirement age. This change abolished the retirement age in the Corps. Nevertheless, for the Corps as a whole, he strongly urged people who had more than thirty years' service to retire. It was optional, but there was no reason not to retire and switch to the Civil Service.

On the whole, the shift from the Corps to the Civil Service made no difference in salary because there are rules against any significant amount of dual compensation. If I had gone to a university, I would have received my retirement from the Corps and a new salary. By staying at NIH we get little or no retirement pay until we actually do retire.

BOHNING: Is there anything else in your papers? You said earlier you've been continuing in that area.

TABOR: We're continuing to study polyamines, but have been taking advantage of genetic techniques and the new techniques in cloning, overexpression of enzymes, and DNA sequencing. We're trying to learn all the exciting things that are going on now. We are trying to acquire all these new techniques, and to use them for all of our studies.

BOHNING: How do you find the training of people coming into NIH today as opposed to fifteen or twenty years ago, given that today's work is certainly more sophisticated, if I can use that word?

TABOR: I think it's hard to answer that, because your question really addresses a rather fundamental problem; namely, the quality of the young investigators coming into the biochemical fields. Most laboratories at both NIH and elsewhere are having trouble getting well-trained postdocs, although this is probably not a problem for the very best departments in the best schools. It is also hard to compare the background of the present postdocs with those who came twenty years ago, since now to a large extent, both here and elsewhere, most of the postdocs are not from this country. You are dealing with a different set of people with different scientific backgrounds and training.

In addition, as I mentioned earlier, in the earlier period NIH was attracting very bright M.D.s who had no training at all but who eagerly picked up techniques and knowledge. Now most of the postdocs are Ph.Ds. Many of the Ph.D.s that are coming to NIH from this country and from some foreign laboratories have adequate training—and often superb training—but the training is usually limited in breadth. In part it is limited in breadth because there is so much to know and they have been forced to focus on a limited area for their Ph.D. work. It is hard to find individuals that are trained and interested in broad areas of science. As I have mentioned before, however, the big problem is that many of the best students at universities and medical schools are not going into basic science. This is a very real problem.

BOHNING: Is that narrow focus a natural consequence of growth in science?

TABOR: That's right. For example, as I mentioned earlier, when I first came to NIH we had a small group of commissioned officers who periodically met in the evening and we covered everything that was being investigated at NIH. I learned an awful lot about classical microbiology. As NIH expanded, we started to spend more time in our own seminars. Right now, as you can imagine, I'm quite busy with our local seminars and our interactions locally, and I practically never go to seminars elsewhere at NIH.

NIH has many excellent seminars, a dozen a day at least, and I don't go to them. If you are going to work in the laboratory and have your own seminar, you cannot attend these other seminars, or at least not often. To a certain extent what we learn is limited unfortunately to what I and others present at our local seminar. I am sorry that this is the situation. Even though we cover a wide variety of subjects in our local seminars, this does not substitute for interaction with investigators in other laboratories. A lot of good science is stimulated by interpersonal reactions with investigators from different laboratories.

Now, you effectively have ten thousand people or more at NIH, but this large size does not have any positive effect if one is not exposed to their work and thinking. NIH always has many very distinguished foreign scientists on the campus, but you just don't meet them. In recent years it has become a bit of a challenge to keep our local seminar going, because the whole principle of our seminar depended on the wide variety of topics presented. For this purpose each person would choose what he or she wanted to present, either from the literature or from their own work. This gave everybody a broad background. As I mentioned above, nowadays individuals and groups are much more specialized, and are less interested in this type of broad approach. This is a problem all over, both at NIH and at universities.

BOHNING: We have not talked at all about any of your other activities.

TABOR: I think that is probably what most people would be interested in.

BOHNING: I'm not sure where we should start on those, although I suppose the most important one is the *Journal of Biological Chemistry*.

TABOR: Yes. That's the main one.

Before leaving our previous conversation, I want to emphasize that in terms of the administrative aspects at NIH, there are other people who could tell you much more about the details. On the other hand, I am the only one who has been here over this whole period. Even though what I've told you is obviously limited by what I know, it's from a different point of view than you would get from an administrative person.

The period from 1943 to 1950 was an extremely critical point in the development of NIH and NIH's role in the scientific community. It is amazing to me that NIH developed so well. I'm still amazed at how we've overcome or bypassed the usual kind of administrative problems associated with a government organization. The people concerned deserve a tremendous amount of credit.

BOHNING: Is part of that due to the fact that the administration was mainly composed of scientists of some sort? Many people today are trained as administrators without ever being involved in the real operation itself.

TABOR: I'm not sure. You could argue that both ways. Perhaps yes, in part because they somehow had a feeling for what was good. But on the other hand, you have the danger, which fortunately did not happen, that very often scientists who are put in administrative positions feel that because of their background they should be able to tell people what to do. That has not happened for the most part at NIH.

We've had some very good directors of NIH, and we've had some very good scientific directors in our institute. I've already mentioned Floyd Daft, Hans Stetten, and Ed Rall. They all seemed to have had good insight. The other institutes also had some very good people. Alan Rabson in the National Cancer Institute is a particularly good example, and over the years there have been many others that I haven't mentioned. Jack Orloff has been very supportive in the Heart Institute, and I could go on and on. In fact, the number of those who haven't been good is extremely small.

BOHNING: Were they removed quickly before they could do any damage?

TABOR: The poor ones? I think one of the advantages of NIH, and it is also a disadvantage, is the structure. It's not that different from universities, but there are certain differences. One is

that nobody has absolute control, with all our checks and balances. Of course, this does have some disadvantage in that you can't really develop your own program to the same extent without selling it to somebody else.

On the other hand, if things are not going right there's peer pressure to change things or to rearrange things to take care of the problem. That works both ways. Departments can be reorganized and space can be rearranged. You can see where at times that could be very bad. It also serves to take care of situations where someone is not active anymore. In the university, before grants and even with grants, some people would stay around indefinitely even if unproductive. To a large extent, despite what people think outside, that's not true here. Despite the tenure system, there are ways of rearranging things. It's also a big enough operation so that people who are not suitable in one area can go into another area and be very productive. Someone who may not be very good in the laboratory may be very good in administration, or may be very good in directing a certain fellowship or grants program. On the whole, it has worked out very well. This is my own opinion, and you might get different thoughts from other people.

BOHNING: I think that what you've said about NIH is very valuable. As you said, you have a window back on a time that not too many people have.

TABOR: I've really had two lives, in terms of my work. For several reasons, I have kept the *Journal* operation completely separate from my lab operation. The technical reason is that, because of government restrictions, you cannot run a journal from a government laboratory with government secretaries and so forth, as you could if you were in a university. Despite the fact that the NIH has been very good at trying to make every possible arrangement to keep it like an academic institution, they still have certain unavoidable restrictions, and this is one. The *Journal* is a big operation now, and we certainly couldn't take half a floor of an NIH laboratory and use it or government personnel to operate the *Journal*. This would not be feasible or permitted.

More important than that for both myself and the *Journal*, I do have an active laboratory in which I'm actively working. I'm a strong believer that it is important for the *Journal* that an active scientist runs it. I say this very advisedly, because many journals are not run this way. Many journals are really run, for one or another reason, by staff with an advisory group. I could list any number. *PNAS* [*Proceedings of the National Academy of Science*], although it has an editor, does not have a centralized review system, and is largely run by staff. (See appendix note iii) *BBA* [*Biochimica et Biophysica Acta*] is basically run by staff. Overall policy is developed by scientific personnel, but the actual operation is not. At *Science*, I'm sure Dr. [Daniel E.] Koshland, the present editor, keeps abreast of what is going on, but the day-to-day operations are certainly run from the Washington office.

