



**Mount
Sinai**

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Interview with Horace L. Hodes, MD by Albert S. Lyons, MD

October 9, 1974

LYONS: This is October 9, 1974. I'm now interviewing Dr. Horace Hodes in his office in the Annenberg Building on the 17th floor. Horace, when did you first come to Mount Sinai?

HODES: I came here on July 1, 1949. I'd never been in this hospital up until about two months before that date. The way I happened to come here was the following. Dr. [Edwards A.] Park, who was my professor at Johns Hopkins and a very wonderful man, had met George Baehr [Director of the 1st Medical Service at The Mount Sinai Hospital] somewhere at a high level meeting. They were both engaged in trying to engineer big changes in medical care in the United States. They were both, as you know, public-spirited people. And at this meeting it transpired that George Baehr was on a search committee looking for a replacement for Murray Bass [Director of Pediatrics, 1942-48] in the Department of Pediatrics. He told Dr. Park that they had decided at Mount Sinai to have a full-time head of the Department of Pediatrics. And in fact, this was the first really completely full-time clinical department the hospital was going to have. [M. Ralph Kaufman, MD, Chief of Psychiatry was the first full-time director of a Mount Sinai service. Ed.] Dr. Park lived in New York for a short time; he went to Columbia Medical School and had actually started into practice. He knew something about the scene in New York that was a good many years old. And we talked about New York in general, and I must say that he advised me not to come to The Mount Sinai Hospital. I'd turned down a number of chairmanships at that time, one at the University of Utah, one at University of Colorado and one at Stanford, which I really regretted. And the real reason I turned down the Stanford position was that it paid the great salary of \$7,000 a year. And I was already making the magnificent salary of \$15,000 a year at Johns Hopkins. So while I liked San Francisco, I decided that wasn't what I should do. At any rate, Dr. Park said that Mount Sinai was a great clinical place but that the research contributions were much smaller than an institution like that should have made, and that the real reason was that there wasn't a medical school or university connected with it. He went on to say very nice things about lots of people in the hospital that he knew and, in particular about Murray Bass and Bela Schick [Director of Pediatrics from 1923-42], who were close friends of his.

Well, I—at that time, my major job at Hopkins was to run the contagious disease hospital connected with it. I was Director of Sydenham Hospital and Associate Professor of Pediatrics at Hopkins. And I thought that contagious disease hospitals should be closed at that point and that they had outlived their usefulness, if they had ever had one and I was decided, I had decided to either leave that hospital and spend all my time at Johns Hopkins or to look for a place elsewhere. As I say, I had offers that you've heard about. I came to Mount Sinai to interview with the Board of Trustees; some of you will recognize the custom of inviting prospective candidates to luncheon at the Harmonie Club. I came to the luncheon and I didn't know anybody there except George Baehr. Murray Bass and Bela Schick, whom I did know slightly, were not there. And it was

quite a luncheon, I remember. I got very sleepy at that. Martin Steinberg [MD, Director of the Hospital from 1948-69] was there. I'd never met Martin but we both came from Philadelphia and we had mutual friends and acquaintances. Mo Kaufman was at the meeting, as I remember, and I guess he was psychoanalyzing me, I'm not sure. But I recall George Baehr and one of the Trustees, whose name I don't remember, doing most of the talking. I left the meeting feeling as if I hadn't said anything and I hadn't heard anything, so went home assuming I had a very pleasant lunch.

AL: They didn't promise anything or offer anything or give you any details?

HH: Well, I couldn't tell. They discussed things in great detail, but it was always with one of the other people in the room and not with me. And they never got around to asking whether I was really interested in coming or what the job entailed. And with Dr. Park's negative input, I really had no intention of coming here. And I didn't think I'd been asked. Well, I got home and got a call—there was a telephone call waiting for me from Martin Steinberg, and he said they were very interested in having me come back.
[chuckles]

And subsequently, after I'd heard from Martin, a year or so later, after I got to know him, that the impression I'd given was that I never said a word and that he said that didn't disturb him, because he had heard I was very quiet. All I can say right now is that there wasn't any choice [chuckles], I didn't have a chance to say anything. I wasn't asked any direct questions. I guess I really was kind of negative about the whole thing.

Well, I did come back, for various reasons. One is that George Baehr had talked with Dr. Park again, and contrary to my idea of it, apparently my being quiet made quite an impression on him; they wanted me to come back. George asked Dr. Park if he wouldn't try to persuade me to come back. Dr. Park is a very honest man and he said, well, I think maybe I ought to come back. And I then came back and looked at the department and I was a lot more impressed than I thought I was going to be. I think the department had, in all honesty, contributed very little to research and advancing things, but the patient care was very good, I thought. There was no newborn service, which I think in general, makes a department of pediatrics pretty handicapped.

AL: But they [the Hospital] had no obstetrics.

HH: No. But they'd said—this was 1949, that same May, I should think, or June—May, I rather think—and apparently beginning to talk about having an obstetrical department. And the Trustees, at least, said that they had just about decided we should have an obstetrical department. And they—it was promised that if I came, that I would have a hand in choosing the professor of obstetrics and that I would have something to do with building a newborn service. And I must—I recall now that the plans were already made for a newborn service. And I did have a chance to modify the nursery. [Plans for an obstetrical service were begun during World War II – ed.]

