

Henry Blackburn, MD

Narrator

Dominique A. Tobbell, Ph.D.

Interviewer

**ACADEMIC HEALTH CENTER
ORAL HISTORY PROJECT**

UNIVERSITY OF MINNESOTA

ACADEMIC HEALTH CENTER ORAL HISTORY PROJECT

In 1970, the University of Minnesota's previously autonomous College of Pharmacy and School of Dentistry were reorganized, together with the Schools of Nursing, Medicine, and Public Health, and the University Hospitals, into a centrally organized and administered Academic Health Center (AHC). The university's College of Veterinary Medicine was also closely aligned with the AHC at this time, becoming formally incorporated into the AHC in 1985.

The development of the AHC made possible the coordination and integration of the education and training of the health care professions and was part of a national trend which saw academic health centers emerge as the dominant institution in American health care in the last third of the 20th century. AHCs became not only the primary sites of health care education, but also critical sites of health sciences research and health care delivery.

The University of Minnesota's Academic Health Center Oral History Project preserves the personal stories of key individuals who were involved with the formation of the university's Academic Health Center, served in leadership roles, or have specific insights into the institution's history. By bringing together a representative group of figures in the history of the University of Minnesota's AHC, this project provides compelling documentation of recent developments in the history of American health care education, practice, and policy.

Biographical Sketch

Henry Blackburn was born in Florida in 1925. He volunteered for the Navy after high school. He received his BS from the University of Miami in 1947, his MD from Tulane University in 1948, and his MS from the University of Minnesota in 1957. He worked as a staff physician at the Methodist Clinics in Cuba in the summer of 1949. He did his residency at the American Hospital of Paris (1949-1950). He then served as the Medical Officer in Charge in the US Public Health Service, Foreign Quantine, in Salzburg, Vienna, and Munich. In 1953, he came to the University of Minnesota as a medical fellow in the Department of Medicine (1953-56). In 1956, he was Chief Resident of Internal Medicine at Ancker Hospital in St. Paul. He then joined the Laboratory of Physiological Hygiene, first as a research fellow (1956-1958), then Assistant Professor (1958-1961), Associate Professor (1961-68), and Professor (1968-present). He was also a Professor in the Department of Medicine (1972-present). He became the director of the Laboratory of Physiological Hygiene in 1972 on the retirement of Ancel Keys and remained director until the merger of Epidemiology in 1983, which he then headed until 1990. He retired in 1996 and remains in the School of Public Health as Professor Emeritus in the Division of Epidemiology and Community Health.

Interview Abstract

Henry Blackburn begins by describing his background, including his education, his experiences in the Navy, and his decision to pursue a career in medicine. He discusses coming to the University of Minnesota and his experiences as a Fellow in the Department of Medicine in the 1950s. He discusses the Laboratory of Physiological Hygiene (LPH), including collaboration between Cardiology and the LPH; the School of Public Health (SPH) and the distance between the SPH and the LPH; the retirement of Ancel Keys and the status of the LPH; space issues for the LPH; the teaching responsibilities of the LPH faculty; the growth of the LPH in the 1970s; the merger of the LPH and the Division of Epidemiology; and his research projects in the 1960s, including the Seven Countries Study. He describes attitudes toward public health and epidemiology in the mid-twentieth century; relationships between divisions in the SPH; the status of public health in the College of Medical Sciences; and relations between the University of Minnesota Medical School and the local medical community. He discusses federal funding, particularly the funding epidemiology studies; tensions between medicine and public health; concern in the 1960s over a shortage in health manpower; the Medical School curriculum revisions in the 1960s and early 1970s; the nurse practitioner program and public health nursing; the reorganization of the health sciences in 1970; the Division of Epidemiology; the recruitment of minority students; women faculty; changes in public health during his career; and the growing emphasis on personalized medicine. He talks about Albert Sullivan, CJ Watson, Ancel Keys, Maurice Visscher, Gaylord Anderson, Robert Howard, Lyle French, Lee Stauffer, Robert Kane, and Edith Leyasmeyer.

Interview with Doctor Henry Blackburn

Interviewed by Dominique Tobbell, Oral Historian

**Interviewed for the Academic Health Center, University of Minnesota
Oral History Project**

**Interviewed at Doctor Blackburn's Office, West Bank Office Building,
Department of Epidemiology and Community Health, Minneapolis, Minnesota**

Interviewed on November 11, 2010

Henry Blackburn - HB

Dominique Tobbell - DT

DT: This is Dominique Tobbell. I'm here with Doctor Henry Blackburn. It's November 11, 2010. We are in Doctor Blackburn's office in the West Bank Office Building, on the third floor in the Department of Epidemiology and Community Health.

HB: You're oriented as to time, place, and person.

DT: Indeed.

HB: Can you say Methodist Episcopal? Then, you've passed your neurological....

[chuckles]

DT: Doctor Blackburn, thank you for meeting with me today.

To get us started, can you tell me a little bit about where you were born and raised, and how you came to enter medicine?

HB: I'm the son and grandson of Methodist preachers, origin from Yorkshire [England]...itinerant Methodist preachers. My father was head of a missionary church on Miami Beach in the 1920s. Obviously, it was a missionary church because there weren't many people there, and that's where I was born and grew up. We were run out by the 1926 hurricane. My father's church was destroyed. Anyway, I lived and grew up in small towns in Florida. I graduated from the University of Miami during the war. The Navy sent me to medical school. I decided to be a doctor from very early on. I joined the Navy at seventeen, and they allowed me to choose to go into medicine. They had a

college program. I got involved in public health as a medical missionary for a summer and saw that missionaries couldn't accomplish very much in medical problems in Cuba due to U.S. ignorance and poverty and oppression. I was in the eastern tip of Cuba. Then, I spent three years in the Displaced Persons Program in Austria after the war in DP [displaced persons] camps. It was clear that medicine was a drop in the bucket in such mass phenomena.

I was thus susceptible to the interesting things that Ancel Keys was doing when I got here for my internal medicine training. I was working with an electrocardiographer, Ernst Simonson. That was my big interest in medicine. He had a joint appointment in the [Memorial] Stadium Laboratory. You've heard about that laboratory?

DT: Yes.

HB: Physiological Hygiene under the football stadium. That's how I met Ancel [Keys]. At that time, he was talking about this "Holy Grail," this relationship between the "mode of life," he called it, and the new epidemic of heart attacks. I had a French wife, an international life, and the international proposition he made me sounded very attractive. So that's how I got into this.

DT: You said you wanted to be a physician from very early on.

HB: Oh, yes.

DT: What gave you that idea?

HB: Actually, it was because—it's a rather selfish thing—medicine embraced all the things I was interested in, in science and the humanities but you didn't have to be expert in any of them. For a while, I was a high school member of an archaeology group at the University, and we dissected some Indian burial mounds and things like that. They discouraged me. "You've got to be a top chemist." "You've got to be a top linguist." "You've got to have sixteen languages." "You have to be a mathematician." I said, "I can't be all those things." Medicine seemed to have the humanities in it as well as the sciences I was interested in.

Then, in wartime, you were given this opportunity to choose between being a deck officer and getting shot off of an aircraft carrier or go to medical school. That was an easy decision when you're eighteen. I went in for the usual idealistic reasons, otherwise.

DT: You went to Tulane University Medical School?

HB: Yes, the Navy sent me to Tulane. That was a real adventure for a nineteen-year-old. New Orleans in 1940s was as exotic as any place on earth with its many cultures, its jazz, and its French language. I was already a Francophile. It was a very distinguished school at the time with Alton Ochsner as head of surgery, John Musser, Pennsylvania, Head of Medicine, other outstanding people.

It, also, was fashionable for Tulane graduates to take part of their residency at the American Hospital in Paris. It was just something that the elite did, so I applied for that in the second year in medical school, and was accepted. So after my American internship, I went to Paris. It all tied together., medicine, music, and French.

DT: You did your internship at Chicago?

HB: Chicago Northwestern and, then, the American Hospital in Paris and, then, residency in internal medicine here at Minnesota.

DT: You spent one year at the American Hospital in Paris?

HB: Yes.

DT: I recently interviewed Tess [Theresa] Sullivan...

HB: Did you really?

DT: Yes.

HB: I'm so glad you went to see Tess. How recently, after her heart attack?

DT: About two weeks ago. Yes, after her heart attack.

HB: How nice. Al was my best friend.

DT: Oh, really?

HB: He was ahead of me one year at Tulane, but he's the one who met my boat train and took me on a marvelous tour of Paris my first day. I was *so envious* of him. You didn't know Al. He was associate dean for many years here. He just talked French! He did so badly, but he didn't care...spinning the ball back over the net, you know.

I refused to open my mouth unless I sounded perfect. So it was three months before I would say a word, and he was high-fiving it with the janitor and talking with the professors. His accent was *terrible* and his grammar was worse, but he didn't care. He communicated and I didn't. Anyway, he was such a good friend.

This is irrelevant to our story. We were having an awfully good time there, and Tess shows up. They decided they were going to get married. I said, "Damn! Al, there goes our winter ski trip in the Alps." He said, "au contraire! You'll go with us." So I went on their honeymoon.

DT: [laughter]

HB: They got married and spent a week in a Paris Hotel, and I took a taxi and met them, and we went off to Tirol [Austria] together and learned to ski together.

[laughter]

HB: We were dear friends, and they were one of the main reasons I came to Minnesota.

He was a great leader. We got into trouble only once. I had a brilliant, young Minnesota student working with me. He wrote two articles with me, led a jazz band, and was married and mature at age twenty-one, and Al rejected him for medical school. He was number one in his class. I said, “You’ve got a cookie cutter, and you just have to fit that cut! Exceptions can’t get into your darned medical school, Al. I don’t understand what’s going on.” He stuck by his guns. Of course, the guy was accepted at Northwestern and led his class there. Soon his marriage broke up and later he was under investigation by the Medical Society and had all the aspects of immaturity that I hadn’t seen now were seen.

[laughter]

HB: He was more experienced in judging character than I was.

DT: I was actually wondering what led you to come to Minnesota.