I think it is extremely important that the scientific journals be run by a scientific staff. Unless you're working in the lab yourself or very close to people working in your laboratory,

you don't really know the problems of the individual investigator and cannot appreciate the problems he has in publication and in the importance of publication for his work and position.

[END OF TAPE, SIDE 12]

TABOR: The *Journal* is a rather large operation. We receive and publish a lot of papers. We printed twenty-eight thousand pages last year and receive over seven thousand manuscripts a year.

The manuscripts are submitted to the Bethesda office. I speak to the office by phone once a day, usually at lunch or after lunch, depending on whether I have a noon seminar or not. I try to assign the papers to an associate editor at this time. I can do that from the title for about three-quarters of the manuscripts. The other quarter are sent to me at night. Fortunately, my home (at NIH) is very close to the *JBC* office, which is nearby on Rockville Pike. The *Journal* staff delivers a bag to me in the evening with the work that I need to do in the evening, such as assigning manuscripts that I could not assign on the phone, letters to write or sign, questions from the Redactory, and other items that need attention. Someone from the *Journal* office picks up the completed materials from me in the morning. Obviously, that keeps my evenings quite busy!

Computers have helped an enormous amount, because I do all my typing on the computer, and I send it to the *Journal* office by modem in the morning. That has been extremely useful from a technical point of view, because it saves time. In many cases, the office personnel can then even sign my name and mail or FAX the letters, because the text comes to them via the modem exactly as I wrote it. There's no worry about spelling which would be otherwise be a problem with the large amount of technical terms that are involved in our letters. We can come back to some of the technical aspects later. I do want to emphasize that, with the large amount of material that is handled by the *JBC* office in Bethesda, the smooth operation could not have occurred without the excellent staff in that office. Of particular importance are Chuck [Charles] Hancock, Executive Officer of ASBMB [American Society for Biochemistry and Molecular Biology] and the Manager of the *Journal*, and Barbara Gordon, Director of Administration of ASBMB and Assistant to the Editor.

Let me just mention the principle under which the *Journal of Biological Chemistry* operates. The Publications Committee, which is an elected committee of ASBMB, has the primary responsibility for the *Journal*. There was a big change in the operation of the *Journal* around 1958, when John Edsall took over. As you know, the *Journal* started about 1905, and it has grown steadily since then. I've forgotten the exact doubling time, but I think it's every ten years. Like compound interest, the number of pages is now enormously larger than when I had my first paper in the *JBC* in 1943.

For historic and other reasons, we are set up with a number of associate editors. There were three or four when I started; now we have about seventeen with expertise in different

fields. We send the papers to one of these associate editors. We have been very fortunate in that we have had a superb group of associate editors. The associate editor in turn sends the manuscript to one or more editorial board members, usually one to start with. The editorial board is appointed by the Publications Committee from a slate that the associate editors and I nominate. We also supply the Publications Committee with such information as which members of the editorial board are retiring, what subjects we need to have covered, and so forth.

As opposed to most journals, our principle is that the editorial board member is an editor and not only a referee. He can act as a referee too, but we want the editorial board member to think of the overall contribution of the manuscript and to even write a draft decision letter. He is usually closer to the field of the manuscript than is the associate editor. Depending on how confident he is in his knowledge and judgment in the particular case, he may or may not send the manuscript out to referees, who send back the referee report to him. If the editorial board member recommends acceptance, that recommendation is almost always accepted. If the recommendation is return to the author for revisions, the associate editor looks to see that it has been reviewed adequately, and whether there are opinions from other referees. If he is satisfied, which is usually the case, he sends the comments and a decision letter back to the author. If the editorial board member recommends declination, then either the associate editor examines the manuscript himself and writes a decision letter, or he sends it to another member of the editorial board for another opinion. It's a very decentralized operation. We try very hard to expedite the reviews at every step. For example, for the last two years, we have sent all our opinions to the authors by fax. We use express mail to the associate editors.

I handle a fair number of the manuscripts personally, as an associate editor, because I wanted to have a feeling for the content of the manuscripts and the problems involved in the overall review process. Up until recently, I took as many, if not more, than the number assigned to each associate editors. However, with the increasing size of the *Journal* in the last few years, I'm now taking a little less than the other associate editors. There are enough other things that come up, as you can imagine, in the operation of the *Journal* and in terms of policy matters to keep me busy.

You probably noticed that we have a mini-review section. The associate editors choose people to write four-page articles in which they review a field. We aim at having these minireviews written so that they are understandable to the average reader of the *Journal*, not just to experts in the field. Once a year, we collect these minireviews in the *Compendium* that is distributed free to each member of ASBMB.

Let me get one of these *Compendium* volumes to show it to you. This compilation is very useful for teaching purposes. Since it is reprinted from the published papers, we can sell the Compendium very inexpensively. If people buy twenty-five, the cost is between four dollars and six dollars a copy. I personally find the *Minireview Compendium* exciting in the sense that, when people ask, "How can you fill up the journal week after week and get a collection like this?" I say it's no problem at all. Science is moving so fast that the list of possible minireview topics and authors is enormous in every field. The minireviews summarize the present status of an area in a very concise manner, and are written by excellent authors who are experts in their

field. It is striking that many areas in which we published minireviews several years ago have developed so far in the interim that we can often consider publishing another minireview now on the same general topic.

Getting people to write minireviews and getting them to write within a four-page size limitation and with good clarity are two entirely different things. It's quite a challenge, but it's been a lot of fun. It also indicates to the readership the kind of subjects that we are interested in having in the *Journal*.

BOHNING: I was just looking to see when it started. That was in 1988.

TABOR: It's not too many years ago. The idea of minireviews was first suggested by Dr. Charles Yanofsky when he was president of ASBMB, although others have also mentioned the possibility of having minireviews. The first minireview was written by Arthur Kornberg.

The *Compendium* was almost completely my idea. Even though each minireview article receives a lot of attention, and the authors of the minireview articles receive many reprint requests, individual articles tend to be sort of lost, especially if they are in a large journal. I reasoned that once people have written these minireviews, why not put them together and have a better distribution. Then I had the idea that, since we didn't want to spend money on extensive advertising, once these articles have been collected in a volume, we could send the collection to our membership without charge, and thus markedly increase the number of people who see the minireviews. This distribution had another practical value, in that I found that it is was much easier for me to get people to write the minireview articles if they knew they would have such a large audience. We are particularly interested in having a wider distribution to students and to biochemists abroad.

We have recently introduced a new modification in printing and distribution of the *Journal*. We now have a CD-ROM addition with infinite retrieval capabilities. All the articles are included in the CD-ROM version. You can select any word in an article and it will immediately list all the articles that have that word. If you type "spermine" you'll find all the articles with the word "spermine" anywhere in the text. The CD-ROM includes the figures, so it's an exciting development. (See appendix note iv.)

BOHNING: This is a very important concept.

TABOR: The main challenge, coming back to the basic *Journal*, is to have a good editorial board and good associate editors. We have been very fortunate in having excellent members of the editorial board and excellent associate editors, as well as many excellent referees. Reviewing is always difficult because you want to do the right thing both for the authors and for the readers of the *Journal*. We do have the principle that we always permit reconsideration of a

declined manuscript if an author feels that he can rebut the criticisms. My personal policy, with which the associate editors agree, is that we will not be dogmatic about any decision. If an author objects to a decision, I feel that the fairest approach is to get another review.