For example, I'd worked during World War II with the effect of humidity on the spread of bacteria, and temperature and humidity. And my ideas on this were

incorporated into the nursery and the nursery was set up with temperature and humidity control. And it was about the first nursery, I think, that was ever built, in this country at least, with that in mind. It still functions very well. They keep the relative humidity around 40 percent and the bacteria stay up in the air under those conditions and virus droplets, a minimal amount of time. At lower humidity, the circulation of particles, including viruses and bacteria, may go up four or five times. And work done by Robertson in the war and by [Francis] Schwenker [Chairman of Pediatrics at Johns Hopkins after Parks] in the Navy indicate this was related to spread of some infections, streptococci. So I got that input and I went to the planning of the nursery.

I was then offered a very attractive change in the situation at Hopkins, and I think I was really going to stay in Baltimore. I was asked to come up again and I did and I met some of the people in the Department of Pediatrics, and I was really quite taken with them. They were people like Ralph Moloshok, for instance; Sam deLange, I remember was a delightful person; he died at an early age. And Al [Alfred] Florman, whom I'd known as a student and an intern at Hopkins in the Department of Medicine and Pediatrics; and Hy [Herman] Anfanger, who was with us at Hopkins for a year. So I really was taken with the department more than I thought I was going to be. To make a long story short, the arrangements were made and I came here in July of 1949, which is now over 25 years ago.

AL: In this time, Horace, what would you judge to be your important contributions to research with, let's say not the institution, but to research?

HH: Well, I think partly—work I'd partly begun at Johns Hopkins and before that, the Rockefeller Institute. I think that I discovered and laid down the principle that if one is going to try to make a virus vaccine that was non-infectious but antigenic, that we would have to put into it some measured agent that would destroy the infectiousness without destroying the antigenicity. And I had worked on rabies for a while at the Rockefeller Institute and made rabies tissue culture virus. Using ultraviolet light, I showed with a physical agent that you could measure, that there was a point between destruction of infectiousness and preservation of antigenicity that would make a successful vaccine. And we made a successful rabies vaccine and that ultraviolet method, which I invented - perfected more - I finally, just before my last—my first few months here, was used in making all kinds of vaccines, influenza vaccine and so on. And—

AL: Had that ever been done before?

HH: No. This was the first killed vaccine that was effective of any kind. That method, incidentally, was used by Salk in putting formaldehyde with polio virus grown in tissue culture—tissue culture work based on what Andrews [RV Andrews?] did. He deserves all the real credit, I think—most of the real credit for this accomplishment. And applying the-- Salk would put the formaldehyde into a measured [unclear] and then neutralize it. So he got to the point where it was not infectious. It wasn't complete, as you remember. There—there were not enough safeguards taken because the sampling to see if the virus was not infectious was too small. We then had the Cutter incident in which, because of

the small sampling, the virus seemed to be all dead and was not. And they then had to take bigger samples. Well, I think that was one of the accomplishments since I came here.

I think my greatest accomplishment in my life was when I was a first-year medical student. A fellow classmate and I, Milton Rapoport, discovered the main action of vitamin D. I'll bet you didn't know that.

AL: No, I didn't.

HH: We were first-year students and it was postulated that vitamin D, which had been discovered a few years before—I'm going back to 1927—that the way that vitamin D worked was that it stimulated the parathyroid glands and these then put out parathormone, blood calcium went up. Looking at this—there wasn't any truth to this but we were pretty naïve and we accepted this, and we thought we'd put an experiment to the test. So we took dogs and took out their thyroids and parathyroids, so we had calcitonin mixed in with this, we didn't know. And then we gave the dogs great big doses of vitamin D [several words unclear] and the blood calcium did not rise, so that the theory looked as if it was correct. But the—it was my turn to feed the dogs over the weekend and I noticed that the dog hadn't—the two dogs hadn't drunk any milk. They were too sick from our operation. So I felt kind of sorry for them and thought they might need some nourishment. I was naïve as that, and I fed these two dogs by stomach tube, and the next morning, the blood calcium was practically normal. So it became quite clear, and we published this work the next year in the *Journal of Biological Chemistry*. And the next year, we showed the secondary action of vitamin D in its effect on the kidney for phosphorous excretion.

AL: Well, spell that out, what that experiment showed was—

HH: It showed that the main action of vitamin D was to increase the absorption of calcium from the intestine, and that's still its major action.

AL: What would you say—

HH: ...it also shows on my part that I worked on Saturday [chuckles]—Saturday or Sunday.

AL: What would you say about your contributions here? That is to say, what do you think you're happy with or not happy with in relation to the Department of Pediatrics?