HB: Well, [War in] Korea came along and the Navy had given me my education, so I knew I was going to be called back to duty. I was very good friends with the physicians in the American Embassy in Paris, and they had a big problem. They had a Hungarian physician who was taking bribes. She had married a consular clerk in Salzburg, and he was a nephew of Jimmy Burns, who was our Secretary of State. So he’s gotten his wife’s citizenship in one weekend, and these two simple people on government salaries lived in a villa and had limousines and chauffeurs, and something clearly wasn’t right. They sent me, a little twenty-six-year-old doctor, into this intrigue in Austria to clear things up—and I did clear things up medically. They were passing people with active TB [tuberculosis]. I cleared that up, but they made my life hell. At any rate, I had public health service instead of going into the Navy. At the end of that service, I applied to several schools, most of them in mountains. I was a mountaineer and skier. But I applied here because of Al and Tess and a good program, and decided to come here.

DT: When you arrived, you were in the Department of Medicine, initially?

HB: Yes, as a fellow. I expected fully to be at the University Hospital. C.J. Watson, the head of Medicine, said, “No, Henry, you’re going out to the V.A. [Veteran’s Administration] Hospital.” I assumed that that was like being sent to Siberia.”

So I was not very happy. The first person that greeted me was Neal Gault and you will hear about him if a predecessor hasn’t interviewed him. I think Erik [Moore] said he interviewed him.

DT: Oh, yes, he did.

HB: Neal was chief resident on duty the night I arrived. He straightened me out in a hurry. He said, "This is an awfully good place." It's true. It didn't have all the internecine foolishness that went on at the university. The education was just as good an experience as there could be. So I was very happy, and Neal Gault was my lifesaver for that.

So then, after two years I was chief resident at what's now Regents Hospital. It was Ancker Hospital then.

Then, the last week of my service, Watson—you'll hear a lot about C.J. Watson; he really developed medicine in our Medical School—offered me a job as associate director of the Ancker unit of the University, a collaboration with the local hospital. And Ancel Keys offered me a job at that same time. When I accepted Ancel, Watson was furious. He said, "What are you going to do working with those crazy people in that crazy place? You'll never be in the elite of Minnesota medicine."

[chuckles]

Ancel had something new and different to offer.

DT: Had you had experience at the lab of Physiological Hygiene during your residency?

HB: Yes. At that time in medicine, you could take a six-month rotation through a research project. Because of my work in electrocardiography with [Ernst] Simonson, who had an appointment in the laboratory, I met them very early. I spent six months there, examined every single fireman in the City of Minneapolis on a study that was purported to look at differences in physical activity. It turns out the firemen are so inactive that you can't really study them across a range of activity. [laughter] But I examined 440 firemen anyway. So that was my exposure to what was going on in epidemiology.

During that time, Keys was making his informal looks at lifestyle and heart attacks in Naples and Madrid and South Africa, and he would come back with these wonderful stories of places where there were more coronaries than we had and other places where they had to wait around for a week and drink local wines before they could find a coronary case. That all sounded very interesting to me.

So, yes, I had that introduction to the laboratory. Then, he offered me the research fellowship. We got an umbrella grant in 1960 that covered many activities in cardiovascular disease at the Medical School. I got in under that and became assistant professor and have been here ever since.

DT: It seems like you have an interesting perspective on C.J. Watson.

HB: Only from a distance. He was a beautiful man, elegant, distinguished, eloquent, among the clinical investigative elite. That's a tradition that goes back to Sir Thomas Lewis, who really put scientific medicine on the map. Watson was involved in a field I had no particular interest in him. He was interested in bile pigment metabolism, and porphyria and other exotic conditions. He was always a little distant, but very correct and very pleasant, a great leader.

DT: Why do you think he had that attitude about the Physiological Hygiene Lab?

HB: I think that was the attitude of some people in the Medical School. It may have had to do with Ancel's relationship with Maurice Visscher, who was the great man in physiology and is the man who hired Ancel from the Mayo Clinic [Rochester, Minnesota], and that just never did go. Ancel set up Physiological Hygiene in Physiology about 1937, I think. He had his own unit in Millard Hall and began doing the things that he was interested in doing: human biology, human physiology, about reactions to stresses, cold, starvation, heat, exercise, and so forth. Visscher would come around when he had visitors and point to *his* work, the work that Ancel was doing. Ancel didn't like Visscher taking credit for it. Then, being the junior faculty, Ancel had to give 50 of the 120 lectures in physiology. Ancel did not lecture well. He was a marvelous scientist, but did not project well. Hated it! Hated dealing with inferior minds—that's my evaluation of it. He'd just had it! So he really was growing bigger and wartime funding came along and he needed space; it was a very happy time for him. You'll see on the interview that I did with Ancel in his later years that he smiled and said how happy he was to leave Physiology and get over her. Gaylord Anderson, the first dean of the School of Public Health, was convinced that what Ancel was doing was part of the future of public health, thus giving Ancel really *carte blanche* to lead a research unit within the school and, yet, there was a distance. I mean we were distant physically. [chuckles] We had almost no teaching responsibility in the school or in the Medical School, just a little bit of teaching to Phy-Ed [Physical Education] students. So it was the best of all worlds for Ancel.

DT: I guess Maurice Visscher was quite a powerful figure within the Medical School.

HB: Tough and distinguished in science, an outgoing person, a strong liberal. He was on [President Richard] Nixon's hit list. He used to call me sometimes on weekends. I remember when he called once, "Henry, those Nazis are after me again!" I said, "You should be proud of the fact that you're on that list. What can I do?" [chuckles] He was a very distinguished man, but an old school guy. It was clear that he and Ancel were going to clash. The attitude about applied physiology and human physiology on this scale different from that involved with beating rat hearts and heart-lung preparations. It just wasn't in the tradition. To them, it was not very interesting and, yet, it was a whole new vision of physiology and medicine and hygiene that Ancel extended to the whole population.

DT: Did those attitudes change after recognition was given to Ancel's work?

HB: No, not among that generation of clinicians.

DT: It was specifically about the kinds of physiology that Ancel Keys was doing rather than about public health per se?

HB: It was both. Public health, still at that time, with epidemiology had about it the faint “odor of the outhouse.” It wasn’t medicine, and it certainly wasn’t clinical investigation, and it certainly wasn’t basic mechanisms, and why would you be interested in such a thing? It wasn’t science, and it certainly wasn’t in the tradition that Sir Thomas Lewis started, about the chemical, physical approach to systems and functions. It just wasn’t in the tradition. That still is true. Doctors think of individuals and they think of organs and functions. They don’t think of populations and social influences on disease very much. Smoking and air pollution are two that are important, but their hearts aren’t in it, and it’s still not part of that *elite* that really controls where medicine goes, where medical research goes. It’s individually oriented. It’s mechanism oriented. It’s drug oriented. It’s surgery oriented. It’s not population science. It’s so refreshing to find an occasional leader in internal medicine, even surgery, talking our language, but it’s so rare.

DT: That was one of the things I wanted to ask you about, because, obviously, at the time that you and Ancel Keys and the lab were doing so much important work on coronary heart disease, you had cardiac surgeons at Minnesota also making breakthroughs. I wondered if there was any communication between the two groups and how relations were between you and the surgeons.

HB: I can give you a couple specific examples. Henry Buchwald, whom you will interview if you haven’t, and Richard Varco... Buchwald was a protégé of Richard Varco, who had many of the ideas that Henry developed. They were research oriented surgeons and they both had Ph.D.s in physiology. It was a tradition for the surgeons to get masters and Ph.D.s in those years, in the 1940s and 1950s.

They were interested in many, many things. They hit upon this idea of short circuiting the bowel to reduce cholesterol. I made an offhand statement once. I said, “I think it’s a great idea for people who have this problem that can’t be controlled by drugs and diet, that you’ve devised a safe procedure. You’re going to do good things for people with familial hypercholesterolemia. You’ve done it. Why would you go any further?” In other words, they wanted to do a trial with hundreds of people and short-circuit their bowel to see whether they could actually prevent coronary disease. They thought that my question was the stupidest thing they’d ever heard. There was always a little, usually good-humored, testiness between us. I said, “You’ve done it. It’s a clinical application. You couldn’t do it better.” But the *grand* idea that you would examine hundreds of people and operate on hundreds of people and compare them to see whether it reduced coronary attacks, I thought was grandiose. At any rate, they did it, and they did it well, and it came out the same year that statins came in, and statins did a better job than surgery. That wonderful contribution to science—he recently wrote a follow up—they still hold on to the view that they were the first people that really demonstrated that

coronary disease could be prevented by lowering cholesterol; they should get that credit. They just shifted their short circuit operation to the treatment of obesity, so they're still short circuiting bowels hundreds of cases.

[chuckles]

HB: As far as the relationship of surgery to our own stuff, there was really none. I had a very warm relationship with [C. Walton] Walt Lillehei and his brothers, Rich and Jim, but never any direct collaboration.

There was a good deal of collaboration between Cardiology, at one time, and Physiological Hygiene, in studying such things as cardiac outputs under exercise conditions, maximal oxygen consumption as a measurement of work capacity and the effect of bed rest on fitness and oxygen capacity. That went on for a number of years. Carlton Chapman was the principal collaborator and he actually had an appointment in the lab, one of the most distinguished internists, cardiologists Minnesota had ever had. He went on to be dean at Dartmouth [College] and, then, head of the Commonwealth Foundation. So there were some long relationships and close collaborations. Yang Wang, who recently died, collaborated with us on these issues.

But in general, we've been out in the population and they've been doing their good things. There has remained a basic issue that physicians think in terms of the individual and we think in terms of population. The twain doesn't very often meet.

For example, Jay Cohn, who was head of Cardiology for twenty years—still we collaborate with him and do some analyses for him—was very interested to tap into our population surveys. We've probably been in the homes of 400,000 people in the Twin Cities over the last twenty-five, thirty years. He sees this as a way of recruiting people and putting them in the medical craw. They come through us and we do our research and, then, they go over there. They select the ones that are going to have bigger and better procedures. I said, "It doesn't work that way. We regard the population as our laboratory. We're not going to feed them into your medical system." He just never could understand how we treated the population as our laboratory. There's *always* that testiness. I'm writing about it in my book, the sources of controversy, the sources of friction, the concern for the individual mechanisms versus concern for the population science and concern for the social determinants of disease. It's a long time before we get together. Training is still in tertiary medicine, in tertiary hospitals where the elite are the clinical investigators and not the epidemiologists.

