Interview with Dr. Robert L. Bowman
January 3, 1992
Dr. Bowman's Laboratory, National Institutes of Health
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***NOTE: TAPE 4 SIDE A WAS ACCIDENTALLY TAPED OVER. THIS TRANSCRIPT BEGINS WITH TAPE 4, SIDE B, FOLLOWED BY TAPE 4, SIDE A (WHICH SHOULD HAVE BEEN TAPE 5, SIDE A: Oral History may have missing information).*

BOWMAN: About the time when the Clinical Center was being finished, we had organized an exhibit. We faced the problem of ordering stuff for it and funding it with money separate from that used for the government approved uranium. We were working on a method more accurate than the uranium salt method for measuring blood sodium. With this technique you had to precipitate out sodium uranate, oven dry it to a constant weight, and weigh it. As anybody who's done analytical chemistry would know this is a tedious procedure. The quantities yielded were quite small and required a very fussy balance arrangement. And when it became obvious at a later point is that the true value of sodium in the blood was higher, than previously accepted, we were able to show that the uranal acetate method always ended up with a slightly lower value because there was a little bit of loss. We had some difficulty in convincing people that the true sodium level was around 120 rather than the lower value recorded in the literature. So, we were fighting City Hall as we established this more promising technique--flame photometry. Flame photometry, at that time, was a method that was well-established as the method of choice by the Navy in determining the sodium content of sea water and of their distilled water which they'd put it into their

boilers. The cement manufacturing people also wanted to have a salt content control method. These people who used water wanted to know how much sodium was in relatively pure water with not a great number of organic materials present. Procedurally these materials were atomized into a flame in a very simple way and measured with a photometric system and a prism that would isolate the sodium and potassium line; this worked reasonably well. We got a Perkin and Elmer flame photometer, which was one of the ones made at that particular time. However, when used with urine or blood, the rate of atomization was changed so radically by the surface tension-lowering agents present in serum and urine that it was impossible to get reproducible readings in blood and urine without separating all the organic material. This was not very easy to do. So, the problem was to make a reasonable feed into the flame that would not be influenced by the proteinaceous material. And, accordingly, tried various atomizer systems until we got one that seemed to give relatively reproducible results, but it was obvious that this was still a weak point in the whole system since it had to tolerate all the various kinds of fluid that you wanted to measure. The difficulty was accurately metering this stuff to the flame. We built several kinds of chambers and systems to produce a reasonable atomization system. It became obvious that the amounts of gas pressure and air pressure that were used in the atomizer were critical. Unless we could stabilize them to rather small differences, we were not going to get a satisfactory atomization. We improved

the atomizer so that it more or less behaved in the presence of these different biological fluids. But, if the pressures were to change there was some difficulty. Thus, the pressure had to be carefully maintained to ensure constant flow of an organic material. One of my contributions at this point was to ensure the use of very accurate pressure gauges and along with accurate characterization of the flame and the atomizing system would ensure a reproducible system. Now, ordinary gauges were obviously inadequate to do this particular job. My contribution to the project was to add very, very high accuracy gauges. High accuracy gauges were available in a kind of a dime-a-dozen situation because of the surplus market at that particular time. The particular gauge that was used to maintain the air pressure constant was a manifold pressure gauge which came from aircraft and was used for high precision measurement of pressures. I think that the gauge we used for the gas was a pitot tube air speed indicator from aircraft. This, also, was very accurately made with very expensive components. All of these things were available for six or seven dollars a piece in the war surplus market because they had been made for the Air Force. We looked at these gauges and recognized their value. Using these gauges in our research, we were able to very carefully plot the sensitivity of output of sodium light to variations in gas and air pressure. And, by very carefully plotting these pressures we found that with certain gas mixtures and certain settings of the gas pressure and the air pressure, we found a relatively flat spot in the curvature of the graph