HH: Well, if we keep following the research and my own personal thing. I think we discovered that, first of all, the antibody in milk, the major antibody in milk was not the same as serum. We were studying antibodies against polio viruses by then. And we invented a method, which was known as the radioactive virus paper method. And indeed we showed that if you took a culture of radioactive poliovirus and put a strip of filter paper in it, very soon the virus would come to be distributed uniformly throughout the paper. And if you would put in a band across that strip, a small amount of serum which

contained antibody, then the spread of that virus was retarded, because what happened clearly was there was a union of virus and globulin antibody, which then moved much more slowly. And this was reproducible very accurately if you used the same filter paper and cut it in the same direction. If you cut it this way, it'd be one thing. If you cut it the other way, it would be different. This really amounts to a sieve. It was really a gel kind of movement. Paper can be looked on as a gel. The fibers are—act in the same way with the same—under the same conditions. If you have the same amount of protein in it and the same room temperature and so on, it would be very uniform. And if there was antibody, then that spread was retarded.

Well, it was interesting; the antibody in breast milk at that time and for reasons that I won't go into, but it was postulated that the antibody in breast milk was the same as serum. And we decided to test this and when we put breast milk across the paper, we would get—the virus would spread all the way up the paper but it would be non-infectious. So we decided right away that, whereas with serum antibody you would retard the spread of virus, and the top of the strip would be non-infectious and had no virus, but the milk antibody made the virus non-infectious. But the particle that you made by joining the virus had different properties. That is, it moved on this paper. It turns out that this was gamma A globulin. And I think that we showed that it neutralized the virus and so on. I think this was the first demonstration of that. Then we went on to find that there was gamma A polio and Coxsackie and other virus antibodies and bacterial antibodies, not only in breast milk but also in serum. And I think this was the first demonstration that gamma A antibody in serum did anything, because the antibody was just being identified. And we had shown that it had a function in breast milk and in—

AL: This was at Hopkins.

HH: No, no. It was all done here.

AL: Oh, this was done here?

HH: Yes. This was done in the early '50s, '52, '54; something like that. And it was very useful. You can do this—you can test for this method with a tiny amount of serum and so on. But we did show that it was a different antibody. And then we showed that, in general, women have more gamma A antiviral antibody in their blood than men do. And that's true for a whole range, about two-thirds of women have this property and one-third of women don't. They always have more "antiviral" gamma A antibody than men. Whether this is connected with their—with the greater resistance in general of women to infections, I don't know, and begins in early life. A young child, the males and females are different. A third of the women looked like—a third of girls look like boys. The other two-thirds are—almost two-thirds-- have twice, three, four times as much. The gamma G antibodies are—these are specific antibodies, not total antibody—is the same in the two sexes.

AL: Where did you get the support for this work?

HH: Well, almost all of it was from—was from the NIH [National Institutes of Health]. I had a little money from here but we got two and three grants a year. I still have one fairly large one to this day.

AL: On this viral—on virology.

HH: Neurology and immunology and things like that. We supported a large part of the department's activities, always from outside sources. Right now, for example, we have three-quarters of the money that's used to run this department -- this is partly clinical, too -- if you exclude the house staff, 76 percent of it comes from outside sources and only 24 percent—

AL: By outside sources you mean what?

HH: NIH, cancer—Holyoke Foundation originally, and later, the national foundation—

AL: These are for investigation.

HH: Investigation and some for service, like the Adolescent Clinic is run largely from grants by the state and federal government.

AL: Did you ever work at the NIH?

HH: No, I never worked at NIH. My training was—specific laboratory training was at the Rockefeller Institute. I was there three or four years [1936-38]. And I was there during the war and overseas in the Rockefeller unit for three, almost—for three and a half—

AL: [sentence unclear].

HH: I—well, too, I left the Rockefeller Institute a couple of years before and had gone back to Hopkins to work. And Tom Rivers—Thomas Rivers, the great virologist, set up the Rockefeller Naval Medical Research Unit and he asked me to join. So I did and I joined that in 1942.

AL: [several words unclear], Horace. Let's see. You were born—

HH: I was born in Philadelphia and I went to college at the University of Pennsylvania and medical school there [Class of 1931]. I was—

AL: What was—what was the time of your birth?

HH: I was born on December 21, 1907. And I was 19 years old, I think, in my first year in medical school when Milt Rapoport and I did this work for—I told you about it. I should have quit then when I was ahead. [chuckles]

AL: And then where did you go after that?

HH: And then I was there two years at the Children's Hospital in Philadelphia—one year and then I went to Hopkins for part of my residency and I was another year at the Children's Hospital. Altogether, I worked in hospitals seven years after I—for seven years after I graduated.

AL: Then what did you do?

HH: Then I was at the Rockefeller for two years.

AL: And that's where you went into the service. Is that it?

HH: No, I went back to Hopkins. I was there maybe two years when—this is now 1942 when Thomas Rivers started the Rockefeller Institute Medical Research Unit, and he joined the Navy. And I joined the Navy and I worked with him. They had some very famous people in the—Richard Shope was the executive—at least one of the senior officers in this unit. Francis Schwenker was the Executive Officer and Wilbur Downs. A lot of people were quite famous. It was mostly—it dealt mostly with infectious diseases, and I had—I specifically was asked to join, I think, because when I was at the Rockefeller Institute, I worked on Japanese encephalitis virus. And I was only one of two Americans who worked on it. But Leslie Webster was my chief at the Rockefeller Institute [several words unclear]. And I was sent out with the idea that they were afraid the Japanese encephalitis virus would break out. So I was in charge of the virus laboratory in the unit. We set up on Guam right after the end of the battle. And in the battle of Okinawa, we began to get reports of encephalitis. So we, I went up there and we brought back material from a child who died. Thomas—Lewis Thomas, whom I forgot to mention, was quite famous, was in that unit. Well, I had—when I was at the Rockefeller, I had worked in the same laboratory as Jordi Casals[-Ariet], who was—who was responsible for looking at—inventing really, the techniques for virus complement fixation. This was a great advance. You could tell the antibody the way you could for complement fixation in syphilis, for example. And I had learned those techniques and I was one of the few people who could do it. So when we went over there to Okinawa, we brought this material back, and we showed in our laboratory right away it was Japanese encephalitis. We suspected it was mosquito-borne and we did isolate the virus from mosquitoes.