DT: It's also a difference in perspective in terms of being interventionists and being preventive, as well, it seems, that kind of tension.

HB: Oh, absolutely, and in the way the whole system developed to intervene with drugs, surgery, or "procedures" versus promotion of health.

I had a fascinating experience in an elevator at Mayo [the Mayo Building] once with B.J. Kennedy, who was head of the cancer program. You'll certainly study his life. He actually seemed very hostile. "You're trying to take away our livelihood, are you, Henry, by preventing all these things?" I assumed he was joking. I laughed. [chuckles] I said, "Yes, that's the idea." But he was dead serious.

Then, Leslie Zieve I met once. He was a wonderful gastroenterologist at the V.A. and, then, downtown, and I mentioned something that we were doing with surveys. I used the word, "surveillance." "Monitoring us, are you, Henry?" The paranoia of the rugged individualists, that we're checking up on maybe how his hospital is functioning versus another hospital. "No, we're just determining the rate of heart attacks in this community and their survival in hospitals. We do have the data on the different hospitals, but we're not going to publish it." There's a suspicion of that sort of aspect of social medicine. It's ingrained, but ingrained through the models that one has in one's education, and the problems that one faces, and the fact that we've not been very inspiring to students.

DT: That begs the question... When you were in medical school, did you get much training in public health, any courses that looked at this?

HB: I had the epidemiology course right after lunch. Need I say more? I still remember how to build a screen on an out-house but, no. We happened to have had Grace Goldsmith as the head of the School of Public Health when I was there. She was a great nutritionist and had done some work in pellagra. We had a little more vision than ordinary medical students, but I didn't get involved in this until, as I say, I was a missionary in Cuba.

DT: As much as there was this tension within Medicine regarding epidemiology and public health, Physiological Hygiene was able to get a substantial amount of federal funding for what they were doing. Was this from the Public Health Service, usually, federal grants?

HB: That's a major issue in Medicine. It's caused many problems, because epidemiology studies are long term and extensive. I've got segments in my book where I describe people's reactions when they see a grant for \$3 million. They immediately divide it into thirty \$100,000 projects that they could have for what *they* want to do. These are people on the Advisory Board of the NIH [National Institutes for Health]. I've been on that board for a four-year term. You see it there. They bristle, such that they actually have developed discriminatory rules that you have to submit any grant that costs over \$500,000 a year for policy review before it goes through scientific review. There are other aspects of the funding that are discriminatory against epidemiology because they don't take recognition of how an epidemiology study usually works. You need a year for development. Then, you have a huge year for recruitment and work and, then, you need a couple years at the end for analysis. You can't have more than a ten percent increment a year in your budget. That's the rule. That doesn't work for epidemiology. There are all sorts of things like that determined by people who don't like any money going this way. That has changed to some degree, and I talk a great deal in what I'm

writing, about how NIH, a few foresighted people, put epidemiology into the administrative framework of research of our government in such a way that it can't be removed and it's on an administrative equal basis with clinical and laboratory research—though, the funding is not equitable and the attitudes are not equitable. But administratively, epidemiology is there now. You can't have a cancer center unless you have good epidemiology, and so on.

Yes, we do get money. I've tried at various times to get figures from NIH to assess how much goes into—"preventive research," but they fake all sorts of things, even Henry Buchwald's surgery to reduce obesity would go into preventive research, and that's not acceptable. [chuckles] So you can't get good estimates in proportions of the money, but, certainly, the proportion is still relatively low for preventive research in epidemiology and even clinical trials. But, we're doing better. It's a long, slow process.

We reached the peak, probably, in the 1980s when I was able to get the Minnesota Heart Survey going and, then, the Minnesota Heart Health Program going. We got a huge grant that people on campus here were just terribly upset about. Those huge community studies don't exist anymore. The Woman's Health Initiative is probably the largest one that's ever really gotten off, and it's plugging along very well. That's a major issue in medicine. We've done far better, but still a long way from being on equal footing.

DT: You mentioned that people on campus, when you won these massive grants, were upset because it was going to epidemiology and not to something in medicine or the humanities.

HB: Exactly, yes.

DT: Going back to the 1950s when you first got involved with the Physiological Hygiene Lab, can you talk about what it was like being in the lab at that time, what it was like to work with Keys, kind of the atmosphere of things?

HB: Yes. I want to go into everything you ask me but some of these things are more effectively brought out in some of the material that you know about: the Ancel Keys documentary, things like that where people describe the atmosphere and people who were there. I wasn't there at the beginning. I was there from 1953 on; whereas, they probably started in 1939 and 1940. It's on our history website.

The Lab was unique and we enjoyed our uniqueness. At that time, we were relatively small, and we were all, though working in different facets of a problem, working on the same general problem, cardiovascular function, which turned into cardiovascular function under stress, which turned into cardiovascular disease in the population. So it was predominantly cardiovascular oriented, physiologically based. Everybody was working toward a common goal. Tea time in the afternoon where one table was big enough to hold the staff, and considerable social life. It was one of those remarkable situations and there have been a few of them in the world. I think of the Harvard Fatigue Laboratory; I think of other places where there was this feeling of it being a vibrant center at the

beginning of an exciting field. And, as I say, we didn't have a lot of responsibility to the University, other than doing our own work. We had to support ourselves; they didn't give us support worth spit. But they would leave us alone. That's changed over time. So it was an active, stimulating environment. Though we didn't attract a *lot* of young U.S. doctors, we had a steady, small stream of them, and we attracted many, many people from overseas—this was a mecca in the field—largely from Japan and Scandinavia and Italy.

DT: Do you think that ability to attract international physicians was because of the Seven Countries [Pilot] Study, that you were going out into these other countries...?

HB: It happened even before that, because of the international issues we were dealing with and the reputation Ancel and Henry Taylor and Joseph Brozek and others had established. Yes, clearly, when you're one of the two or three centers in the world in a new field, you're obviously going to attract people.

DT: Do you think that tension we talked about earlier between medicine and public health was international or do you think...?

HB: Oh, yes, very much so; although, there were remarkable pockets where things were different, as in Scandinavia. Yes, the most vocal resistance to *all* of the ideas about prevention, particular diet/heart theory, was in the UK [United Kingdom]. I write a good deal about that and I can send you some of this. That's where clinical investigation was born, with Sir Thomas Lewis and all his students. I came to know most of them. Jerry Morris, George Pickering, and John McMichael, all were his number one handpicked fellows, and they all became distinguished in medicine of one sort or another. Some never accepted the idea. I have a South African friend, Gerald Shaper, who worked for many years in London, who said, "Henry, it's not scientific. It's very little scientific." If it's American, they don't like it. If it's cholesterol, they don't like it. Cholesterol is American, so they don't like either one.

DT: [chuckles]

HB: You know, these sorts of things.

How we get into that is something that I'm trying to analyze in a couple of chapters I'm doing. What led to this? That white plastic case there is full of 150 letters of exchange between Fritz Meijler, the leading cardiologist in the Netherlands, and Howard Burchell. *Wonderful* correspondence over forty years of modern medicine. Fritz Meijler wouldn't have *anything* to do with what we were talking about. He'd get furious when the Dutch Heart Foundation would tell people what they should eat. [chuckles] It's just attitudinal and it's pervasive and, as I say, when there's an exception to it, it's *marvelous*, when you hear a professor of medicine getting your message it's time to move on. That happens occasionally. McMichael wrote that professional action should be taken against people who tell mothers what they should feed their babies! They were just incensed by the

idea. [laughter] It's a rich field and I understand little about it, except that I think it mainly has to do with what you grow up to respect.

DT: It's interesting that the UK was so resistant.

HB: Oh, yes.

DT: I was wondering...in countries where you have a more socialized system of medicine, of health care, that they'd have an extra stake in promoting prevention.

HB: Well indeed, but the elite held out in the U.K. They went to *unusual* efforts. They even stopped publication of a report called the COMA [Committee on Medical Aspects of Food Policy] Report, which was just advising the physician population, not even the public, on how they should try to prevent cardiovascular diseases. They got an injunction against printing it for a while. It was an unbelievable thing—not only the personal attacks. [laughter]

I gave a talk at the Paavo Nurmi Symposium [in 1969], the famous Finnish miler [runner] in Helsinki, and Sir John McMichael was there. He was chairman of the meeting. After I'd given my talk, which was a very straightforward analysis of the predictive value of the electrocardiogram adjusting for blood pressure, cholesterol, age, and so forth, he got up afterwards and he said, "What you've heard this morning, ladies and gentlemen, is pure entertainment. It is not science." That's how direct and antagonistic they were. Remarkable.

DT: They had a very narrow conception of what is science, what counts as science.

HB: Exactly. What is science? Epidemiology and statistics certainly are not science, in their view.

[chuckles]

DT: It's interesting that that didn't change once the randomized control clinical trial came as the gold standard, because that was built on statistics and a lot of their methodology.

HB: Absolutely. It's a fascinating era. I'm writing now about development of multifactorial risk and computation. Yes, it revolutionized our ability to compute, to demonstrate a difference of a certain amount with a certain degree of confidence. Well, now, the randomized clinical controlled trial became the platinum meter. As you say, it's illogical. That doesn't apply to other aspects of observation or anything else.

DT: What was Gaylord Anderson like?

HB: Just as an aside... I think that the book you know about, the J. Arthur Myers book, [*Masters of Medicine*] pretty well describes, though without a lot of detail, the origin of the School of Public Health.