of sodium illumination: with initial increases in pressure the curve went up; with further increase the curve flattened out and then came down. We discovered that if we worked on the flat spot of the curve we got much better freedom from fluctuations due to changes in the atomization rate. And, so, we were able to choose a set of conditions which made the system more stable by working on the so-called flat spot of the curve. Therefore, these very accurate gauges allowed a person using the photometer to find that spot and make a stable system. This reminds me of an incident where somebody who had exposed one of the gauges to a sudden over-pressure and broken the gauge, called me and asked, "Could it be possible that you're using a gauge that costs $700 in that instrument"? And, I said, "No. No possibility. But, if you ordered it from the company that made it as an air speed indicator, than you might have to pay that kind of a price for it." And it turned out that she had decided that she would call the company that had its name on the gauge she found out the way that I found out that the gauges that we were buying for $7.00 were $700.00 if you bought them new. Although we were able to meet some of our needs from war surplus stuff, we ran into another problem when we started to make additional flame photometers: we had some difficulty getting the lenses used to focus the light down onto the photo tube. We wanted to get enough lenses to make several flame photometers and it turned out that the lens makers couldn't supply us with the lenses that we wanted. Fortunately somebody recognized the fact that railroad signal light lenses, could

be used and they were available. But, they were 5 inches in diameter instead of what we wanted--something about 2" in diameter. So the flame photometer had to grow up a little bit to accommodate railroad lenses. The signal light lens is made in the form of a Fresnel lens which is a prismatic arrangement that is familiar when you look at the shape in the circles on the railroad lenses. The Fresnel configuration is a method of making lenses in which their thickness can be reduced. We also found that we were able to use a certain kind of photo cells that had been recommended by some people working at Columbia Presbyterian Hospital who wanted to try to make flame photometers. We used some of the information that they had and when I made the one that worked, everybody was perfectly willing to quit and use mine. And so, we made two or three of them, one for Berliner and another one, for Saul Farber. Saul Farber had worked with Dr. Shannon in his earlier years and is now dean at NYU Medical School. He was the man that had the flame photometer in the 28th Street and First Avenue building when we moved over there. That flame photometer was such a success that everybody wanted one. And I really didn't want to make a dozen flame photometers. We tried to interest a number of manufacturers in it, and most of the manufacturers asked to see what the demand was, how many sodiums they did in each hospital around the country. Most of them said that they didn't do sodiums. And the manufacturers said, "There's no demand for them." But the reason they weren't using them was because there wasn't anything adequate

available. This, in spite of the fact that information was available that made it perfectly clear that upset electrolyte balance in post-surgical situations of one sort or another, was responsible for a substantial number of deaths. If you made an educated guess as to sodium and potassium levels, but guessed the wrong way treatment didn't do them any good. So, there was no interest in making the instruments. Beckman and several other companies didn't want to make them. So, we actually asked if anybody wanted to. Dr. Pitts was at Syracuse at that particular time and he said that maybe the Cornell shop at Ithaca would make it. Then they came down and looked at what I had and copied it exactly. It came out the same size because they, too, had to buy the railroad lenses, the only type they could get in quantities; they didn't want to change anything. So, they made it just like the other one. The electronics was extremely simple; it consisted of nothing more than a galvanometer and a balancing system that used a potentiometer to find the balance point of a Wheatstone bridge that would measure the output of the photocell. And, the most important thing about this particular one was the introduction of the internal standard. Now, use of an internal standard was a problem more important than working on the flat spot of the curve in systems that were plagued with problems of the atomization rate. We just added a certain amount of lithium; the lithium put out a red emission and the sodium put out an orange emission. We had to make special filters to separate these two wave lengths, and this allowed the ratio of lithium to sodium to

be measured. Since we knew how much lithium we added, when the ratio of lithium to sodium was balanced-even though there were some changes in the delivery of the total amount of fluid-we knew the value was correct. So, the internal standard was the most important thing about that particular instrument. But, relying on the internal standard alone was inadequate. Unless you would find this relatively flat spot on the curve, as I call it, the internal standard could not be used. At any rate, that was successful and I think the Cornell shop made about ten or twelve of them. They were all absolutely identical. They copied everything, including some of the parts that were relatively hard to machine. I had used them only because I happened to pick up a particular kind of alloy in the junk supply that was reasonable to work with. There was nothing wrong with it; it was just unnecessarily difficult to machine parts out of that particular alloy. I think that being able to make the flame photometer perform gave me an amount of popularity. Everybody knew I could fix a flame photometer that didn't work. So, I frequently got invited to all kinds of places around the country and incidently, was asked while I was there if I might look at their flame photometer. They would pay my way to the place and because they were all friends, I could hardly refuse them. So, at least I got to see quite a few places that were doing the flame photometry. I had many hair-raising experiences since some people said that their flame photometer had been perfectly all right until it just suddenly stopped working; nothing they had done to it. At which time I would decide to