And I guess the other thing I did while I was there, which is probably, I would think the best scientific thing I ever did, was we infected mosquitoes as eggs, and they then—we hatched them out and they were infectious. They could transmit Japanese encephalitis virus to mice. This opens up another virus reservoir pool, you see, because the hatching mosquito, the egg can over-winter sometimes, so the virus goes along. The virus is not—viruses are not transmitted by mosquitoes to their egg, but the egg and the larvae can be infected. We actually infected the larvae. So if you have a contaminated liver—all these, the viruses we're talking about—these are [parvo?] viruses—are excreted in urine, for example, and in the stool so that there's another way of infecting. This story is fairly well known now. Birds were involved in this and we showed that the birds all were infected, horses were infected. So—they had had outbreaks in Japan of Japanese encephalitis and on Okinawa for many years. They were devastating things.

There were 50,000 cases in Japan in one year. We had some soldiers who got—and sailors—who got encephalitis [unclear]. The camp was sprayed with DDT by people connected with our unit, actually. And that was reason enough that the epidemic stopped.

AL: [several words unclear] interview [several words unclear] symptoms, because it's an important story. We're going to get that in first and the, if necessary get to some of the other aspects about your life in [unclear]. What is the development and the establishment of the [Mount Sinai] medical school, that story?

HH: Well, that's an interesting one. A lot of people have had credit in this and some of the people don't know about it. One of the people in the Rockefeller unit with us was Harry Zimmerman, who was the pathologist for the unit.

AL: He ended up at Montefiore Hospital.

HH: No. At that time—

AL: [sentence unclear].

HH: He was—let me tell you. He was a professor of pathology at Yale at that time. He was an assistant to [Milton] Winternitz for a long time. And he was interested in neuropathology for a while and did some very interesting work. He—I know that he did the autopsy on Harvey Cushing, who was a friend of his, and I know he did it on Albert Einstein, who became a friend of his, as I'll tell you in a little while. Well, Harry and I got to be close friends and we lived in the same tent in Guam, and Okinawa for a brief period. I was only there a few days; he was there longer. And he planned, when he came back, to found—help found the Einstein Medical School but it didn't have that name then. He knew [Samuel] Belkin, who was head of Yeshiva [University]. And we talked at great length about what was going to happen then. And when we came back and when I moved to New York, Harry got together a group of doctors, all Jewish, as I remember, from New Haven and from New York with the idea of founding this medical school. Dr. Belkin, who was president of Yeshiva College, was in on this scheme and was instrumental in founding the school, as you know. One of the people on that committee was Leo Davidoff, who was a close friend of Harry's and he was on the staff here [at Mount Sinai] as head of the Department of Neurosurgery [from 1951-55], as you may remember. He was here a few years. And he and I went to the President of the Board of Trustees and suggested that maybe Mount Sinai be the hospital for this medical school, because they didn't have a hospital.

AL: Who was president of the Medical Board here at that time, Horace?

HH: The president of the Medical Board—I'd guess it was Ralph Colp, but I'm not certain.

AL: Not the Board of Trustees.

HH: No, no.

AL: Who was the President of the Board of Trustees?

HH: It was either Harry Rose or—not Harry Rose—it was Al[fred] Rose or Joe Klingenstein. I [gap in tape]—[several words unclear] when I came here, Al Rose was president, and I must say he was a very fine person. I think I came here partly because I was so impressed with him. One of those two men. But it didn't come off because the Einstein—it wasn't Einstein—the Yeshiva people didn't want to have anything to do with it, because Dr. Belkin felt—and maybe I shouldn't quote him so directly, but at least the Yeshiva group felt that this wasn't a good idea, and he expressed a fear that this would become the Mount Sinai medical school and not the Yeshiva medical school.

Now, I remember what Harry [unclear] said, about this. I don't remember in one direction or other, but this very—we were very, very upset by this. We thought this was pretty shortsighted business now and, anyway, our trustees didn't want to do it. I was given various reasons and opinions as to why we didn't want to do it, but I don't think there's any point in going into it. We didn't do it. It is a pity. I think that Leo and I were right about it and everybody would have been better off had we done it. The plan probably was to get Montefiore into it too, but I don't—don't remember that. And what went on there, I have no way of knowing. Harry was by then at Montefiore. He was at Yale when we were in the Navy. He came back to Montefiore and not to New Haven, he didn't return there.