Gaylord, of course, was the first head of the Department of Preventive Medicine and Public Health and the first dean. I knew him in the sense that he welcomed me as a young up-and-coming physician who would join hands with Keys. He was also a guinea pig. Ancel had enlisted the 300 top people in the Twin Cities, heads of industry and deans at the school, the governor, and so forth in the Minnesota Professional Men's Study that we're still working on fifty, sixty years later. He was one of our guinea pigs, so I saw him at least once a year in hospital garb, examining him to put him through his paces on the treadmill and so forth. Quiet, gracious, dignified, thoughtful, interested in *you*, and convinced that what we were doing was worthwhile, and that means a lot when your dean thinks that, because he has to defend you against these other attitudes. He gives you a lot of leeway, a lot of independence in functioning. I can't imagine anyone ever saying a word about Gaylord that wouldn't be admiring of his low-key, effective administration. Becoming a department head myself eventually, I had very little idea of how good he was, but he was able to discern who was going to do something for the school. I'm sure he recruited well. There weren't a lot of funds to be had at the time that he was dean. He was just a marvelous person, and he had a national reputation and certainly put us in good stead. He had been, as most such people have been, involved with state health departments and that gets you on the side of the public health people, as well as the academic side. I don't know how much of a scientist he was. I can't even cite you any articles he wrote. He was gracious and kind and a low-keyed, effective leader.

DT: What was the relationship between the School of Public Health and the Health Department during the 1950s?

HB: Boy, you're going to find that I really don't know a lot about the early School of Public Health. As I say, we were *happy* for our independence. We were happy for our displacement. We didn't have to fight surgeons for space over there. We had our cubbyhole. It was vermin infested. I didn't, certainly, as a young research fellow or assistant professor, know much about the School of Public Health at all—and I still don't know a lot about it. I think that's been a problem throughout in our schools, the identification with the public health community, and each new dean tries to improve on that, and I think our current dean [John Finnegan, Junior] is probably doing as well as anybody ever has. Are we really meeting the needs of the public health community in terms of training, in terms of ongoing service, in terms of post-doctoral short courses and that sort of thing? I think we have a long way to go on that.

I don't think there's any real town/gown issue, as there is in the Medical School. That's really been a terrible thing for our Medical School. People can't wait to get out of here and get to a practice and see if they can beat the University in doing whatever they want to do in medicine. I don't think that exists in Public Health. That's probably a problem for any school of public health. It becomes more and more academic and more and more trying to be a research institution and more and more dependent on grants and research

and less and less trying to produce practitioners. Isn't that true in journalism? It's true in almost anything. Academic institutions get into that mold, and they are not as aware of the needs of the profession as they should be.

DT: My sense was that neither the School of Public Health nor, specifically, the Physiological Hygiene Lab was getting much money from the state in the 1950s and 1960s.

HB: Or now.

DT: Or now. [chuckles]

HB: Our division was always between five and ten, sometimes fifteen percent funded by the state. I don't think it's any different now. I think the School of Public Health still earns eighty-five percent of its keep. That little bit from the state is crucial as far as certain guaranteed appointments and resources. That's going to be cut back this year and next, and we're going to have trouble. It's always been that way. We're on our own.

DT: I appreciate your saying you may not have a lot of information about the School of Public Health, specifically. But, I'll ask you and if you can comment...

HB: Sure. I was here in the 1950s.

DT: [laughter]

HB: I was alive.

DT: The School of Public Health is quite fascinating for all the different divisions that exist within it and how professionally diverse they are.

HB: Yes.

DT: I wondered what was your sense of how the different divisions in the School of Public Health in the 1950s and 1960s related to one another. Was there much engagement between the different units?

HB: You'll get more from others, the people who have been heads, that you will be interviewing I'm sure. I think it's only in more modern times that there's been an active relationship between division heads. We're still division heads in the School of Public Health. They collaborate. But, as you say, their missions are totally different. Health Services research has almost nothing to do with Environmental Health and has a little to do with Epidemiology, not a lot, and so on down the line. At one time, we had Public Health Nursing. All the little units that couldn't function on their own came in to us. We got Public Health Nutrition. We got Maternal/Child Health. All these things came in here, because we had an effective administration and could give them strength that they

didn't have as a small unit. I don't think in those days, which you're talking about, that there was a lot of active collaboration. But I really wasn't in a position to know.

DT: That's interesting, though, that, here this really significant unit, within the School of Public Health, as you say, is physically separate but, also, for all other purposes fairly separate from the rest of the school.

HB: Yes, well, you'll find that there's *not* a lot of correspondence between Ophthalmology and Obstetrics either in the Medical School.

[laughter]

DT: That's true.

You didn't get a sense that there was some kind of hierarchy among the divisions?

HB: No. No, I don't think so. Some people had that feeling about *us*, particularly during the 1980s when I was head, and we had thirty-five or forty faculty. We had as many faculty as the rest of the school and we had a budget, and a lot of people resented it. I kept saying, "The solution for that is not for us to get smaller. It's for you to get more faculty, get more people." [laughter] That has gradually begun to happen. I don't think there's any hierarchy now. In fact, I think we are reduced to about four divisions now.

DT: Another thing that's fascinating about [School of] Public Health that you don't see in the Medical School is that you have different professionally prepared individuals. You have MDs, Ph.D.s, engineers, hospital administrators.

HB: Yes.

DT: There was no hierarchy based on profession either then?

HB: No. That's still a problem in the Medical School. A Ph.D. in the Medical School just has a devil of a time getting appointed and getting salary increases. It's *shocking* in some cases, as important as they are in cancer and everything else, for example. I can't say that there was a high degree ever of collaboration. We had more collaboration outside of the School of Public Health than we had inside the School of Public Health. We had collaboration with Nutrition, with Journalism, and other units. But hierarchy, no.

DT: In the 1950s and 1960s, the organizational structure of the health sciences was quite different. You had the College of Medical Sciences of which the School of Public Health was a part.

HB: A department within Medicine.

DT: Yes. What was your sense of the status of the School of Public Health within the College of Medical Sciences?

HB: [pause] I have not much idea. Largely ignored, except when they needed a little of our space, or except when they needed our temperature and humidity-controlled rooms for certain experiments on the treadmill, or underwater weighing, or when they wanted us to recruit subjects for them or when they wanted to use statistics in their study. Sure. Things went on that had to do with the sharing of a particular problem or research. But, largely, they ignored us, and we were reasonably happy with that.

DT: [chuckles]

Do you recall when Bob [Robert] Howard was appointed as dean of the College of Medical Sciences?

HB: Vaguely. He was a very important dean for a very important period. He did a good job. I knew Bob, but I forget the exact year that he came in.

DT: He was appointed dean in 1958 and, then, left in 1969, 1970.

HB: Well, that's a big time in everything, isn't it, 1968? Yes, I think he was generally supportive of us. In 1969, you say he left?

DT: He left around 1969, 1970 when the health sciences was created and Lyle French came in.

HB: I'm trying to think who was dean when Ancel retired in 1971, 1972.

It's an aside. There was a very strong movement by some of the leaders still around that didn't regard what Physiological Hygiene was doing as very important when Ancel retired. It was required retirement at sixty-seven at that time. It's a one-man laboratory, so it should *stop*, just as a medical research unit in Britain stops when the guy retires. The institution disappears. Terrible. There's certain amount of truth to it. It might not have the same flare or the same impetus or the same leadership. So there was a real movement. The issue was whether they were going to replace Ancel Keys, and the Medical School, which had nothing to do with this, was all against continuing it. They could see money. They could see state funding. They could see space.

Then, we get to the issue of Dean [Lee] Stauffer, finally offering me the job [of head of the Laboratory of Physiological Hygiene]. That's another period. No, I don't remember much about the details of the Medical School and Bob Howard, but I think we went forward very effectively during that time. I think the building was already built. The Mayo Building was already built by that time. He was a strong leader.

DT: I know he confronted some challenges during his appointment, particularly around the faculty practice issue.

HB: Yes.

DT: Did you have any perspective on that?

HB: No. I don't know, except that under his administration, it went wild, and it's still wild. I don't think we want to get into that issue, the conflict of interest on this campus. I just can't talk about it.

I was head of a task force in the 1980s which looked at academic/industry relationships and every principle and guideline that we outlined was violated, and people lost their jobs, and the University lost its reputation shortly after that because nobody paid any attention to our report. We could have prevented all these things that happened—and they're still going on. Now, with the *big* drive to rapidly convert University ideas into products and procedures, the whole guide now to bring industry closer is gone. It's an insidious influence on training, emphasis on funding. It's a problem.

DT: Another issue that he faced—I wonder, again, if you were aware of this—was there was a push by local physicians to have a second medical school established. This was going on, basically, in the 1960s onwards. It speaks to this town/gown issue that you referred to a little while ago. Were you aware of that movement?

HB: I was very aware of it. I wasn't particularly involved. The leader of that movement is still alive in Saint Paul.

DT: Davitt Felder.

HB: Davitt Felder. He's a wonderful guy. He's a clarinet player, the only reason I know him. He's still intact "upstairs." It would be worth...

DT: I actually interviewed him.

HB: All right. Fine. You're ahead of me. He could express to you far better than I can how much the town felt about the gown. I don't know how much that motivated him. I have no idea why they wanted a medical school. Mayo could possibly justify one. Duluth could possibly justify a two-year school. It's interested in rural medicine. But why those doctors wanted to do this in St. Paul! I wasn't in on it. Most of us around here were horrified by the idea.

DT: Really?

HB: I would be interested to read your interview with Davitt Felder and see how he justified it. [laughter]

DT: Yes, definitely. It's very interesting.

HB: I think that he thought, "We're going to beat those guys at the University." I don't know what it was. Were we stealing their patients? Were we elitists? Did we think we

were the only...? One attitude was that we're going to do it better than the University and, in some cases, they've done it better in the community hospitals.

DT: I know you spent your career here, but I wonder if you have a sense of whether those same kinds of town/gown relations exist in other cities or if it's something distinctive about Minnesota?

HB: I don't know, but I think they're universal. They're even worse in Europe. Here, I think we make some effort to reach out in the community and the community needs and to not steal patients. But, in Europe, of course... I think it's universal. I don't think it's probably any worse here than anywhere else.

DT: Beginning in the late 1950s and into the 1960s, there were national concerns about health manpower, basically a shortage of physicians and practitioners.

HB: Yes, absolutely. There was money available for a new medical school.

DT: Yes. I know that the Medical School here benefitted from those federal funds that were available for increasing student enrollment. It also seems that that manpower concern was related to public health, too, that there was an effort to increase public health workers?

HB: Absolutely. I think you'll get that from Stauffer, because it was during his period that the public health training grants were established. I'm not intimately familiar with that, but, yes, it was very important. They're not entirely gone. We have training programs now, NIH primarily, but they're struggling.