light it up and see what happened. I sometimes turned on the gas and lit the chimney to get the thing going at which time it would blow up. It often turned out that somebody had disconnected the gas and hadn't hooked it back up, which fixed it so that it was booby-trapped. So, I got to be more careful about the history of what had happened before I got there. Well, I think that's the story of the flame photometer. I got very good results and everybody started using them. It also became perfectly obvious that a hospital buys a new instrument only when they're in a situation where if they don't have it they'll be sued. And, so, when it got to the point where it seemed like inadequate sodium and potassium monitoring would get them sued they began to buy flame photometers. The man who made my pump, finally, was Dr. Joe Greenspan, who had a company called *Process Instruments* and usually made mass spectrometers. When he made my pump, it turned out that I had had no patents on it. I thought that it was not worth patenting since it would be a very restricted use, such as measuring minute quantities of stuff injected into people. But he said he would just give me a percentage of the sales price. While there was no requirement that he had to pay anything he agreed to pay a little royalty. That was just in case I happened to have something else that I wanted to have manufactured, I would come to them first. So I got a few checks for small amounts every year from the pump. Then suddenly the pumps got immensely popular and I started to get several thousand dollars every year. It turned out that the pumps were beginning to be useful in

industry, for purposes such as adding minute quantities of vitamins to each loaf of bread. They did this instead of adding vitamins to the batch because with batch injection it was hard to be sure that each loaf would have the right quantity of vitamins in it. An Arabian oil company began buying the pump. They used it to add indicator dyes to the pipe lines so that they could identify specific batches of distillates pumped through these pipelines. With each change of material to the other, they added more fluorescent dye. They added this marker in very small quantities to larger volumes of oil; it would seem that they could have added a lot more than they did, but they wanted only a very small amount. At any rate, it sold pumps; at this point I saw that there was some value in patenting and holding. However I thought better of the idea and decided that the best way of doing it was the way I had started before the pump really began to get popular. Everybody eventually started making them. I thought that if I patented this system, it would have inhibited other people from doing it that way, and they would probably do it another way to avoid the patent requirements. So, when the pump began to become popular, and other people began manufacturing similar ones, I thought that the competition was a good thing. The same thing was true of the flame photometer. When, very shortly after Greenspan started making the flame photometer, a lot of companies started making them all over the country. Since there were no patents on them, it was perfectly obvious that very soon with a little more engineering here and there companies would no longer make them

my way. They would make improvements on things as soon as possible. So, I felt strongly that the instrument would more likely be improved and rapidly marketed, rapidly if I didn't have patents.

HARDEN: I think we run out of tape and time on this session but would like to have discussion on the point depression apparatus.

BOWMAN: The freezing point depression apparatus comes after I got to NIH.

HARDEN: Maybe we can now talk about the transition from Goldwater to the NIH.

BOWMAN: Well, it seems at this point, I think I had developed enough things that were working and some other things that I can't recall. This business of making instruments is amazing in the fact that occasionally somebody comes up and tells me that when I was at Goldwater they worked with me and I had made them an instrument to do something or other. They say that if it wasn't for me, it would have taken so much longer. They'd tell me about it and are so appreciative; I haven't the slightest idea of what I did. Except that I had generated an advocate, at least.