And so that stopped, and then we began thinking about the medical school, a number of us, from that point on. [unclear] was really founded, my guess is we're talking about 1956 or something of that sort. Sergei Feitelberg [Director of Physics Lab and Department of Physics] was very interested. Klemperer [Paul Klemperer, Director of Pathology, 1926-56] was interested. I remember that after I'd been here about two years, I wrote a letter to, I know that was Alfred Rose, saying that--or at least I told him, I can't swear I wrote a letter--but: this hospital had everything but a medical school that they had at Johns Hopkins and why didn't they make one. And we tied up with Columbia, you know, in this sort of way and—

AL: [unclear] post-graduate education only.

HH: Well, not in pediatrics.

AL: [unclear]

HH: We always had—

AL: The students didn't [unclear]?

HH: Before I came here.

AL: Students were here in the fourth year?

HH: No, no, only third year because the clerkship was third year. And whatever the clerkship was, we had a specific number of students assigned here. They took all their clerkship here. Now, they came here on a voluntary basis. After I came here, I set up with Rusty McIntosh a required clerkship for some of the students. [Rustin McIntosh, Director of Babies Hospital at Columbia-Presbyterian Medical Center, 1931-1960, and Chairman of the Dept. of Pediatrics at Columbia P&S for the same period.] And we always had six or so here, and they generally were pleased with it. I don't think this was so in any of the other departments but I don't know, actually. It wasn't true of medicine and surgery. At any rate, nothing was done about it.

And then—I can't remember the year but it was a good ten years, I should think, before the medical school was started—maybe 12, in which there was discussions on the faculty's part about having a medical school. I remember that Paul Klemperer took a big role in this. We used to meet at his house once every two weeks. Sergei Feitelberg was involved in it, Klemperer, Mo Kaufman and Al Gutman [Director of the Dept. of Medicine, 1951-67] attended some of the meetings. I recall that, at one point, we got to the place where we went to the City College to talk with [Buell G.] Gallagher, who was president of City College at that time, and they were interested in an affiliation whereby they would be the university. Nothing came of that. Then after Hans Popper came [in 1957 as Chief of Pathology], the activity increased. You remember, probably, that they approached a number of colleges. I arranged for us to meet with the president of Princeton—oh, what was his name? I've forgotten who was president of Princeton; it'll come to me in a minute. And he was interested and he had his senior faculty; went down there and had dinner, actually. I remember it was at the table, at Woodrow Wilson's table, and I remember one of our party spilled his ice cream on the table. We won't identify him but it was quite embarrassing.

Well, that was dropped. The City Hospital—the City College was dropped in part because some of the people at Montefiore said, “Well, if you're going to sign up with Mount Sinai, why don't you sign up with Montefiore and have a medical school with us?” The rest of the—well, the point I'm making is that this—intents were known for a long time, you know. It wasn't just a few years before the medical school started. And efforts were made in all kinds of directions.

Remember at one point, we were going to tie up with Brandeis. And Dr.[Abram L.] Sachar, who was president at Brandeis, came here a number of times. We had discussions with him. We actually went, some of us, with the trustees to Brandeis. Brandeis wanted some money for this affiliation and one of the senior [Mount Sinai] trustees—I think he was that, not president—very influential (I won't identify him any more)—thought it was a bad idea. And he kind of vetoed our giving any of our money to Brandeis. I've always thought that was a big mistake and the people—the faculty people who went to this meeting also thought so. But since we didn't have any money to give, that wasn't our decision, so that was the problem. Whether that was good or bad, I don't know. That came fairly close, I think, to happening.

AL: [unclear].

HH: Well, I think when Guy Levy came, we convinced him that we should have a medical school. [Gustave L Levy joined the Trustees in 1960 and served as President then Chairman from 1962-76.] He says we—as you know, he keeps saying we fooled him about the cost, but he wanted to be fooled. And we told him it was going to cost \$2 million, he says. I don't remember any figure that small. I wouldn't have agreed to that figure because I knew that the Einstein people raised some more than that. They had serious trouble and I doubt if I ever said, I personally ever said \$2 million. Well, Gus wanted to be fooled. He was all for it from the beginning, and I think he and Hans put in the tremendous energy they had to get the school. It was talked about and thought about a long time. And when the decision was made, I think there were a lot of trustees and just about everybody on the faculty, I think, who wanted it. I think the money-raising part of it has just been phenomenal. It's way beyond any number that we were thinking about, as you know. We were talking about 5, 10 million dollars.

AL: [sentence unclear]. In the Department of Pediatrics, Horace, indeed, you have tenure here and you are scheduling to retire in two years, isn't it?

HH: Two years, yeah.

AL: What do you think, from your standpoint, you have accomplished here?