DT: It seems that because of these federal grants, there was the Nurse Training Act in 1964 and...

HB: Right.

DT: ...the Public Health Training Act was in 1956, I guess. There was so much federal funding around and there had to be a commitment to increase student enrollments. I know the Medical School expanded its enrollment significantly during that decade and that, then, you had the Health Building Program to increase space.

HB: Right.

DT: Did you feel that the Physiological Hygiene Lab actually got to benefit from this increase in enrollment and funding that was available?

HB: Almost not at all. It was a theme in J.'s [J. Arthur Myers] book. Ancel's constant complaint was that he was assigned to these temporary quarters in 1939 or whenever it was—and they're still temporary—and that they produce millions of dollars in overhead,

indirect costs, and they hadn't gotten a penny back. That was true. We never got any space until I was director.

That's an interesting little story. It shows the problem. It's sort of an aside. There was such a thing as a legislative special that if a University professor had a marvelous idea the Legislature would actually vote on it and give the University a little money to develop this idea. We presented our idea of community programming, three intervention communities, three control communities in Minnesota to teach people how to live and avoid heart attacks, and the vice president Lyle French and his administrative assistant, Dave... I'm forgetting his name.

DT: Preston?

HB: Yes...reviewed it and reviewed it with what other staff they had and said, "This isn't going to fly, Henry. We aren't interested." When we got the multi-million dollar grant the next year from NIH, they came over and the first thing Lyle said was, "Henry, we're here to eat crow and we're here to build you whatever you want."

[laughter]

HB: So we were able to add 10,000 square feet under the stadium. It was all torn down ten years later. Anyway, it was the first time the laboratory got *any* benefit of *any* of this was when we got this commitment to use our indirect cost to build the space that we needed to carry out this multi-million dollar project...just to show you how things work out. We really had to support ourselves. Money talks!

DT: This issue of indirect cost, it's something that I don't think is fully appreciated. You might get this big federal grant, but it doesn't cover the maintenance, overhead, and salaries of other workers. How did you pay for those indirect costs before Lyle French and Dave Preston...?

HB: There were indirect costs from the beginning for any government grants. They were twenty or thirty percent. Now, it's fifty percent. All the funding since I took charge has been predominantly NIH. We've not gone after private funding, and Ancel didn't have much private funding either.

DT: So, then, to cover those indirect costs, you have to go your general operating budget?

HB: Yes.

DT: Is that where the ten to fifteen percent of state money goes, straight into that?

HB: Yes, to support your chief administrator and one secretary.

DT: [laughter] It seems like it would be tough to find enough money to cover those costs.

HB: It's tough now, because the University auditors are not allowing us to put certain things on indirect costs, our computing, for example. It's a very complex issue. I don't know what you're going to go into. I think the town and gown is more interesting.

DT: [laughter]

HB: With indirect costs, you find yourself in trouble with everybody.

[laughter]

DT: It's just something I've been toying with in my own work...

HB: Of course.

DT: ...looking at kind of increased federal funding of medical research. Well, that's great, but it also pulls money away from education...

HB: It does, indeed.

DT: ...because those general operating funds are used to pay for education, as well.

HB: I'm afraid so.

DT: Let's talk a little bit more about what it was like to work in the stadium. That space was cramped, it sounds like. Can you describe a little more about what it was like?

HB: I don't know what you think about this campus. But can you think of anyplace on the campus, except perhaps the Wangenstein [Historical] Library [of Biology and Medicine] that you like to be in, that's cozy, that's comfortable, that's attractive? I think it's one of the coldest places on earth. Think of Cambridge, Oxford, and Harvard Square. Where is there any place, even in the summer time...? That formal mall? It didn't do much for me. Well, it was cozy under the stadium. We were very happy there, and we've been displaced persons ever since.

[chuckles]

HB: That's all I can say. We felt cheated, but we felt happy in our cozy little place. We made it go. It was fine in a functional way. Sometimes, it would flood. The heating worked, more or less. Because there was intellectual excitement, we came to work happy with what we were doing, and it was a very collaborative group, so we weren't terribly bothered or terribly envious of other people's spaces, but Ancel worried about that. He complained about it. Otherwise, I mentioned that neither Gaylord Anderson nor Lee

Stauffer rolled their eyes very often at what we were doing. They gave us a great deal of leeway, including travel.

We're getting onto other areas, but my function from very early on was to increase our contacts with the School of Public Health and to increase our responsibility of trying to become, eventually, a well-balanced academic unit, which we finally *had* to do when we became Epidemiology.

DT: In the 1960s, the lab was part of these big multi-national studies?

HB: Those multi-national studies didn't really start until the 1970s. I describe something I called "The Pause" in my book between the 1968 and 1972, when NIH and the world couldn't make up its mind what the research policy was going to be in our area of diet and lifestyle and heart disease.

DT: I was asking you, basically, about some of the research projects you were involved in in the 1960s.

HB: And I was indicating to you that the big ones didn't really get started until the 1970s. The 1960s, the multi-center trials began. There was the University Group Diabetes Program. There was the Coronary Drug Project, which was a national multi-center thing. We had started our Seven Countries Study. Yes, you're right.

DT: You must have spent a lot of time travelling with the Seven Countries.

HB: I think I looked at it once for income tax purposes. For some of those years, I was out of town 100 days a year. I'm sure my wife then could have accounted for that better than I did. [chuckles] I was gone too much. I had to run these surveys in various parts of the world. Ancel would come for two or three days and leave me behind to keep things in order. Yes, too much.

DT: You mentioned earlier that you didn't have a lot of teaching responsibilities. It's just as well, given that you had so much research responsibility.

HB: It was a very intensive, focused period on those pioneering areas both in observational epidemiology and, then, the clinical trials. The Coronary Drug Project was the first big multi-center trial NIH ever did and a model one that hasn't been followed since appropriately.

We gradually assumed more responsibility for teaching in the Laboratory of Physiological Hygiene. We offered a course in Public Health Aspects of Cardiovascular Disease. We taught some nutrition courses. But when I took over, we really tried to integrate with the school program, and, then, when Epidemiology came on, we had to initiate teaching programs in all the areas: cancer, infectious disease, and so on.

DT: That teaching was within the Public Health...?

HB: Right.

DT: Was there any teaching in the Medical School curriculum?

HB: The battle to get on the Medical School curriculum was a bear from the very beginning. We had to fight with the biochemists and the physiologists. I think during these fights, we came down from 110 hours to 10 hours over a period of twenty years. [laughter] We ended up with a course on something about humankind. I don't know. It was something the students didn't like. Yes, we were, theoretically, responsible for teaching them epidemiology and statistics. I have no idea how they ever passed their exams in it, because we didn't teach them. I really have no idea how they passed their national exams in it. That would be worth looking into, because they didn't have courses in any of those in Public Health, Epidemiology, or Statistics. Yet, they were tested in these areas. That was supposed to have been our responsibility, but we lost out in the battle as cell biology increased and molecular biology, all these things. They kept cutting us down.

DT: It seems that the Medical School curriculum was being revised from 1960 through the mid 1970s. There was a lot of wrangling over this time.

HB: Yes, and we lost out during that period. I must say, we didn't fight terribly hard. We were perfectly happy to do our thing and the medical students weren't interested anyway.

DT: That kind of raises the question of... There's a shortage of public health workers, but medical students aren't being exposed to it. How often do you get medical students that, then, want to go into public health?

HB: Almost never. Where we get them now is they, all of a sudden, realize, if they're interested in academics at all, that they've got to know how to do clinical research, and they have to know how to think and propose hypotheses and do sound methodology and know statistics. So my colleague Russell Luepker has been very active in developing the Clinical Investigators Program for the campus. In recent years, it has not done very well, and it hasn't gotten a national center, but it still is a good course. So we're getting people at the fellowship level and post doc [doctoral] level that want to be researchers and don't know how to do research, so we teach them how to do research.

DT: [chuckles] That's kind of ironic given that, earlier, you didn't do science. Public health wasn't science.

HB: But now it is the science of numbers, where, before, it wasn't. We must have at least a half a dozen MDs around here getting MPHs in epidemiology.

DT: In the 1960s, again... You mentioned that rather than having a lot of collaboration between divisions, you actually had more collaboration with groups outside. You

mentioned Journalism, but I wonder, did you have much relation with Nursing, Dentistry, Medicine, the Medical School? We talked a little about the Medicine.

HB: Are you talking about Physiological Hygiene or the School of Public Health?

DT: Either.

HB: Well, the development of the Nurse Practitioner Program and Public Health Nursing was very active and a vital thing. I've never really understood how we lost that and they went back to Nursing. They really had a very fine reputation, great people, and national leaders. I guess I don't know... I guess I do know; they couldn't support themselves and all the academic activity and research, so they had to take that back into the School of Nursing. No, there wasn't much collaborating. We got Journalism because we had these community projects and we had to learn how to communicate.

DT: [laughter]

HB: That's how I met my wife.

DT: Oh, really?

I actually took a public health communications class in graduate school, and it was in communications rather than in public health.

HB: John Finnegan was our communicator, and he became dean.

DT: Do you have a sense of when the reorganization of the health sciences was taking shape in the late 1960s...? What was your attitude towards that reorganization and that of your colleagues? Do you have any perspective on it?

HB: Not really. We didn't understand it. We all respected Lyle French, and I told you my little anecdote dealing with him. We weren't involved. They didn't ask us and we didn't insert ourselves. We really weren't involved.

DT: So you don't feel like it had much effect on faculty?

HB: In Public Health... I didn't recognize any issue there. No, I guess I can't say that we understood it or that we were involved with it, particularly.

DT: One of the rationales given for the expansion and reorganization of the health sciences was to incorporate a team teaching approach or the team concept within the health sciences. Of course, Public Health already does that. I mean, it's a team concept manifest. Did you get the sense that there was more collaboration between the health sciences after the reorganization, more team teaching, team practice?

HB: Not in any practical sense. Lyle French was very visible to us; whereas, the Medical School dean had been a very distant figure. So we had somebody to go to. But it had absolutely no effect on our teaching strategies or organization, nor did we have any effect on them, as far as I know.

DT: There seemed to be no effect on the status of the School of Public Health, vis-à-vis the rest of the health sciences?