You might argue that to a certain extent we've been too successful. The number of manuscripts received is still increasing by eight or ten percent a year. Next year we undoubtedly will have to publish weekly. We're trying to cut back a bit, and that brings up the very difficult question of how many pages we can or should publish. More importantly, how many pages can the readers absorb? This is why we think the CD-ROM is a good development, because it permits easier retrieval of the articles of special interest to an individual reader.

I think the large size of the *Journal* is a function of the success of the science establishment and the success of the grants system. It has been very exciting to see this all develop. Someone calculated that if we published the *Journal* every day, each issue would contain sixty-five pages! [laughter] Incredible!

BOHNING: Let's go back to some early history. Let's go back to 1961. My indication is that you were at least on the editorial board beginning in 1960.

TABOR: I was a member of the editorial board from 1961 to 1966, under John Edsall. I should use this opportunity to say what's perfectly obvious and very important, that John Edsall was extremely influential in setting the pattern for the present operation and reputation of the *Journal*. The atmosphere, the fairness and the lack of dogmatism, and the confidence people have in the *Journal* are the legacy of his tenure as Editor-in-Chief.

Before about 1946 the *Journal of Biological Chemistry* was the only biochemical journal in this country, and there was some perception—although I have no reason to believe that this perception was justified—that the members of the small editorial board were occasionally arbitrary. This perception still persisted even after the War, and posed a problem for some authors despite the introduction of other biochemical journals in this country such as *Biochemistry* and *Archives of Biochemistry*. Perhaps the criticism was unfounded. Nevertheless when John Edsall became editor, he quickly created the atmosphere of a society journal unequivocally operated for the good of everybody. I think John made an extremely important contribution in this regard.

BOHNING: I was going to ask, since you said Edsall was the person who selected you for the editorial board, what do you look for in a person as an associate editor?

TABOR: I should divide my answer into two parts; namely, appointments to the editorial board and appointments as associate editors.

First, what do we look for in editorial board members? We usually like to have people who have been used as referees on a number of occasions by previous members of the editorial board, and who, in the opinion of the editorial board members or the associate editors, have given very fair, honest, balanced, knowledgeable reviews without any bias. We also would like them to be leaders in their field, or, if they are relatively young, not necessarily the leader in the field but with an excellent scientific reputation. We feel that it is important for the people whose papers are being reviewed to be able to look at the editorial board roster and to have the confidence that all of the names listed are excellent scientists.

Also, we are influenced by who is leaving, in which fields do we have the greatest demand for new editors, and what fields we want to attract. There was a period where we worried that we didn't get much of the newer molecular biology. Correctly or incorrectly, we may appoint a few new editors who represent such new areas to show the community that we want papers in these fields. Before invitations are issued, the possible candidates are discussed extensively by the associate editors and me and a list is sent to the Publications Committee for their final approval.

In the past the Publications Committee took a very active role in the appointments of editorial board members. In the early days, when the editorial board was smaller, the Publications Committee really went over each name individually and often made new suggestions. As the *Journal* has become bigger and bigger, they still went over each name carefully, but this is harder when we have fifty appointments for a given year. Since not everyone invited will accept, this means going over seventy-five or eighty names. Obviously the Publications Committee has to depend more on what we tell them about the qualifications of the people. The associate editors have a very important role in these nominations.

With regard to the associate editors, when we have a vacancy or we expand and need another associate editor, we try to use the same criteria except that we usually select the associate editor from present or past members of the editorial board. The associate editor requires a different combination of techniques and competence beyond that required for members of the editorial board. The associate editor has to be able to deal with the authors directly. He doesn't have to know the individual fields as well, but he has to know who the best editorial board members would be for the reviews. He must be able to evaluate the competence of the reviews and has to decide when another review is needed. The final appointment of each associate editor is made by the Publications Committee.

In very recent times, the associate editors have had another very important function. With the *Journal* getting this big, the associate editors get together two or three times a year and decide on a lot of policy questions, subject of course to the Publications Committee. The associate editors know so much about the needs of the *Journal* operations and of the problems that each may have. At these meetings we try to reach a consensus on such subjects as what criteria do we use for acceptance of certain types of papers. For example, how do we define a "significant contribution". One of our continuing problems is an effort to attain some uniformity in the criteria used by the different associate editors and by the members of the editorial board. One of the problems of the large number of associate editors and of the large

editorial board is that two different associate editors or two different editorial board members might act differently on manuscripts that are comparable but are sent into the *Journal* at different times and by different authors. We discuss all of these problems amongst ourselves and if indicated send the results of our deliberations to the Publications Committee

BOHNING: You were selected as associate editor in 1968.

TABOR: That was by Bill [William H.] Stein, who was editor at the time.

BOHNING: Do you know why he selected you?

TABOR: No, I don't. I hope he did for the reasons we just mentioned. [laughter] Of course, it is also possible that Dr. Stein and the Publications Committee also thought that it would be nice to have one of the associate editors in Bethesda. That isn't essential, but it might have been one of the considerations.

I might mention also, from a historical point of view, that when John Edsall was Editor, he operated out of Cambridge, and before that the *Journal* had an office in New Haven. I'm not sure whether they continued the latter office for a period when John was editor. Then it became apparent that with the increased size of the *Journal* it couldn't operate completely out of the Editor's office. A similar problem became apparent in the operation of the Society's offices. It also became clear that the *Journal* offices and the Society offices couldn't move every time there was a new editor or a new president of the Society. Since 1964, the Society offices have moved here permanently, regardless of who is president of the Society, and the *Journal* has its offices here too.

TABOR: What kind of computer do you use?

BOHNING: I prefer Macintosh, but we use a PC clone at the office.

TABOR: The same thing is true here. I use a Mac since I find that it has many advantages for me. This is in general a difference between the scientific community and the office community. All of us use Macintosh in the lab, and I use a Macintosh at home for my Journal work, but in the *Journal of Biological Chemistry* office they have PC clones. Anyway, as I mentioned previously, I send my typed material to the *Journal of Biological Chemistry* office by modem each morning, and, after they receive this transmission, it is all compatible with their PC computers.

BOHNING: I now have a Mac Power Book, and I have my own Macintosh, and then I have the office PC. At the office I may do things on either the Mac or the PC. With the Power Book, I have it configured so that one quarter of it is DOS.

TABOR: I see. You can do that now?

BOHNING: I think it's called Soft PC; there are other products out there.

TABOR: I've seen them listed in the computer magazines.

BOHNING: I have WordPerfect in DOS on there, so I can type something and when I get back to the office it's ready to go. I don't have to convert it.

TABOR: I see. I send mine over as text only. Of course, then I can't use Greek letters or bold fonts, but usually I don't have to. I have been using a new computer that I just obtained because of the CD-ROM. It's so nice that I am almost ashamed of it. [laughter] I need it for the CD-ROM, because I think it's very important that I examine the CD-ROM disks of the *Journal* when they are issued to be sure that the printed version has been transferred correctly to the electronic version.

[END OF TAPE, SIDE 13]

TABOR: If it weren't for computers we could never have been able to handle this amount of material in terms of our total operation—including the office operations, keeping track of manuscripts, and the material that I send to the office each day.

Years ago Waverly Press, our printer, tried scanning manuscripts, but they were ahead of the time and only had limited success. Now we get the disks from the authors, and the Redactory edits and prepares the manuscript for printing on the screen. The authors are very satisfied, because you don't get the errors that you get from keyboarding the text at the printer. The printers now use computer input and fast photographic methods prior to the final printing. No hot lead has been used for many years. It's rather incredible.

BOHNING: Do you see the day coming where there won't be a physical volume of the *Journal*, but rather an electronic one?