HH: Well, I think that we have a very good clinical department, without any question. I'm told by people that it's probably the best kind of the patient care is given in this department. I think part of that is because we—the people, the faculty people and the doctors, some of them—many of whom I found here, some of whom I've brought—for example, Ralph Moloshok was here; Donald Gribetz, I brought, great clinicians and great doctors and interested in it and patient care, who just don't give lip service to it. I think that's part of it. I think they've had assistance from the administration. I think they've been very understanding. This is true of Martin Steinberg and Dave Pomrinse [Director of MSH, 1969-75] and the Trustees, a good many of them. And Janet Levy, for example. Yeah, Levy gave us a lot of money to remodel our very ancient wards in that old building. She gave us fellowship money. We had gotten a sum of the Welt money, which was given to the hospital by a pediatrician, as you know, Sara Welt. We were not getting any of that money till I came here and was pugnacious enough to make sure we got it. We should have gotten more; we got a third. When I came here, it was being used by George Baehr for medicine and Paul Klemperer for pathology, and I insisted that we get a third of it, which we did. It's basically been taken away from all of us and put into the medical school, which I think is really unfair, but that's the way it is. We used this money to help train people, like Ralph [unclear], Sidney Blumenthal, Donald Gribetz, Irwin Gribetz. The Welt paid like \$4,000 a year [several words unclear]. So I think we've been interested in patient care and I think we've been interested in the outpatient patient care. I must say that I think we often, in this hospital, like others, we give lip service to outpatient care and we don't really care about the patient care in the outpatient

department. But I think that's not been true in our department. I think that contributes to it. I think they insisted on high standards in that, in that area.

I think the teaching of house staff has been very good, and I think the teaching of the medical students has been better than good. I think it's outstanding. I know from the students' questionnaire answers that in all the categories that they answer, we're either first or second in the five or six years we've been going. The clerkship's very popular. I think the teaching is important. We have some really great teachers. We have Stanley Uretsky, who was a chief resident here, went through our training program, was I think, one of the best, most gifted teachers that I ever saw, and spends all his time at it, pretty much. He's a very gifted researcher, so it's done with real heart. He's here all the time and he's able to teach both medical students and house staff in a fairly remarkable way. So I think we are very good in the teaching part of this.

I think the research has been very good, too. For the size of our department, the outside research money we get is very out of proportion to most of the departments. And we made some fortunate recruiting—recruitments. I hope I'm partly responsible for it. One of them is Kurt Hirschhorn, who's an extremely gifted investigator, runs the Genetics Department [division in Pediatrics]. And I'll say now—I don't care who knows this—he was trained in medicine and he was offered—at least was brought to the attention of the Department of Medicine first. Lou Wasserman heard that Kurt wanted to leave Bellevue where he was and NYU for reasons that had nothing to do with him personally, he was under the department of Medicine. Head of Medicine then was Lew [Lewis] Thomas, whom I'd known for many years, as you know—was in this unit, the Rockefeller Unit, was overseas with us and is quite a remarkable person. And Lou Wasserman tried to get Kurt to come to the Department of Medicine. They were not interested in him, and then Lou told me that, why don't I try to get Kurt, and I did. And we got support from Martin and George James [Dean and President at Mount Sinai, 1965-72]. And I think he came, partly because of the effort that all these people and I put into it, and he's been a great asset.

We recruited other people of considerable prominence all through the years. We trained Sidney Blumenthal. He's one of the outstanding pediatric cardiologists in the world, and he's now no longer here, he's in Miami. He went from here to Columbia to Miami and has made great contributions. I think our—in the last year, we recruited George Acs, who's a world-famous—he's a physician but he's a world-famous molecular biochemist. And he's done quite remarkable things: DNA synthesis, has shown one—some of the significant enzyme changes that occur in malignancy, for example. And this work, which is just being published, I think is of Nobel Prize caliber.

I think our own research [several words unclear] been very helpful in the group that I got around. They've done very well. The department's also recognized nationally and internationally. Aaron Rausen, for example, came here as a chief resident, ran the Greenpoint service for us and then the Elmhurst service. He's now head of the Department of Pediatrics at Beth Israel and has really improved that tremendously. The people in the department are recognized there, a lot of them in honorary societies. I think

for the size of our department, we have a lot of people in the American Pediatric Society, which is an honorary society, [and] in the Society for Pediatric Research than probably any department in the United States.

AL: I wonder if I can ask you now just a few things more, and I'm going to try to partition the time. I am interested in your comments on where you see the hospital and the medical school today, what you see as its strengths and weaknesses and its future. I will not ask you for a total essay, but just in brief.

HH: Well, let's see what the strengths are. I think, first of all, there are a lot of talented people here. They were here long before the medical school. They've done a great deal of clinical medicine. They've done a great deal of research, and there are people here—I don't have to tell you—they're famous all over the world. The medical care in general is, frankly, not as good as it can be, as it should be and I think it can be. Part of it is the size that we have, and part of it has to do with things over which we have no control. I'm sorry to say this includes labor unions, what I think is over-administration. For example, an administrator is quite attached to the Department of Pediatrics. I never see the man. He's a very nice fellow. I don't know what he does. I must say, I've been here long enough, I guess, so that I know all these people. When I have a problem, I'm afraid I go to Dave Pomrinse, who's very responsive. So I think there's a tremendous amount of administration. There's a lot of needless paperwork. We have union problems. I guess this is true everywhere, I'm told. It doesn't make me any happier that we've got them. I think this is a real handicap. I think a large part of our effort and money is wasted. I think the basic science departments have not gotten their fair share of our money.