HB: I don't think so. I'm not aware of any.

DT: That's interesting, because part of it was to equalize or give equal status to the different health science units so that everyone had their own dean, as opposed to previously where it wasn't. But it just doesn't seem to have had much bearing on Public Health?

HB: I don't think so. That was Lyle French. I don't know how tight an organization he ran. I have no idea how often he met with department heads. Certainly, he didn't after I became division head. He never met with us in any formal sense. No. Well, [Frank] Cerra hasn't either. Cerra comes around, talks to us once or twice a year. I don't know that there's any regular contact or sharing between the deans and the vice president of the Academic Health Center. How that's going to be affected *now* when these deans are reporting to the provost and not to the dean of the Medical School, I have no idea how that's going to work.

[chuckles]

HB: I see all sorts of interesting complexes...conflicts of interest, actually.

DT: Yes.

HB: How can the Medical School dean be vice president of the health sciences and give fair treatment to everything he's responsible for?

DT: That was, indeed, potentially the issue in the 1960s...

HB: Yes, yes.

DT: ...when you had Howard as dean of the College of Medical Sciences. It's an interesting situation.

HB: Yes.

DT: What were attitudes about the appointment of Lyle French as vice president?

HB: Ummm... All I can say is that everybody respected him as dean, and knew that he would be fair. I experienced some discriminatory behavior, but he apologized for it and

came with his hat in hand. So much depended on the person. What it did as far as integrating the schools, in terms of decision making, I really don't know. I wasn't there.

DT: You alluded to this earlier. Probably one of the most important things for the School of Public Health was the retirement of Gaylord Anderson.

HB: Yes.

DT: Were there concerns about what would happen after Anderson retired?

HB: Sure. I'm sure Lee Stauffer felt very insecure and junior in that relationship. It turned out fine. Have you talked to Lee yet?

DT: No, I haven't.

HB: He's well enough now. I went to see him in the hospital a couple weeks ago. He had his hip done. He's the one who can respond to these questions more effectively than I can.

DT: Were you party to any of the discussion about the appointment of Stauffer?

HB: No, this was before I was division head and I just wasn't involved. I was tenured faculty, but I don't think tenured faculty played any role in the choice of dean.

[laughter]

HB: I'm not even sure they had the same formal national searches that they do now. At that time, I think Lee was a convenient internal appointment.

DT: I don't know how national the search was, but there was a search.

HB: I didn't know how they ranked the candidates.

DT: I don't know how exhaustive it was, but there is certainly documentation and discussion about the ranking of candidates.

HB: Good. At least Staffer competed against those.

DT: Yes.

He was, I guess, universally, people's top pick or second top pick.

How did you and the lab respond to his appointment as dean? You say it was a good thing.

HB: He was appointed when I was overseas on a sabbatical, and that was also the time that Ancel's retirement was envisaged, and that was the time that there was the move to eliminate our existence. So I was interested in all of this, but I couldn't be very much involved. I didn't know Lee Stauffer from Adam. He was a sanitarian on the Environmental Health faculty. So I did not know him. I had no contact with him at all until after I came home from sabbatical.

I did do the only political thing I'd ever done in my life for myself. I called Al Sullivan, my lifelong friend, and I called Richard Ebert in Medicine, and I called Howard Burchell for whom I had worked for several years as associate editor of *Circulation*. I told each of them—I called them from Geneva [Switzerland]—"It looks like they're going to sink the laboratory and they're going to acquire its resources, but they're also thinking about appointing somebody, and I'm on the list. So if you have anything good to say about me, now is the time to say it." They all responded and, Lee, not knowing me from Adam, was very receptive to their comments. So, he offered me the job with one hand and took away Ancel's \$40,000 salary with the other.

He's always regretted how he hired me with less enthusiasm than he should have, because, within a year, I had a major budget and an effective program. [laughter] But, we've been very good friends. Nobody knew Lee, except people in his division, Environmental Health.

DT: What was his rationale for taking away the \$40,000?

HB: Oh, he wanted to develop new space in his program, and he wanted to give money to Occupational Health, things that he understood. He didn't understand us. Yes, you can stay over there in your rat trap. He used his new dean prerogative to remove \$40,000 of our state funding. That was his prerogative. [chuckles] You'll have to ask him.

DT: How did you come to terms with that? How did you resolve that...less of your money? How did you kind of subsist with a lower budget then?

HB: I forget how much I was supported by research funds. We were a minimal operation. Epidemiology was down to two people at the time I took over and the laboratory was down to three people out of a \$220,000 a year budget. We were nothing; we were nothing. There was very little chance we would become viable. They took a chance.

DT: You did a remarkable job, then, of building up the Laboratory.

HB: Well, I came on at the right time. I'd been involved in all the developments and planning for the new NIH policy on cardiovascular research that was announced in 1971 and 1972, the Multi-Center Studies, development of a research center into a multi-risk factor intervention trial, development of the Hypertension Detection Program. All these things that came along in 1972, I called the "generation of big trials." We were there, we

had the experience, all I had to do was put it down on paper. Everything came at once. [chuckles]

DT: With so many research grants and projects going on, did you have to increase your number of faculty and staff?

HB: Yes, and in a hurry. That was real fun. I'm still working with the first group of people I recruited—some of them are still here—in 1972-1973. Then, I had a new wave in 1976 when we got more big money. Yes, that was a period when you didn't have to be a genius, but you had to be hard working. You had to have a reputation and put it down on paper. It was easier then. It's tough now.

DT: Yes. You were able to get new faculty positions then with that?

HB: Yes, mainly non-tenured. I had to get medical help right away because all these programs involved people that had to do examinations as well as research, and I had to have people who had research potential as well MD licenses. We already had a reputation for pretty good work here. I had no problem. Minnesota has really profited from the brain drains from the West Coast and the East Coast. I had a lot of people from Harvard, because they saw that they were *never* going to be anything more than assistants in Boston. We got some of the best people in the world from Harvard and from Stanford just because of the painfully slow academic process, and we offered them the world, and they opted to come.

DT: With that pulling in of so many new people to the University, people, the school, and the health sciences more generally, did they start paying attention to what you were doing?

HB: Oh, yes. Sure. As I say, the vice president came with his hat in hand. Money talks in this place. Money may be the only thing that talks. There's no question about it. It was a good time. It was a golden age. The 1960s and 1970s were a golden age in epidemiological research and prevention research and public health research.

DT: At the same time that the lab was able to bring in these federal grants, in general, the Federal Government was trying to reduce the amount of money it spent on medical research, and there were significant budget cuts, particularly towards the end of the 1970s. How did this impact you in the lab?

HB: [pause] I've been reading a little bit about the heyday of the 1960s at NIH. It was only in the late 1960s that Congress began to get a little tired of dumping all this money into research and began asking questions. Then, there was some stupid grant in cancer and skullduggery. I don't know. Yes, you're right that it became more critical, but public health policy, NIH policy, on cardiovascular disease really took off in 1972, and that sustained us, really, well into the 1980s. It's only since then that things have been lean. We had lean periods, but sometimes our lean periods coincided with other's good periods, and our good period coincided with other's lean periods. I very seldom had to

let anybody go and almost never had to fire anybody, or persuade them that they would do better elsewhere. We were very fortunate in our resources in the 1970s and 1980s.

DT: It seems like that within the School of Public Health, your lab may have been unique in that sense. Did you have any awareness of how the other divisions were affected by these budget cuts?

HB: No. Biometry, we called it then, Bio-statistics, prospered, as we did, for the same reasons. In fact, we got them involved in some of the major multi-factor studies. Jim [James] Neaton's marvelous center now is one that we started with the multi-risk factor intervention trial in 1972. Minnesota bio-statistics started growing then—*much* against the desire of the program to grow. [chuckles] They wanted to stay small. They wanted to be consultants. They are very active, international leaders in trial coordination now. That all started with their grants. So that's the only thing I knew about. I had no idea how Environmental Health was doing. Health Services research was really struggling, but they came along very strong in the 1970s. I guess I'm not terribly aware of problems, except, as I say, we took over some of the programs that, obviously, couldn't sustain themselves. We even took over Clinical Psychology in the Medical School and acquired its faculty, as well as Maternal/Child Health, Public Health Nutrition, and Community Health Education. We took five or six small units. We were *much* affected by that, in that those units could not stand alone, and there was no other natural integration of them with those other divisions, so we took them all.

DT: Was that after the merger with the new division of Epidemiology?

HB: No.

DT: This was before that?

HB: Before that, yes. All of that happened in my administration from 1972 to... I was head of the laboratory from 1972 to 1983, and, then, head of Epidemiology until 1990. Almost all of that took place before we were Epidemiology.

DT: I don't remember if I read this in your memoirs or if this is something that I saw from the archives, but it seemed like there was a shift in the School of Public Health's priority from an emphasis on prevention to more of a research focus on health care delivery in the 1970s or from the 1970s onwards.

HB: That's when that center developed.

DT: The Health Services Research [Center]?

HB: Yes. I think that was also a period that prevention was expanding rapidly. Yes, that was a big thing. You'll probably interview John Kralewski and Bob Kane. That was a big shift, and they were recruiting, and they were growing at a time when HMOs [Health Maintenance Organization] were forming. Yes, they were very influential—still are very

influential on the national scene. But I wouldn't say that we shift from prevention to that. It's my old story. You've got to grow. You've got to grow. But we in Epidemiology don't have to shrink.

DT: Yes. Do you know why there was this new emphasis on health care delivery? Was it specifically related to the HMOs?

HB: Yes. You know all that started here, the HMO, the concepts, the ideas. The dream started here. What's happened to it is something else. Yes, it started here. We were the center for that, and, then, they've moved very well with the field, with senior care and Medicare. They're the experts in this. We do have some collaboration.

DT: I've seen in the archives... I don't know if you'll be able to answer this question for me. I saw something called the Pilot City Health Project that was established in 1970. Do you know what that was?

HB: No. Stauffer can tell you all of that.

DT: Okay.

HB: Stauffer had to do with that, and he had to do with that and with the Community Health Center's. I can't tell you anything.

DT: How involved was Dean Stauffer in what you were doing in the lab?