TABOR: I think so, but it's a long time off. I think our CD-ROM shows where things are going. Some institutions already have carrousels in their library where you can put the CD-ROM in and retrieve the contents anywhere on the network. It's going to involve all kinds of complicated questions of financing the *Journal*, but I would think that electronic publishing is going to be the thing in the future. When, I don't know. I don't think it will be immediate. I think they'll both live together for a while.

There is a real generation gap, too. However, even among the young people now who are much more familiar with the computer, there's still a reluctance to do everything on the computer. People still like to handle paper. I think this reluctance is decreasing, and I think people are going to get much more used to using CD-ROMs or the equivalent.

I think it's just wonderful. We just got our own network at the NIH, and we can upload and download so much material instantaneously. I definitely think an electronic journal is in our future, but I don't think it's going to be this year or next year or the year after.

At first we worried about our pricing for the CD-ROM and licensing arrangements, and the implications of the use of carrousels on a network. I don't have the feeling we have to worry about that now. I want to see the technique accepted, and then we can worry about how we pay for it. At this moment, I don't think we're at that stage. Some of the commercial people have already called us about licensing arrangements for the CD-ROM, because they are perhaps ahead of us in wanting to be able to download it to any place in their institutions. I think it is in the future.

BOHNING: I still like to hold something in my hand. [laughter]

TABOR: Yes, I think most people do. There are other advantages of the print version too. Still the technology of computers is going to get better even though the present CD-ROM has certain limitations. It is still somewhat cumbersome if you read a new article and you want to see the references. It is still not too convenient to turn to the bibliography, and then get back to the original line in the text. Also, I think it's a matter of being brought up on this technique to be able to use it easily.

BOHNING: I used to tell my students that the one thing that I felt was always important when using the literature was to browse.

TABOR: That's right. However, you can do that even better on the CD-ROM because you can quickly go down the line. The other thing that I think is important is that now with the size of the journal and the size of the scientific literature, you can't keep many journals in your house. Since CD-ROM disks obviously take little room, this is not a problem. Right now we're selling

the CD-ROM version very inexpensively to members. I think it was only forty-five dollars last year for a year, for the twenty-eight thousand pages.

There are other conceptual problems that time will have to take care of. For example, what do you put on a disk? Right now, we're doing three months' worth, because you don't want to get a disk every week. You could also have more than one journal on each disk. The limitation on capacity is the memory that it takes to store figures. There's a lot of scanning involved. If we only used text and no figures, we could put up to ten years of the journal on a single disk. However, we only have three or four years available at present on the source computer at the printer (or on the disks stored there). You can retrieve an article by the subject you want, get the title, and then go back to the full disk for the whole article. Then there are other techniques like downloading, and you can print out the article.

We were talking about your Power Book. The incredible thing is that this is so much more powerful than the most powerful computers were just a few years ago, and they were much more expensive. This is practically the same price range as I think a Mac 512 probably was when it came out. It's just incredible how the technology has developed. (See appendix note iv.)

BOHNING: It doesn't take long. It's amazing. I have some questions about John Edsall's article in 1980 called, "Seventy-five Years of the *Journal*" (44). For example, he mentioned something about page charges. Page charges were instituted and then dropped and put back in because of controversy. Do you have any comments about that?

TABOR: They are controversial. I think they're very important, conceptually and as an additional source of funds and as a financial buffer. The controversy is whether or not it's discriminating against people who don't have big grants, and some object to the general concept of having the authors pay for publishing their article.

I personally have a bias for page charges. The charge is small, compared to the total cost of the research. For example, the charge now is roughly fifty dollars a page. The average article is seven pages; that's three hundred fifty dollars per article. One might argue that if your lab or department publishes a lot of articles, it really amounts to a lot of money. On the other hand, if you divide the average total budget—including overhead and everybody's salary, including supporting staff, such as dishwashers—by the number of papers published by the department per year, the work responsible for an average paper costs a minimum of twenty thousand to forty thousand dollars a paper. Often the cost is closer to a hundred thousand dollars per paper, much more than the page charges for the publication of the results. I frequently note when discussing page charges that many ordinary laboratory materials (a little bit of simple restriction enzymes) cost a minimum of forty to one hundred forty dollars. I think the concept of page charges is a good one, and they should be considered as part of the cost of the experiments, as with all other laboratory materials. If the federal government is going to be supporting research, the publication of the research is a very integral part of the research. It's just as important a part

as a restriction enzyme. You don't expect the company that sells the restriction enzyme to give it for free. It's an integral part of the operation. I don't think it's unfair.

We also have a system whereby the Society will give a grant to anybody whose institution says that the person does not have funds for page charges. There are occasional cases where someone is retired and writing up what they have done, or where they have lost their grant. The Society does give a grant to pay their page charges. The total money needed for these grants is not great. Also, for some foreign authors such grants are needed because of problems with currency exchange, et cetera. There is one small problem, which fortunately does not occur too often. There are some institutions, especially a few institutions abroad, which won't pay page charges. The policy of a few governments is that if it's a government laboratory or government-funded research, the institution will not pay page charges for publication.

Page charges only pay less than a third, or maybe now a fourth of the cost of the publication, perhaps even a little less; I haven't calculated the percentage recently. But the advantage is that it offers us a financial buffer. If there is a sudden change in paper charges or some expansion in the number of pages, we can't change the subscription rate right away because we're committed to that for a year in advance. The page charge system gives us something we can change immediately. If we predict that we are going to have a loss, we can easily change the page charges and partially compensate for the loss. That's a very important option.

Apropos of what I was saying about the CD-ROM, we don't know what's going to happen to publishing in general. If it gets to the point where technically one CD-ROM will supply an entire institution, we'll have to have other ways of paying for the publication. Some people say it won't cost anything to publish because you won't have a printed journal. That isn't true because there's always composition charges, redactory costs, the costs of the review process, et cetera. Those are the arguments for page charges.

BOHNING: Who instituted them? Was that you or Stein?

TABOR: It was during my time. It was done by the Publications Committee and the ASBMB Council. Dan Koshland was president of ASBMB at the time. It took a change in federal policy to permit the use of grant funds to pay page charges. I remember there was a final decision of some intergovernmental group. The government will only permit the use of grant funds to pay for page charges for nonprofit journals, which is, again, a little controversial. It has come up for discussion again in one of the recent articles in either *Nature* or *Science*. I gather that the for-profit journals are asking that they be allowed to have page charges. That's getting into more of a controversy. I don't know what will finally happen.

That's the background of page charges. It's no longer a major question, at least right now. The libraries are having troubles, too, and even though we could increase our subscription

charges, we'd rather not. The bottom line is that you have to pay for the *Journal* one way or the other. What we've tried to do is pay for it in a number of different ways.

We do, for various reasons, have a very efficient operation. Our subscription charges per word are about one-tenth or one-fifteenth the cost of some of the well-known commercial journals. The efforts of the present manager of the *Journal*, Charles Hancock, and his associate, Barbara Gordon, and the previous manager of the *Journal*, Robert Harte, were very important in effecting such an efficient operation.

BOHNING: In John Edsall's article he said, "The *Journal* continues to publish all papers the editorial board deems worthy. According to a policy statement of 1968, there is no arbitrary limit on the number of pages."

TABOR: That's true. This is why we like to have the flexibility of the page charges, so that if we do receive and accept more papers than we had predicted, we could still cover the cost of their publication. However, that's no longer a real factor because from simple statistics, when you have this number of manuscripts, your predictions are pretty good.