And I think one of the weaknesses is that the department chairmen here have never been able to talk to the administration about where our money goes. We've gotten most of the money; we're responsible for it. But the administration's taken a position that they won't tell me about what happens in the Department of Medicine. It's fatuous because I talk with the Professor in Medicine and I know what he has. Their excuse is that they think if we go around the table, we'll start arguing with each other. I think that may be, but the real truth is that they don't want us to know. Nobody wants anyone looking over his shoulder. I think this is too bad and I hope it gets corrected. I think our department's gotten its fair share of it. I must say that it's taken a lot of irritation and struggle and unpleasantness sometimes. But I think that the institution's been good to us, but I think we ought not to have to fight for it, and I think we deserve it, frankly. It should be done in another way. This was not done this way at the Johns Hopkins; that I can tell you.

Another criticism I had has to do with selection of students. At Rockefeller they select their students all year long [?]. But there's tremendous wasted effort in this here. You can't tell the difference between Student A and B, if you interview him a half hour or six people interview him, you can't pick people that closely. You have to stop worrying so much about it and getting everybody in a dither and do it more easily. I would do the system that is at Hopkins: a few people are involved in it; they get help from other members of the faculty, and they don't sit around in a room and talk for three

hours about one candidate, who's decided to go to Penn anyway. You know, this is what happens here and it makes for, I think, hard feeling and I don't think they accomplish anything. Again, I don't think we'd get better students. We'd do it with a lot less trouble and a lot less....

AL: You mean you'd have fewer people making the decisions?

HH: Oh, sure. Well, there should be fewer people making that decision, for certain, and they ought to realize their limitations. I've interviewed a hundred intern applicants every single year since I've been here, and I do it all myself. And I assume, I am as smart as most of the people on the committee, and I've been at it longer. You just can't tell people apart this way. So you make the best choice you can; they also have a choice. And you let it go and stop beating everybody over the head. Makes for a lot of hard feelings.

AL: Horace, I'd like to get in one or two personal things now. For, instance, you were a baseball player.

HH: Yeah. I was a professional baseball player.

AL: Tell us a little bit.

HH: Well, when I was—when I went to medical school, I played in baseball in college, and this won't surprise you, but I was on the boxing team [chuckling] until my mother found out about it and she stopped me. So I guess I'm pugnacious by nature. I played for three years but I played in—it was a minor league team in the Scranton vicinity, what became later the New York-Pennsylvania League, and I've forgotten the name of the league. But it was Binghamton, and Elmira, Scranton, and Wilkes-Barre.

AL: When were you in that league?

HH: Oh, let's see. Well, I was in college [unclear]. So I must have been—well, I left college when I was 19, went to medical school when I was 19. So I guess I was 17, 18 and 19.

AL: You played part time then.

HH: Well, in the summer.

AL: Oh, I see. In the summer, I see. Make money?

HH: I can tell you how much money I made—about a hundred dollars a season. Plus expenses.

AL: [sentence unclear]. [chuckles] Let me ask you one or two other things, Horace, about some of the other people who were here. You told me a story about Mark Ravitch, [Director of Surgery, 1952-55] with whom you had a great friendship.

HH: Yeah, we were very close friends—

AL: When?

HH: —in Baltimore, and I guess Alan Guttmacher and I were responsible for his coming here.

AL: That story about the marksman.

HH: Do you think I ought to put that in the—.

AL: Oh, yes [sentence unclear]. No, no, I think it's a [?] story.

HH: Well, one of my other athletic accomplishments was that I was a very good rifle shot. And when I was in college I was, as I told you, on the baseball squad until they found out I was a professional. I was also on the boxing team and I was on the rifle team. We all had ROTC, and I'd never shot a gun in my life. I don't know what a Jewish boy was going to do with a rifle but [chuckles] they made us shoot and I had a natural knack for this. And I soon got on the rifle team. Then the World War came, and at the end of the war Mark and I came, and one day there was a picnic arranged. His family and ours were going on this picnic. I guess our—my children were—my son was maybe four years old and his children were a year—about the same age or a year younger. And my daughter was four years older. So much to my surprise, Mark brought along a rifle. We were going to have a rifle contest. I was very unhappy with this. I don't think you ought to shoot 22-rifles with little children running around but, well, he insisted and we put the thing way off in the distance. And our wives kept the kids away and they were watching this contest. Mark said he would fire first and he fired. He hit maybe the bulls-eye once out of five shots. And I was very irritated by the whole thing, that he insisted and he was bragging about how great a shot he was. I stepped up to this target, put five shots in the bulls-eye. That was the end of that contest. Anyway—

AL: What is the story of Ravitch's leaving here, from your point of view?

HH: Oh, it's pretty complicated. You probably know your part of it [as a surgeon on the staff] better than I do. [several words unclear]—

AL: [talking over HH; unclear]—

HH: To summarize it very briefly, I think Mark was, and is a very strong personality, as you know. He's an extremely bright person and I think a fine scholar. I thought that he was a good surgeon. He never claimed to be spectacular in, like tying knots behind his back or whatever it is you fellows do. But he was, I think, good. He went through a residency at Hopkins. He stood up to the neurosurgeon down there, Walter Dandy, who would hit all of his assistants on the head with things. And he hit Mark on the neck [?] once, and Mark left the room and they wanted to fire him, he wanted to get Mark fired but they wouldn't fire him. He's a swell character, and a very good teacher and a very good scholar.