HARDEN: Let me ask you one question about that. Obviously, you were the one at Goldwater who had the aptitude to whom people turned. Were you aware of other people around the country at that time, in the late forties, who were also trained in medicine and in making instruments? Were you unique or…

BOWMAN: Yes. There were few. But they were very few and it's hard to recall just who they were at what time. Frequently somebody had hired a technician as a

machinist or whatever and found he was particularly valuable in a particular area. I think there were very few of them that were interested in instrumentation, in general. There were very capable people at MIT and one especially at --oh, was it Baylor--in Texas. I can't remember his name, now. Yeah, he is a physician and he was mostly involved in heart-lung instrumentation.

HARDEN: DeBakey?

BOWMAN: No, this guy ran a biomedical instrumentation program and didn't work directly with DeBakey. But, at any rate, there were a number of people that were specialists in that particular thing. He was working on a heart-lung machine and he taught in the physiology department. Most characteristic about him was the fact that he used horses as the experimental animal and people who would go through the dissection procedure with him in their pumping system used horses. Apparently, horses were a dime a dozen in Texas at that time.

BOWMAN: Anyway, although there were a few people around, I don't think anybody wanted to stay with it. When I came here, I decided that even though I was a physician, I was going to work on instruments and not practice medicine. By that I think I cut off opportunities to go elsewhere but I didn't care about that. I thought it was pretty nice here.

HARDEN: Now, I interrupted you as we were getting you here. You were at Goldwater and, I suppose, when Dr. Shannon came to the Heart Institute as the Scientific Director and started looking around for other people, he selected a number of people like

Berliner. Tell me how you linked up with Dr. Shannon.

BOWMAN: Well, I kind of got involved at the point where, Brodie, Axelrod and Udenfriend stayed at Goldwater and I went to the Department of Medicine at NYU. And, I think the reason for going there was to continue work on this myotonia congenita project, which I told you about, and do some neurophysiology I wanted to get closer to the department of physiology at that point. And I thought Goldwater looked like a blind alley. I wasn't going any place and it was shutting down, really. The opportunity came to go to NYU medical school and I spoke to Ludwig Ikna who was associate professor of medicine, I guess. The professor was Tillich. And, I had expressed an interest in instrumentation and physical science and was doing neurophysiology, mostly. The idea of instrumentation had come up and Dr. Ikna, Ludwig Ikna, thought it would be a good idea to see if he couldn't get some physical science into things. He thought I would be good at beginning to get the physical scientist into the medical school as a instrumentor, of a sort. At that point, I got involved with instruments for heart-lung catheterization, and, so on. Richardson and Cornand were evolving the idea of cathetering patients to get blood samples to measure physiology. And I put my two cents in there by making some improvements in their system of recording blood pressures with a little mirror galvanometer. It was a particular kind of a gauge in which a mirror soldered to a thin diaphragm was exposed to the blood pressure at the catheter. A beam of light shown on this mirror went across the

whole room and into a recording camera. You had to work in semi-dark, at least, if you wanted good contrast. The problem of positioning these little mirrors and keeping them stable in relation to the camera was something of a difficulty. At which point, I think I instrumented a bed system for the catheter group that utilized optical gun sights. This was based on a device called a battery commander's scope that the army uses to look at the impact area of shells. This enabled them to make a correction before firing for effect, and then make measurements on a precision device which is nothing more than a tripod that holds a telescope and determine where the gun is pointed. These were made by Carl Zeiss for the German military and became available on the surplus market. Thus for a few dollars you could buy a precision mounted device that would allow you to direct this light beam to where you wanted it, you keep it there, remember where it was and to write it down. So you could avoid a lot of awkward adjustments necessary to keep the light beams all shinning in the right place. I made a rather elaborate arrangement in which I built a little bridge across the bed with heavy aluminum bar on which I mounted these battery commander scope bases. This allowed us to focus their mirrors into the cameras. This was the time when we used lead tubing in the measuring part of the instrument--not in the blood container part of the thing. This was used to avoid the bounce that occurred in rubber tubing. This arrangement was a success. I think that Cournand, Richards and a whole bunch of people that had been

involved with that sort of thing liked the idea. It was pretty much just using the mechanics that was available from Zeiss which does very well in this particular sort of thing. So, I made these things when it became very obvious that they were useful not only for holding telescopes but also for holding small things that should be rigidly mounted. Very shortly thereafter, somebody saw that you could make a micromanipulator out of them. And, well, I modified some of them and made micromanipulators. I made a lot of things out of military surplus of one sort or another. Such items were available at very low cost and very fine material was available. And, so, to some extent I learned a good deal about how to make precision instruments from seeing how they were made.