We are trying to meet the problem of the continual increase in the size of the *Journal* by urging the editorial board and authors to effect some condensation. We say that only papers that make a "substantial contribution" should be accepted. However, this is a difficult policy conceptually since almost every manuscript makes some contribution. You never know what a given paper might lead to in the future. For example, right now we will not take descriptions of the same enzyme from another source if the enzyme has already been well described from one source. But some day the comparative biochemistry of fifteen of these enzymes might lead to some very interesting results. It's very difficult to cut down on the number of papers. Fortunately, if a paper is declined, this journal is not the only journal available to the author. In other words, there are other places to publish. The problem is that now a lot of the granting agencies care very much where you publish, so we're under even more pressure to publish the manuscripts that are submitted.

I'm very sympathetic with the author who has done a very nice piece of work but whose paper is not accepted because it doesn't fit these criteria. Frankly, I have the same problem in my own work. I will not send a manuscript to the *Journal of Biological Chemistry* even though the study is of interest if I feel that it does not clearly fit the criteria that are being used or if I think that the manuscript is more suitable for another more specialized journal. Yet that doesn't mean that the work is not significant.

BOHNING: What kind of an acceptance rate do you have?

TABOR: I think it runs around fifty-five percent. That's a somewhat misleading figure, in the sense that only about five to seven percent of our papers get accepted with no changes at all. Most of the time they go back to the author with—I like to believe—very good suggestions, and it's a better paper when it comes back. Even if it's declined again, it may eventually get accepted by this journal or another journal, and be a better paper. Unfortunately, some authors do not critique their manuscripts adequately before the first submission, and depend on the editorial reviews for this purpose.

We used to have a handling charge of twenty-five dollars. We spend a lot of time and money on handling papers from all over the world, so some journals have a handling charge, and we did for a while. Even authors of declined papers were charged this fee. We finally gave up handling charges because the overhead for collecting the twenty-five dollars, especially from abroad, was too great. Once after a paper was declined, I received a letter from the author saying, "This was not a very good review. It wasn't worth twenty-five dollars. In fact, it wasn't worth twenty-five cents. Please send me a check for twenty-four dollars and seventy-five cents."

It is true that we do perform a very real service, which I think a lot of our authors have appreciated and some have written to me thanking the editors for their helpful comments and suggestions. Especially those authors who are isolated and don't have any colleagues to go over the paper carefully really depend on our review. Even though we are glad to perform this additional service to the biochemical community, it is a little unfair to the editors or reviewers for authors to ask them to review a paper that has not been adequately critiqued before submission. I do want to emphasize that we try very hard to help the authors, and to appreciate their problems.

BOHNING: Has the number of submitted manuscripts increased steadily?

TABOR: Yes. As I said, the increase is about eight to ten percent a year, which is very interesting in terms of the whole grant question. The acceptance rate is hard to evaluate because there's a lot of prejudgment about what manuscripts authors send to us. Most of the manuscripts that we receive are of higher quality than what some other journals might get. Obviously, the very prestigious journals, like *Biochemistry* and others, get the same quality papers we get. But if you go to some of the more general journals, this is not the case, and therefore you cannot compare acceptance rates for different journals.

BOHNING: In the same vein, John Edsall did comment that at one point there was a "recurrent desire for a smaller and more highly selective journal that would publish only the papers of exceptional interest and importance." You sort of answered that by saying how you determine what's acceptable.

TABOR: That's right. Coming back to what you were asking before about why we are getting more papers, I think one reason is that, as we discussed earlier, techniques are so good that you can press a button and get a spectrum. You can publish a paper that would be much better than what you could have written in 1935, for example.

The other reason is that people are under much more pressure to publish. Thus, authors may now publish material that they know is good, but that they would never have gotten around to publish; it would have remained in their notebook. If they know that renewal of their grant is coming up, they are more likely to publish this material. It is also true that some authors divide their papers, or publish some minor modification and submit each part to a different journal.

Coming back to John's point, we've had a lot less pressure for splitting the *Journal* in recent years. There are several reasons for this change. In previous years there was real pressure for a division based on discipline. Now, however, it is much more difficult to categorize most manuscripts into specialized divisions. The findings and techniques are useful to everybody. In other words, someone working in pharmacology on an adrenergic factor will be using DNA techniques to clone the receptor, and the results would have broad interest. There are still highly specialized areas, but in general it's hard to divide the papers by categories in terms of establishing separate journals.

Although some say that we should just publish the most exciting papers, I feel that this type of a decision, based on a value judgment, would be impossible and unfair. Some of our critics say that if we didn't publish all the "junk," we wouldn't have as big a journal. What they define as "junk" might be what someone else considers to be very important material. I think we lose sight of the fact that, particularly with grants being the way they are now, if you're only going to aim at the most exciting things, you are not going to do the solid work that very often leads to the most exciting results. I think it's very important conceptually that we don't push the concept of "exciting" too hard.

[END OF TAPE, SIDE 14]

BOHNING: You made the comment earlier, when I asked about the *JACS* publication, that there weren't any communications at that time in *JBC*.

TABOR: There were for a while, and they stopped. Presumably at the time I published our *JACS* article there weren't any communications in the *JBC*.

BOHNING: Referring to John's paper (47), he talked about how difficult it was to even get communications from people at one point when he was editor. He said that under your editorship there's been an active encouragement of communications. They are now in the front instead of the back, and things like that.

TABOR: We're still doing that, and we're now trying very hard to give even faster service to attract the communications. We have an announcement in the *Journal* coming out in the April 25th issue about this. Here again, you get into this very difficult issue that what's very exciting to an author may not be to a reviewer, and you have to have very good insight to know what is "exciting" (and suitable for the Communication section).

There's hardly a manuscript I get that doesn't have an accompanying letter saying, "I hope you will give this your most careful and rapid consideration. I wouldn't ask for this except this does happen to be an extremely important piece of work." I think the author really means it. This just indicates how different people have different views on what is "exciting", and how difficult it is for the editors to make that kind of judgment. We're getting enough communications, but we'd like to get some that are perhaps even more "exciting." That's a very difficult thing conceptually, and one that I'm not entirely happy with.

BOHNING: Another comment from John's article (47) was that one possibility is to put documentation that is of high interest to a few people but of very little interest to most people in some remote repository.

TABOR: Yes, we've done that in the past. However, readers have found that material in a distant repository is too difficult to get. Then we went over to miniprint, which is the format used by the *Oxford Dictionary*. We did that for a long time. We put information at the end of the article in miniprint. This material could be read with a magnifying glass. That was really quite good, and I was in favor of it. The problem is that, since the miniprint is reproduced photographically directly from the authors' copy, the material is not in the database, and thus when you use the CD-ROM, the material is not searchable. We've given up miniprint for that reason.

You could argue that, except for what I was just saying, the CD-ROM could contain the supplementary material, even though it wouldn't be searchable. That's something for the future. I think most of our associate editors feel that the large size of the *Journal* makes reviewing this amount of extra material problematic. I have mixed feelings about it. I think most of our associate editors feel that editors and reviewers did not review the supplementary material as carefully as the regular text of an article.

Returning to the question of depositing supplementary material in a repository, there are technical and administrative problems involved in a repository. Readers not only have to write for the deposited material, but somebody has to pull it out and make a Xerox copy or make a microfilm, none of which is inexpensive when you think of the cost of labor. These are interesting technical questions that I think are worth pursuing in the future. With the different technical developments, they might be feasible. This repository material could be deposited in a bank that you reach by e-mail and perhaps would not even be technically part of the journal.

There are so many variations you can think of that would be feasible. Obtaining the deposited material by e-mail would be comparable to the way we retrieve a sequence from GenBank. It's something that I think we'll be actively pursuing.