Part of the problem was, I think, in that—I know this for a fact because I was in the room when Mark was told he was going to be head of surgery. They also had three other surgeons. At least two of them told me that they were told it wasn't going to make any difference; that they were still going to be head of their services. So I think that the problem arose right then and there. Then, too, Mark would be quite rude once in a while and nobody called him to—challenged him. It got to the point that he exceeded somebody's patience. I think Martin Steinberg [Director of the Hospital] one day said, "That's enough." And he went and the Trustees decided they would have him leave. Well, Alan Guttmacher and I went to whoever was president then (I think then it was [Joseph] Klingenstein) and said, "Look, we shouldn't do this. And you know, it's going to be very bad for this hospital if you just fire the Surgeon. He's well known. He's a very close friend of [Alfred] Blalock's, who actually was asked about him before he came and arranged to have him come." And now, Al Blalock [Surgeon at Johns Hopkins] was a world famous person, deserved it in every way, and this wasn't good. Well, they didn't—they insisted on not going that route, they just fired him. And they also gave him some money, not very much but enough.... None of this was necessary. He had just turned down the chairmanship at the University of Virginia, which was pretty rough on him. And—

AL: [sentence unclear].

HH: The whole thing was unfortunate.

AL: I heard he wanted to leave since that [?] when he had gone in front of the Board of Trustees and it had to do with the appointment of a former [?] member of the staff to the Associate-ship or Associate-ship on the surgical service. And he didn't want to submit two or three names [several words unclear] came up. And he said, 'Well, if you put me in charge, you should trust me.' And in the course of the conversation [unclear] 'If you couldn't abide by it, then you should leave.' He said, '[several words unclear] contract. You'll have to fire me.'

HH: Well, that he said, but not in that context.

AL: I see.

HH: He said that and they very foolishly, I think, took up his challenge; they didn't have to do it. And, well, I think he did something that was just too much for Martin one day. I don't remember what it was. It *was* pretty bad. My criticism is that they never stopped him at any of these points, and Mark is one of these guys who keeps pushing. And he wanted to be fired. I think subconsciously, all of this was that he really was fed up with a lot of sniping. Now, you know better than I do that sniping went on against him all through—I must tell you, too, that he was a little disappointed in [Ralph] Colp and [John] Garlock and I understand why. They were very good surgeons but they really weren't very good teachers. And he used to say that they insisted on making rounds and their teaching was at that kind of level, the daily bedside teaching. Mark was much more of a scholar than they were. He put in a lot of work in the teaching, as you probably know. And he was a

better teacher than either of them, as far as I can make out. They never accepted the fact that he was head of the department; that, as I say, one of these two gentlemen told me.

AL: Tell me, can you say anything—

HH: He probably wanted to leave and then I think they obliged him in an unpleasant way and did the hospital, at least the Surgical Department, a great deal of damage, because my reading is that we came off in public opinion, by far, the worse in this. I think you agree.

AL: This tape will be over in just a little bit but if you have some remarks about what you know about Bela Schick or Murray Bass?

HH: When I came here, as I say, I knew Bela, I didn't know Murray. And there's a dictum that the old chief should leave, but that did not happen here. I had the most wonderful relations with them. They came to all the meetings. They talked, at my urging, I must say. They wanted to fade into the background. We wouldn't let them, and I wouldn't let them. They made great contributions to the department for 10 or 15 years. I really enjoyed my relationship with them. They were helpful in every way.

AL: Do you remember much about their personalities?

HH: Well, Murray was one of the most delightful people I ever met. He was a talented violinist. He was so interested in every patient. He had the enthusiasm of a first year medical student. Bela Schick [chuckles] knew everything. He was a little more vain than Murray, enjoyed great triumphs all his life, as you know, and you had to be a little more careful about calling on him first before anybody. And—

AL: [sentence unclear].

HH: Oh, he was the world famous man. Murray was famous. [tape on/off] He was a marvelous diagnostician. He discovered several signs. For example, he was the first one who discovered bulging of the fontanel in babies who'd gotten too much vitamin A, for instance. He discovered blue—kind of blue lines under—or dark shadows under the eyes in mononucleosis, for example, in children and so on. He was a great man and a wonderful person.

AL: What do you intend to do, Horace, after you leave?

HH: You mean after I leave being Chairman?

AL: Yeah.

HH: Well, I'm going to go back into my laboratory, if they let me stay and work, I think, with Joe [?], whom I mentioned before. We're starting work on fusion of cells. Miss [Helen D.] Zepp [a long-term research assistant] and I fused the mosquito cells and human cells. This is a tremendous difference in genetic [?] phylum, as you can imagine. And this

makes a cell, which we think would be useful for treatment of cancer in human beings, and I expect to devote full time to that.

AL: Do you have children and grandchildren?

HH: Yes, I have a daughter who lives in Irvington [NY]; a son who lives in Dobbs Ferry [NY]. My son is an Assistant Professor [of Pediatrics] at Columbia...[three grandchildren] [end of tape]

End of Interview