HB: Oh, he was very much involved, more actively than Gaylord though Gaylord protected us and convinced the rest of the world that what we were doing in public health...and was happy to have us because there was very little research in the School of Public Health, except for us, at that time. Stauffer soon recognized what we were doing, and that we were interested in integrating with the school and contributing to training, so we worked very closely with him and [Edith] Leyasmeyer. Have you talked to Leyasmeyer?

DT: No. Is she still around?

HB: She's still around. She lives locally, I think, not too far away. She would have many insights. I shouldn't use the term "poor lady," but she was interim dean in all these changes. She was interim and, and, then, somebody else would come in and, then, she was dean again, and, then, somebody else would...

DT: Yes.

HB: Then, she was dean again.

DT: That's good. She was on my list of people to interview.

HB: She will have been aware of all these transitions and all the politics. [laughter] She was assistant dean under Stauffer, and yes, much more attuned to the politics than I am.

[chuckles]

DT: Do you have any sense of how the other division heads viewed Stauffer's leadership?

HB: I think everybody was happy he was dean. I don't know anybody that wasn't—well, Len [Leonard] Schuman wasn't happy with anybody. You'll have to talk to Stauffer about his relationship with Schuman. That's another issue, and we probably don't want to get into it...our relationship with Epidemiology and Schuman. Now, that changed when Schuman retired.

DT: When did he retire?

HB: Nineteen eighty-three.

DT: Oh, so right when the merger was...

HB: Yes. They were down to two people: Jack Mandel, who had recently gotten his Ph.D. and Robert K. Anderson who was about to retire. They had maybe nine Ph.D. students and forty MPH [Masters in Public Health] students, and nobody to teach them, and it had been getting worse over the years, and Stauffer was fit to be tied. So, yes, this was when we were going to take over, and we did, but Mandel was not happy with that.

DT: Did you have much interaction and collaboration with Epidemiology before?

HB: It was very strange. It was just like with the Medical School. We were happy. We were doing our thing; they were doing their thing. They were doing all the teaching. We were happy doing research.

But, eventually, over the years—this is a long story—it seemed strange to a lot of people outside. We had this marvelous summer course in Epidemiology that Len Schuman and his buddies in the East had formed, and it was a good course. Everybody came from around the world here for some weeks in the summer. All these people that regarded us as the center of cardiovascular epidemiology in the world would come here and find that Len Schuman didn't know us or we didn't know them. [chuckles] That was interesting and sometimes embarrassing. It didn't bother us.

Then, once, a week before the class was to begin in the summer, the person teaching CVD [cardiovascular disease] epidemiology in that summer course, which was very important, making money, had to drop out.

Schuman let down his hair and begged me to do it. I had just organized a national seminar on cardiovascular disease and epidemiology in Lake Tahoe—it's still going on

thirty-five years later—in 1974, and I couldn't do it. So another strike against me with Schuman!

But, then, it was a very natural thing for Stauffer to do...integrate Epidemiology and the Laboratory of Physiological Hygiene. We had faculty; we had thirty, forty people. We were eager to take on new issues. But Jack Mandel, who stayed behind, was not happy about it, so he went to Environmental Health. There were no vestiges of Len Schuman in Epidemiology from 1983 until two, three years ago when Bernie [Bernard Harlow] was appointed. Bernie had gotten his MPH under Len Schuman—not that we had tried to wipe out all of the past. There was nobody around. There was just nobody in the department when Len retired and only one person was there for a few years that had ever studied with Len. So there was just no residual of that long-term epidemiology tradition. It was all “our” history. The history that I wrote of the division was our history. Then, Bernie was appointed and, now, it's going to be a more balanced picture of where we came from, and it's perfectly natural, the integration of two distinguished chains or strains that became one. Either because of defensiveness or the need to control or something, Len never recognized me as an epidemiologist. He didn't recognize Henry Taylor as one of the outstanding field epidemiologists ever, also, a physiologist. I was an electrocardiographer, as far Len was concerned and Henry was a physiologist. Ancel, well, Len didn't know what he was.

Then, I hired people with Ph.D.s in epidemiology and he still didn't recognize them and didn't want them on his faculty. We could have helped him over the years. Finally, all was changed by his retirement. So there was no actual enmity, but there was no collaboration between our divisions. Then, we became one in 1983. Now, because our new head graduated from Len, we have a more “balanced” history.

[laughter]

DT: Why was Jack Mandel so unhappy with the merger?

HB: He didn't think he wanted to work with me. He wanted to be head. It could have worked. It would have taken him many, many years to develop the division, but Stauffer with us had a ready-made division, a lot of faculty were ready to go. So Jack felt cheated. He stayed several years in Environmental Health. He, eventually, became dean at Emory [University, Atlanta, Georgia] for a few years and, now, he's dean in Toronto [University of Toronto, Dalla Lana School of Public Health]. So he's done very well.

DT: Yes.

Did anything change once Stauffer stepped down and Leyasmeyer became interim dean?

HB: She did a marvelous job of holding things together. I can't say that we moved forward. You'll have to find out from her what she regards as what she did, but she held things together in very effective ways. The school continued to prosper because good people were in place. She knew everybody. She had a good relationship with Cheryl

Perlmutter, who became vice president for Health Sciences after Lyle French. You may find that she has a chip on her shoulder. She had a hard time working with the other deans and with the Medical School. She was not a medical person. She had a degree, I suppose, in health education or maybe health services. I'm not sure which. Anyway, she couldn't talk medicine, and it was unhappy when she had to. She really couldn't represent medicine or public health. She was just a fine person, fine administrator, and knew the campus very well, but it was hard for her to deal with these things. She probably didn't get the attention or respect from other schools that she should have. So it was hard for her.

DT: And that's why she was only interim; although, she held the position for over three years and, then, again later?

HB: Yes. It was always considered that she was interim. It's always hard to find somebody; it always takes two, three years.

DT: It's interesting that she had a tough time working with the other deans because she didn't have a medical background, but Stauffer, also, didn't have a medical background.

HB: That's right.

DT: But he had an easier time?

HB: He was a man.

DT: Yes.

HB: Fortunately, that's changing.

DT: You referenced, I think, that there was some kind of economic crisis or retrenchment in the 1980s.

HB: Yes.

DT: What was that like? How did that affect things?

HB: [pause] I don't know. I think Stauffer and Leyasmeyer will give you a better idea. None of this really affected us. We continued to do very well. We got our biggest grants in 1980. The grants are still going on. The Minnesota Heart Survey is still going on thirty years later. I got it in 1979. None of this really affected us. The only way it affected us is that the deans, the deans that succeeded Stauffer, tried to take as much money away from prosperous divisions as they could to build others or to build themselves. That was one of the reasons they didn't succeed. You'll have to talk to Leyasmeyer and others about how the school as a whole suffered. We prospered and that was always a little clumsy for us. People thought we were too big.

DT: When Robert Kane appointed dean in 1985, how did that, again, affect the lab?

HB: That affected everybody. You'll talk to Bob. He was a bright fellow and never administered more than three people in his life, as far as I know. He was with RAND [Corporation], the think tank, and he's a thinker. He really pioneered in senior care in this country, and it turns out he was a major researcher in this area, he and his wife, but he never deaned. He was academically elite, so what we were doing was fine. He appointed me Mayo Professor. At that time, that was only one person, and you held it for life. [chuckles] Schuman had been it first and, then, I was the second. Kane was concerned with academic stature. The way he developed that and enforced that was in a way that didn't make people happy. It made people who weren't necessarily in the top ranks insecure. So he affected a lot of people. I think the school went on very fine, and he developed a closely knit group of division heads. In fact, we were so closely knit that we threw him out, eventually. He really was responsible for getting us together, and we saw what the problems were, and we saw that many of our problems were what he was taking away from us. So we were responsible for him stepping down. He was academically for us and for quality, but he had no concept of what "Minnesota nice" was.

He had no concept that people have strengths—not everybody can be a three-striper—and that you should encourage people in their strengths and try to build up their weaknesses. Use them for what they're good for, the sort of thing that would make things go in Minnesota and any good leader does. He really established an evaluation system that we hadn't had previously, a more formal one. So it was tough. That's good if it's within a person who is thinking of your interest primarily as well as the school and not just personal interest. Every position that disappeared he acquired to his office one way or another. Every fund that was available was retrenched to him instead of back to the people who had developed the position or developed the funds. After while, nobody could take it anymore. But he was a strong dean. He just, eventually, created a revolution.

DT: It seems like, obviously, a very different leadership style than what had come before.

HB: Yes, exactly, far different from Gaylord and Stauffer. He didn't have the public health experience, but he was a brilliant person.

DT: Gaylord Anderson was a national and international leader in public health.

HB: Yes, absolutely

DT: That was not Stauffer's reputation when he became dean but, then, it seems....

HB: He was a good old boy, and he knew everybody.

DT: Yes, so it seems like a different priority, but, then, with Bob Kane, maybe wanting to have an emphasis on a national leader.

HB: Absolutely, and do it my way, top-down.

DT: Something that I noticed about Minnesota—I've seen it within the Medical School most—is the idea of an outsider, a non-Minnesota person. A lot of the people that I spoke to in the Med School were born and raised here, went to school here, and I know that there can be that provincialism. It's interesting that in talking about Bob Kane, obviously, when he was appointed dean, that was his first time in Minnesota....that there's something to that. That kind of outside perspective can be a downside, it sounds like.

HB: On the other hand, I, also, mentioned to you we profited from the brain drain from Harvard in our departments. But, you know, that's certainly true. Then, his successor was even worse. He came from the New York City Health Department and, by god, he was a Caesar, the czar of New York. Yes, not only outsider but different attitudes, whereas, Minnesota is "Let's talk it over."

DT: It's very interesting to see the role that local culture can play in an institution.

HB: Yes. It can be stimulating to get outside influence, but it can, also, be a disaster, as we found.

[laughter]

DT: This is kind of a different kind of a question. Within the health sciences, there were discussions about this in the late 1960s again, but, then, in the 1970s, this effort to increase the recruitment of minority students. I'm wondering if this reached Public Health and if this reached Physiological Hygiene and Epidemiology, and what efforts you saw being made to increase recruitment of minorities.