Again, we come back to what we were discussing before about people being used to computers. I think the community is still not used to using e-mail enough to completely cooperate and accept this. That's just a matter of a short time, so there's no reason why we couldn't deposit that kind of supplementary material in the future in a central depository to be accessed by e-mail.

BOHNING: A year after you became associate editor, Stein became ill.

TABOR: Yes, that was a very sad picture. I don't know how much you know about it.

BOHNING: That's all I know.

TABOR: Yes. It was extremely sad. He developed the Guillain-Barre Syndrome, which is an ascending paralysis, somewhat like polio. Usually it reverses and the patient gets better, but in very occasional cases, it doesn't. He was completely paralyzed from about the mid-waist down for quite a few years, which was a very sad situation. In fact, it was a little higher than his waist. I think it even included his arms. This was very, very sad because he was an extremely competent and good person. He was particularly unhappy, he said, that this had to happen to him just when protein science was developing with techniques and so many exciting things to do, and there he was completely bedridden. That was a terrible situation. Just for the record, I might mention that his wife [Phoebe Stein] was just wonderful during that period. She kept him going over the years and she did a wonderful job. As you must know, Dr. Stein was an excellent protein chemist, and he was equally superb as the Editor. He continued the tradition of John Edsall for integrity, fairness, and service.

BOHNING: In 1971 you became editor-in-chief, so I'm assuming that Stein then realized that he could no longer continue. Is that when he retired or resigned?

TABOR: He became sick and then, effectively, he just couldn't do it anymore, which was very sad. I obviously wanted him to stay on with the title, and he was willing to do this for about two or three years. Eventually he insisted that he literally retire. It was very sad when he realized that his paralysis was irreversible. Very sad.

BOHNING: When you took over the *Journal*, what goals did you have in mind? What did you see as the things you wanted to accomplish back in 1970?

TABOR: It was very simple, and there was nothing special. I didn't have any exciting goals except to publish the best biochemistry in the fairest way, and to keep running the operation the way John Edsall and Bill Stein did, and to do the best I could. As new publication techniques came along, I wanted to use them; not with a broad concept of changing the mission of the publication, but just to do what is best for science and biochemistry. That's still the way I feel about it.

BOHNING: That's a very good testimonial to you and to the *Journal*. That's a good way of summing that up.

TABOR: It has all been very exciting, especially with the new developments in publishing. Basically, I think the important thing is the publication of good science.

BOHNING: Am I correct that *JBC* is now the most cited journal? I know in *Chemical Abstracts* they publish a list of the "one thousand most cited journals in *Chemical Abstracts*."

TABOR: I have to look at the figures. I think it depends on which list you look at. Certainly the *Journal of Biological Chemistry* is one of the most highly cited journals in all of the lists. Of course, to be fair, one has to correct the figures for the fact that we publish so many papers. One needs to divide the number of citations by the number of papers published per year. I ought to get those figures out and send them to you. I have the feeling that depending on how you phrase the question, we may be second or third, excluding review journals. We're certainly way at the top. For example, I wouldn't be surprised if for certain periods, *PNAS* or *Cell* (or perhaps *Nature* or *Science*) might have a higher citation listing.

It is very interesting to me that the *Journal of Biological Chemistry* is the most cited non-clinical journal in the two most prestigious research-type clinical journals, namely the *New England Journal of Medicine* and the *Journal of Clinical Investigation*.

People sometimes say, "Well, ninety percent of these articles in the scientific literature are never quoted." These statements are often made at meetings where it is hard to respond. [laughter] We've looked through the *Science Citation Index* for each article in some representative issues of the *Journal of Biological Chemistry*, and have found that every article is quoted. They are quoted to different degrees, but they're all quoted.

BOHNING: Do you have anything else you want to add at this point?

TABOR: No. I didn't think I could talk this long. [laughter] I am particularly pleased that we spent some time on the material we have discussed in the last part of our conversation. I have the feeling that we spent a little too much time from my point of view on the early part, and should have spent more time on the *JBC*. The early part applies to me personally, and I think that is of less interest to history or to science than the last part except in a very indirect sense. I'd like to believe that my contribution to the *Journal* is the thing that really has had a social impact. I'd like to believe my research has too, but that's much more fun for me. [laughter] In fact, it's all fun for me, as you gathered.

BOHNING: That comes through very clearly. Thank you very much for spending the time with me.

TABOR: Thank you for coming down. My goodness! It's all of interest to me, and you have to listen to it. [laughter]

BOHNING: I found it enjoyable.

TABOR: I haven't mentioned that in medical school I was quite interested in psychiatry and had some psychiatry courses as part of the routine. There was a cartoon where someone says to a psychiatrist, "How can you spend all day listening to all this stuff?" The response was, "Who listens?" [laughter] Anyway, I hope I haven't bored you to death.

BOHNING: No, not at all. I've enjoyed it, and thank you again.

TABOR: Well, thank you.

[END OF TAPE, SIDE 15]

[END OF INTERVIEW]

NOTES

1. Paul de Kruif, *Microbe Hunters*, New York: Harcourt, Brace and Company, Inc., 1926.

2. Hans Zinsser, *Rats, Lice and History*, Boston: Little Brown and Company, 1934.

3. Sinclair Lewis, *Arrowsmith*, New York: Harcourt, Brace and Company, 1925.

4. Arthur Kornberg, *For the Love of Enzymes: The Odyssey of a Biochemist*, Cambridge, Massachusetts: Harvard University Press, 1989.

5. Jerome Karle and Isabella Karle, interview by James J. Bohning and David van Keuren at the Naval Research Laboratory, Washington D.C., on 26 February, 15 June, and 9 September 1987; Philadelphia: Chemical Heritage Foundation, Transcript 0066.

6. Lawrence J. Henderson, *The Fitness of the Environment: An Inquiry Into the Biological Significance of the Properties of Matter*, Boston: Beacon Press, 1958.

7. Meyer Bodansky, *Introduction to Physiological Chemistry*, New York: John Wiley & Sons, Inc., 3rd. edition, 1934.

8. Philip B. Hawk and Olaf Bergheim, *Practical Physiological Chemistry*, Philadelphia: P. Blakiston's Son & Co., Inc., 10th edition, 1930.

9. Herbert Tabor and A. B. Hastings, "The Ionization Constant of Secondary Magnesium Phosphate," *The Journal of Biological Chemistry*, 148 (1943): 627-632.

10. John P. Peters and Donald D. Van Slyke, *Quantitative Clinical Chemistry*, Baltimore: The Williams & Wilkins Co., 1931.

11. James Hopper, Jr., Herbert Tabor and A. W. Winkler, "Simultaneous Measurements of the Blood Volume in Man and Dog by Means of Evans Blue Dye, T1824, and by Means of Carbon Monoxide," *Journal of Clinical Investigation*, 23 (1944): 628-635.

12. See, for example, J. D. Ratcliff, *Yellow Magic: The Story of Penicillin*, New York: Random House, 1945.

13. Dewitt Stetten, Jr. and W. T. Carrigan, eds., *NIH: An Account of Research in Its Laboratories and Clinics*, Orlando: Academic Press, Inc., 1984.

14. Herbert Tabor, "Development of Enzymology," in *NIH: An Account of Research in Its Laboratories and Clinics*, edited by DeWitt Stetten, Jr. and W. T. Carrigan, Orlando: Academic Press, Inc., 1984.

15. Harland G. Wood, interview by James J. Bohning at Case Western Reserve University on 19 January 1990; Philadelphia: Chemical Heritage Foundation, Transcript 0082.

16. see for example *Harvard University Yearbook*?

17. *William Evans, member of the Board of Supervisors of Elections of Baltimore City, et. al. v. Tillye Cornman, et. al.*, 236 U.S. (1969).

18. Bess Furman, *A Profile of the United States Public Health Service, 1798-1948*, Bethesda, Maryland: National Institutes of Health, 1973.

19. Ralph Chester Williams, *United States Public Health Service, 1798-1950*, Washington, D.C.: United States Public Health Service, 1951.

20. Victor H. Kramer, *The national Institutes of Health: A Study in Public Administration*, New Haven: Quinniplack Press, 1937.

21. Herbert Tabor, Herman Kabat, and Sanford M. Rosenthal, "The Chemotherapy of Burns and Shock. VI. Standardized Hemorrhage in the Mouse. VII. Therapy of Experimental Hemorrhage," *Public Health Reports*, 59 (1944): 637-658.

22. Arthur Kornberg, Herbert Tabor, and W. Henry Sebrell, "The Effect of *L. casei* Factor ("Folic Acid") on Blood Regeneration Following Hemorrhage in Rats," *American Journal of Physiology*, 142 (1944): 604-614.

23. Konrad E. Bloch, interview by James J. Bohning at Harvard University on 22 March 1993; Philadelphia: Chemical Heritage Foundation, Transcript 0109.

24. Sanford M. Rosenthal, Herbert Tabor, and R. D. Lillie, "The Local Nature of Acquired Resistance to Trauma," *American Journal of Physiology*, 143 (1945): 402-406.

25. Arthur Kornberg, Herbert Tabor, and W. Henry Sebrell, "Blood Regeneration in Pyridoxine-Deficient Rats," *American Journal of Physiology*, 143 (1945): 434-439.

26. Herbert Tabor and Sanford Rosenthal, "Experimental Chemotherapy of Burns and Shock. VIII. I. Effects of Potassium Administration, of Sodium Loss, and Fluid Loss in Tourniquet Shock," *Public Health Reports*, 60 (1945): 373-381; Tabor and Rosenthal, "II. Electrolyte Changes in Tourniquet Shock," *Ibid*., 60 (1945): 401-419.

27. Arthur Kornberg, Herbert Tabor, and W. Henry Sebrell, "Blood Regeneration in Rats Deficient in Biotin, Thiamin or Riboflavin," *American Journal of Physiology*, 145 (1945): 54-66.

28. Sanford M. Rosenthal and Herbert Tabor, "Electrolyte Changes and Chemotherapy in Experimental Burn and Traumatic Shock and Hemorrhage," *Archives of Surgery*, 51 (1945): 244-252.

29. Herbert Tabor and Sanford M. Rosenthal, "Body Temperature and Oxygen Consumption in Traumatic Shock and Hemorrhage in Mice," *American Journal of Physiology*, 149 (1947): 449-464.

30. Herbert Tabor, "Diamine Oxidase," *The Journal of Biological Chemistry*, (1951): 125-136.

31. Sanford M. Rosenthal and Herbert Tabor, "An Improved Colorimetric Method For the Estimation of Histamine," *Journal of Pharmacology and Experimental Therapeutics*, 92 (1948): 425-431; R. Carl. Millican, Sanford M. Rosenthal, and Herbert Tabor, "On the Metabolism of Histamine. A. Urinary Excretion Following Oral Administration. B. Conjugation *in vitro*," *Ibid*., 97 (1949): 4-13; Herbert Tabor and Erich. Mosettig, "Isolation of Acetylhistamine From Urine Following Oral Administration of Histamine," *The Journal of Biological Chemistry*, 180 (1949): 703-706.

32. Herbert Tabor and Osamu Hayaishi, "The Enzymatic Conversion of Histidine to Glutamic Acid," *The Journal of Biological Chemistry*, 194 (1952): 171-175.

33. Herbert Tabor, Alan H. Mehler, Osamu Hayaishi, and Julius White, "Urocanic Acid as an Intermediate in the Enzymatic Conversion of Histidine to Glutamic and Formic Acids," *The Journal of Biological Chemistry*, 196 (1952): 121-128.

34. Herbert Tabor, Milton Silverman, Alan H. Mehler, Floyd S. Daft, and Hugo Bauer, "L-Histidine Conversion to a Urinary Glutamic Acid Derivative in Folic-Deficient Rats," *Journal of the American Chemical Society*, 75 (1953): 756

35. Celia White Tabor, Herbert Tabor, and Sanford M. Rosenthal, "Purification of Amine Oxidase from Beef Plasma," *The Journal of Biological Chemistry*, 208 (1954): 645-661.

36. Markus Guggenheim, *Die biogenen Amine und ihre Bedeutung fur die Physiologie und Pathologie des pflanzlichen und tierischen Stoffwechsels*, Basel: Karger, Fourth Edition, 1951.

37. J. E. Seegmiller, M. Silverman, H. Tabor, and A. H. Mehler, "Synthesis of a Metabolic Product of Histidine," *Journal of the American Chemical Society*, 76 (1954): 6205.

38. H. H. Hiatt, M. Goldstein, and H. Tabor, "Urinary Excretion of Formiminoglutamic Acid by Human Subjects After Antifolic Acid Therapy," *Journal of Clinical Investigation*, 37 (1958): 829-832.

39. Herbert Tabor, Sanford M. Rosenthal, and Celia White Tabor, "The Biosynthesis of Spermidine and Spermine from Putrescine and Methionine," *The Journal of Biological Chemistry,* 233 (1958): 907-914.

40. Herbert Tabor, Sanford M. Rosenthal, and Celia White Tabor, "The Role of Putrescine and Methionine in the Enzymatic Synthesis of Spermidine in *Escherichia coli* Extracts," *Journal of the American Chemical Society*, 79 (1957): 2978-2979.

41. D. Kaiser, H. Tabor, and C. W. Tabor, "Spermine Protection of Coliphage Lambda DNA Against Breakage by Hydrodynamic Shear," *Journal of Molecular Biology*, 6 (1963): 141-147.

42. Reed. B. Wickner, Celia. W. Tabor, and Herbert. Tabor, "Purification of Adenosylmethionine Decarboxylase from *Escherichia coli* W: Evidence for Covalently Bound Pyruvate," *The Journal of Biological Chemistry,* 245 (1970): 2132-2139.

43. Herbert Tabor, Celia White Tabor, and E. W. Hafner, "Convenient Method for Detecting $^{14}CO_2$ in Multiple Samples: Application to Rapid Screening For Mutants," *Journal of Bacteriology*, 128 (1976): 485-486.

44. Herbert Tabor, Celia White Tabor, and F. Irreverre, "Quantitative Determination of Aliphatic Diamines and Polyamines by an Automated Liquid Chromatography Procedure," *Analytical Biochemistry,* 55 (1973): 457-467.

45. Elbert A. Peterson and Herbert A. Sober, "Chromatography of Proteins. I. Cellulose Ion-exchange Adsorbents," *Journal of the American Chemical Society*, 78 (1956): 751-755; Herbert Sober, Frederick J. Gotter, Mary M. Wyckoff, and Elbert A. Peterson, II. "Fractionation of Serum Protein on Anion-exchange Cellulose," *Ibid,* 78 (1956): 756-763.

46. Celia White Tabor and Herbert Tabor, "1,4-Diaminobutane (Putrescine), Spermidine, and Spermine," *Annual Review of Biochemistry,* 45 (1976): 285-306.

47. John T. Edsall, "The Journal of Biological Chemistry After Seventy-five Years," *The Journal of Biological Chemistry,* 255 (1980): 8939-8951.