HB: I'll bet Edith [Leyasmeyer] would be the best one on that. Certainly, it affected all of us in our awareness, in our attempts, but it's the problem that our culture has had from the beginning. You can't start out from nothing and what do you call this program when you give people credits that they haven't earned in order to raise the... I forget what the political term is.

DT: Affirmative action.

HB: Affirmative action. We all tried. I was very happy to recruit a very fine young faculty member from Harvard, who was black, Richard Gillam, so that put me on the right side of this thing. Then, we always did pretty well with staffing. But mostly we've done a terrible job at Minnesota, not only is the system difficult, not only is the leadership not prepared. Maybe it's better now. I don't know how it is. But a lot of people don't like to live in an environment like Minnesota when they've been living in a more cosmopolitan environment.

DT: Did you have many women on the faculty?

HB: That changed when I took over. I created a lot of appointments for areas in nutrition and communications and social medicine and child programs. We had a very active youth program, and in the Minnesota Heart Health Program, I got an outstanding person, from Stanford, who is now leading an institute in Austin, Texas. Yes, I think that's changed. Again, it's not because of any bias that I know of. It's a question of people...and that has changed just dramatically. There were almost no women epidemiologists in the 1950s and 1960s, and, now, two thirds of the schools of public health are women, but that's what it took.

My wife won't let me have a segment on women in early cardiovascular epidemiology. I've got to integrate it some way without doing that. But I have a list, and I have Emily [Hagens, a graduate student in the Program in the History of Medicine] working on this, getting anybody that had anything to do with cardiovascular epidemiology before 1972. I've got three people I know in the UK and one or two in Finland, one in Spain, one in Italy, a few in this country. Well, there just weren't any Ph.D.s in epidemiology or any women in epidemiology in the 1950s. But, now, it's different and women are taking over the field, because it's a great field for them.

All that those movements for diversity did in Minnesota was make us aware and made us try. I'm never convinced that we put in any real time and effort as an institution. I think there's been an equal opportunity officer for some years, but I have no idea what they've accomplished. They usually left unhappy to get away from this "honky" place. So we haven't done an adequate job. We've done a good job with women, because women have gotten the education and are, obviously, tops in our public health field, but we haven't done well in minorities.

DT: I'm wondering if you have any comments to make about changes in public health practice, research, the culture that have taken place during your career. That's a big question.

HB: Yes. The one I complain the most about... You might not have seen this interview with me in *Epidemiology*—I can send you that, too—...

DT: Sure.

HB: ...in which I complained about the fact that we have now become a major arm of the medical strategy, and we've lost our mission with the population strategy, in our training, in our researches. Most of our youngest and most vigorous in this department are working on gene-environment interaction and that has *almost* nothing to do with the epidemics that we experience. It should be done, because it will increase our understanding of disease and health, but it's still a part of personalized medicine and we're the tools. We're given them the tools to do good research in these areas. But our mission for population *health* and mass *disease* is second rate now in terms of funding and training.

The whole strategy of NIH is “personalized medicine.” The motto, the credo, is “from the bench to the bedside” and every time they would say that or the directors would say that, I’d get up to say “from the bench to bedside to the population outside.” It’s an alliteration and it makes sense. They never adopted my idea. [laughter] As they told me: “You would name this the National Institute for Prevention if you could.” I said, “You’re right.”

At our scientific sessions in the spring in San Francisco on cardiovascular epidemiology, 390 out of 400 presentations were personalized medicine in one way or another, the individual, the novel risk factor in the individual, almost nothing to do with the sociocultural, socioeconomic causes of mass diseases. That’s the big change.

The other big change that I complain about is tenure. I talked about how I developed all this faculty in the 1970s, not from tenured positions. Tenure has its advantages and it has its distinct disadvantages. When you didn’t have tenure, you didn’t have security, but you had the security of your own enterprise. You could have an apprenticeship “x” years. I became an epidemiologist by osmosis, by working in this field for ten, twelve years. I didn’t have to go up or out within six years. That’s destructive, because I wouldn’t have joined a team and worked with a team and supervised the Seven Countries Study for twelve years if my survival had been determined on my research output in six years. I couldn’t have done it. So that has been part of the orientation toward the individual, toward rapid research, toward the not long-term views, away from social issues. Just tenure itself and our requirements for tenure, I can say have not been good for public health. I’m not sure whether it’s been good in medical education. It’s not been good in public health. I’m all for it in history and sociology, because they’re not a large enough enterprise for you to be enterprising.

[chuckles]

Those are two major changes and, then, of course, the waves in the availability of funding change one’s life greatly.

DT: That’s interesting. That’s a really important about the tenure process. In the same way that the nature of epidemiological research doesn’t fit the model of federal funding agencies, it doesn’t meet that tenure model either. As you say, you’ve got long term...

HB: It’s sharing with twelve other people!

DT: It’s collaborative. It’s long term.

HB: Yes. It’s tough.

DT: That shift towards this emphasis on personalized medicine, what do you see as being the cause of that, the reasons for that shift?

HB: That's another issue I would like to work on, and I'm not going to be able to get it into it in my book. The choice of careers goes back, again, to the exposure and training. It all goes back to philosophy. It all goes back to politics. [chuckles] From the time that [President Ronald] Reagan came in, we head, then, back to the rugged individual again and the focus on happiness and enterprise and profit of the individual and completely leaving aside the common good. So it is political, it's philosophical. We hope that the pendulum is going back the other way. The pendulum has stopped for now.

DT: Yes.

[chuckles]

HB: The pendulum stopped in mid position. Part of it is that business of what is an American? What is science? You don't see many surgeons who are liberal Democrats. You don't see any Republicans as deans of a school of public health. It's the way you look at life. That's the main reason.

DT: Do you think the philosophical and political aspect to it, do you think the Cold War had a particular influence on the development of the field?

HB: I hadn't thought about that. What is the connection that you see? The focus on militarism?

DT: Well, I was, actually, thinking more kind of aversion to a community perspective...

HB: Communist.

DT: Exactly. [chuckles] Of course, in the heyday of the Cold War, this is when public health and epidemiology were thriving, in the 1960s and 1970s. But there is this emphasis on the individual and a resistance to anything socialist. I don't know if maybe, particularly within public health that kind of played out.

HB: I don't know the Cold War situation... I haven't thought of that. But, certainly, in terms of the philosophy of the individual versus the common good.

DT: Do you have anything else that you would like to share about your experiences?

HB: I made a little list and I haven't looked at it so far.

You were going into the formation of the vice president, reorganization. I don't know anything about it.

Oh! Mead Cavert. Please, talk to him.

DT: I have, yes.

HB: Oh, good. I'm so glad. He would know everything about all this stuff, the first deans. He was appointed under Harold Diehl. You've got to find out what Harold Diehl did for our school besides the bio-med library. I'm glad you talked to him.

DT: Yes, he was wonderful.

HB: [Doctor Blackburn is reviewing his list] Diehl, you've got to go into. Gaylord, you're going into. We talked about the distance of our lab from the school and from Epidemiology. Space, I don't guess I'll talk about it here. We always had problems with space and developed the center at Mount Sinai. One of our studies was at Hastings Hospital [in Hastings, Minnesota]. We always had space problems.

I need and you need to know something about the School of Public Health's summer course. I'm going to try to get some of that from Finnegan's office. We had a real reputation, and that's another thing we lost when Len Schuman retired. He took the summer course to Michigan. It was really a bee in our bonnet here.

DT: There seemed to be a whole assortment of summer courses.

HB: Yes, but this was a big one.

DT: The Epidemiology one.

HB: And the Public Health one.

DT: It pulled in a lot of international people.

HB: Oh, yes, and we lost it in Len's lack of support for what happened after he retired.

[pause as Doctor Blackburn continues to review his list]

HB: Oh! the lack of a School of Public Health building and the whole Mayo endowment, you will go into, I'm sure.

DT: Yes.

HB: You probably know more about it than I do...how we got the Mayo Building, how I got the Mayo professorship. We bought it. We gave them graduate school status and they gave us money.

DT: Yes.

HB: But I don't know any details.

You have asked peripherally about how the School of Public Health stood in terms of the mismatch between our productivity, our productivity in credit hours, our productivity in

grants and in grant money per faculty. It's so far above most other schools on the campus and, yet, our low priority and stature in building space and so forth. This is an issue that needs to be historically analyzed.

DT: Yes.

[laughter]

HB: Another issue that I've thought about a little bit is the role of the land grant function and the role of the School of Public Health. I think Finnegan is doing more than anybody else in the past to make us a global institution. I'd always gone along with the idea that let's do the best with what we have and let Johns Hopkins be the international school of public health. Fortunately, he hasn't adopted that provincial idea. We're moving ahead with very limited collaboration with India. Then, our Seven Countries Study did a lot with the whole issue of how much land grant institutions should be international. [It's a question of interest, I think.

It's a modern phenomenon how parts of our school went to the Carlson School of Business and went away for a few years and, then, has come back. But that's modern history.

That's all I have on my list. You touched on everything else.

DT: Those are great areas for me to think about and to ask others who were involved.

HB: Yes. I'm so glad you've seen some of these people. Edith would be a good person, because she wouldn't have any axes to grind. She'd be very level, and, yet, she had to fight for us. I think they're down on the river somewhere. She would come into town.

DT: I can travel to her, as well.

HB: Good.

DT: Who else would you suggest that I speak with?

HB: Stauffer is number one and he will be delighted to see you. Ellis Benson?

DT: I met with him.

HB: Have you?

DT: Yes.

HB: Wonderful.

DT: Another wonderful character.

HB: Then, you've been well advised. Yes. You've missed Howard Burchell. He's gone.

DT: Yes, unfortunately.

HB: If you really want to know the soul and heart of Burchell, look into those forty years of letters that I've got of his.

I haven't hesitated to give you my ideas in the past.

DT: That would be great. Thank you.

Thank you so much.

HB: Yes. And it's great to meet you.

DT: This has been fantastic.

HB: I'd like to learn more about what you're doing. I'll read your book.

DT: Great. Thank you.

Transcribed by Beverly Hermes

Hermes Transcribing & Research Service
12617 Fairgreen Avenue, St. Paul, Minnesota, 55124
952-953-0730 bhermes1@aol.com