APPENDIX

i. It was pointed out to me after this interview that, of course, the Department of Agriculture had an excellent research program with laboratories in Beltsville and a number of centers elsewhere in the country. In addition the National Bureau of Standards in Washington had some excellent laboratories.

ii. For more details and confirmation of this story see the attached two pages that are copied from a book entitled, *Crossing Boundaries. Biological, Disciplinary, Human*, by A. Baird Hastings (edited by Halvor N. Christensen), The Four Corners Press, Grand Rapids, Michigan 1989.

iii. The policy of *PNAS* changed in 1996 to give more authority to the editor and to the scientific board.

iv. See the following addendum for the more recent and dramatic developments in electronic publishing.

Addendum, September 21, 1998
On-Line Publication of the *Journal of Biological Chemistry*

At the time of this interview in 1993 our use of CD-ROM techniques for electronic distribution of the *Journal of Biological Chemistry* was very exciting, and was at the forefront of scientific publication. I indicated this clearly during the interview. Putting the *Journal* on the CD-ROM was no small feat at the time, and involved the cooperation of Waverly Press (now Cadmus Journal Services), Peter Goldie of Lightbinders, and the Bethesda staff, particularly Chuck Hancock and Barbara Gordon. We also had the cooperation of the Finance Committee, the Publications Committee, and the Council of ASBMB. We were also fortunate that the finances of the *Journal* were good enough to permit laying out the costs involved. This good financial status was due to the careful operation of the *Journal* in past years under the supervision of Chuck Hancock, Barbara Gordon, and in the more distant past of the late Robert Harte.

The *Journal of Biological Chemistry* was the first biochemical journal and probably the first biological journal to use the CD-ROM technique. In the intervening years, however, the developments in electronic publication of scientific literature have progressed at such a dramatic rate that it makes the CD-ROM usage seem archaic. Therefore I feel that this addendum is a most important addition to the interview, particularly with regard to the *Journal of Biological Chemistry* and to electronic publication of scientific literature.

I am particularly proud of the fact that the *Journal of Biological Chemistry* was markedly ahead of other biological journals in this development, although it is true that some of the Physics journals had already started the use of this technique. I feel that it is

particularly noteworthy that most of the other biological journals were very reluctant to introduce the electronic versions since they were concerned with the possible financial loss that might occur if all of the subscribers suddenly gave up their print subscriptions. Indeed, several of the editors of other journals told us that they would not start in this direction until they saw whether we were successful or whether we had a catastrophic failure. I also want to note that our success was particularly noteworthy since we are the largest biomedical journal with over thirty-three thousand pages per year, and many critics said that a journal of this size could not undertake such innovative developments.

The history of this development is obviously of interest. It all started with the CD-ROM. Already at that time some libraries such as the one at the Uniformed Sevices University in Bethesda, had put other CD-ROM disks in a carousel on line, so that researchers at libraries at other locations in the Washington area could use them. I mentioned this to the Director of the NIH Computer Center (during a casual conversation on a bus going to an Awards ceremony for Dr. Kirschstein at the HHS Building downtown), and he agreed to try our CD-ROM on the NIH network, although I do not think there was a carousel arrangement. This was somewhat successful, but quite limited compared to what we have now.

I mentioned this to Dr. Simoni, one of our associate editors, and I suggested that he discuss this with his librarian to see if they would also try this out. He did mention this to Michael Keller, the Stanford University Librarian, and this started the ball rolling. Mike Keller said that they felt that the CD-ROM was already outdated, and that he and his staff were interested in electronic publishing via the Internet. They considered this a challenge and a research project in scientific communications, and were clearly most anxious to carry out a cooperative venture. This was a most fortunate coincidence since the Stanford Library staff was obviously very competent and dedicated, and Dr. Simoni was both interested and located on the Stanford campus. Dr. Simoni was a very enthusiastic participant in the project and his location at Stanford, as well as his ideas and interactions with the Stanford group and with us, proved to be most important.

It was very important that Chuck Hancock, Barbara Gordon, and I had the background with the CD-ROM because there was considerable discussion at the meeting of the Finance Committee and the Council of the ASBMB on who would be best for this operation. In addition to Stanford the company that was doing the CD-ROM, as well as Waverly Press (now Cadmus Journal Services) could be considered for the operation. Dr. Hammes, the President of the ASBMB at that time, who was also Provost of Duke University and Editor of *Biochemistry*, felt strongly that it would be best to have a university association rather than a commercial partner. Chuck, Barbara, Bob Simoni and I agreed with Dr. Hammes that this would be best, and the final decision was to be associated with Stanford.

The Stanford group under Mike Keller was very dynamic; special mention should be made of John Sack who has been most important in getting all of this done. Chuck and Barbara made several trips to Stanford, and the plans proceeded rapidly. I

am also glad to say that Cadmus Journal Services was most cooperative, as one important facet in the operation is getting the computer tapes in the correct format and delivered in a timely manner to the Stanford Library for transfer to their computer. Of course, I should add that there were a number of technical developments in printing that permitted all of this, which would not have been possible in past years. All printing is now done from the computer output (and not from hot type), and the computer languages used became compatible with easy transfer to the Internet.

Many decisions had to be made along the way, including how we would arrange the subscriptions, et cetera. There were a number of meetings, and the final decision was that any institution would only have to have one electronic subscription, and everyone at the institution would then have very easy access with little or no need for passwords, et cetera. My feeling and that of Chuck and Barbara and Bob Simoni was and is that we wanted this development to be successful, and we did not want to worry much about the finances. Fortunately so far we have been proven correct. The financial aspects are still uncertain however, since we realize that these will become more important as our readers become more accustomed to using the On-Line *Journal* rather than the printed version.

In addition to the institutional subscriptions we also have personal subscriptions. Recently the ASBMB agreed to give all members of ASBMB in the US and in foreign countries free access to the On-Line *Journal*. Numerous other innovations have been developed recently or are being considered such as e-mail notification of articles in the *Journal*, articles that are relevant to a given set of keywords of interest, et cetera. We will probably also have some arrangements for "pay per view".

I do not wish to spend more time on the technical aspects involved in the On-Line version, but I do want to summarize the enormous impact this development has had. Now, essentially all of the biomedical journals have an On-Line edition, and many have adopted our policy of easy access to the On-Line version via an institution subscription. (Some journals or their financial offices are still concerned about the eventual financial impact, and some journals feel that the On-Line version should return some profits to the *Journal* or to the parent company or Society.)

Of course, the most exciting part of the On-Line development is its contribution to scientific communication, and to the use of the *Journal* by research scientists all over the world. The On-Line version offers many advantages that are not possible with the printed version. Some of these are as follows (but there are many more advantages too):

Instant availability in one's laboratory, home or office.

Easy retrieval of articles of interest by keyword or by author.

Hyperlinking from the article being read to the text (or MedLine abstract) of articles cited in the bibliography. From the MedLine article one can get other

similar articles through the Entrez system. In this connection I want to emphasize the enormous cooperation and help given to us by Dr. David Lipman of the National Library of Medicine.

Availability of the published articles all over the world at the same time that the *Journal* is released in Baltimore by the printer. Indeed the electronic version of the *Journal* is released one or two weeks before the printed version. Our statistics show that each week nineteen thousand computers in over seventy-five countries access the On-Line version, and access over twelve thousand articles per day.

Finally, I want to emphasize again that this enormous development was only possible because of the cooperation and competence of many people. Chuck and Barbara spent many hours and trips in the development of this project. I have already mentioned the great importance of the Stanford group (also called Highwire Press), and the very effective interaction of Mike Keller, John Sack and their associates with Chuck, Barbara, and me, and with Bob Simoni. Their collective input and that of the various associate editors have been invaluable.

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