HARDEN: I think we're just about to run out of tape. So, maybe I should just say this will be the end of Tape 4, Side B.

HARDEN: **Dr. Robert Bowman. It is now January 2, 1992. This is tape 4, side A.**

HARDEN: Dr. Bowman, now we've been chatting here before we started about the transition that you and other people made from the Goldwater Memorial Hospital to NIH, and you were about to tell me that everybody up there was hearing rumbles that Shannon might move to the NIH. And what this meant as far as possibly going with him to a government laboratory. You want to comment on that?

BOWMAN: Yeah. As I recall there were several people who were invited early in the game to go with Shannon. Although I don't know who exactly they were, I think that Tom Kennedy was the first one of them who decided to go with him. Most of us

had some reservations about going to a government laboratory. We didn't think that we wanted to get involved with Civil Service red tape and other things rumored to be impediments to getting anything done.

HARDEN: Was there also some concern that, people might tell you what to do rather than let you work on what you wanted to?

BOWMAN: Yes. We felt that there was some question whether or not we would be expected to perform directed research according to the requirements of government politics. We really didn't want anything to do with that. Now, I didn't know a great deal about how government laboratories were reputed to behave since after medical school, I went right away to the army and was overseas for three years, or so. So when I came back I was pretty green as to what had happened in laboratory science during wartime. And I more or less listened to other peoples' statements about what had happened during the war in the Department of Agriculture and the Department of Health-places that were concerned with medical research. Almost everyone at NYU had some reservations about going to a government laboratory. And, the whole idea of the Civil Service mechanism was something that was to be shunned. So, I held back since I felt that I had some future at NYU. My work had been now transferred to the NYU in the department of medicine. I also had an appointment in the department of medicine as instructor in medicine I think. That was on the low part of the totem pole, I think, as a beginning scientist in medical school. But, I had made a fair

number of contacts there, and was interested in some aspects of research, which, because they were of interest to people in the medical school, were going to stay there. Dr. Ikna--Ludwig Ikna--who was associate professor of medicine, was very much in favor of keeping me there. He was concerned with getting the physical sciences into medicine. One of the arrangements proposed was that we would form a physical biology department that would involve neurophysiology and cardiac dynamics and functions. I also had support from the department of physical medicine which was very well financed through the Baruch Foundation. I think I was able to get about a thousand dollars a year. A fabulous sum to work in a laboratory with all the instruments I wanted to buy up to a thousand dollars' worth. It was also indicated that a physicist to work with me. I would be the person who presented the medical problems to the physics scientist. The physicist however, would not be required to work on medical problems. And that would presumably attract a physical scientist to work in the vicinity of the medical school, or in the environment of the medical school. It was hoped that by bringing physics talent into the medical school environment the physicist rubbing elbows with the medical people would develop insight into medical problems and medical scientists could see how physics might contribute to our research program.

HARDEN: I wonder if this was happening in other medical schools as well.

BOWMAN: Well, a number of them tried this sort of thing since I think they had pretty much

the same problem that I had. I also proposed the idea to friends who were well versed in physics and whom I thought very highly of. They were enthusiastic and thought it sounded like a good approach. But the amount of money available to finance their physical science was inadequate. I recall my experience with Ben Bedderson. Dr. Bedderson, a student at Columbia University, was a disciple of I. I. Rabi, the well-known physicist. He was quite a competent fellow, working with high voltage discharges in low pressure and the dynamics of the current discharge of arcs. His requirements to continue that work could not be met in the little laboratory that we had available for him. We were going to give him a one-room laboratory and he would work with me on problems of his own choice. I remember that at the moment I was looking at problems in the association of myosin and actomyosin in muscle contraction. Dr. Bedderson got very interested in that but the more biological the problem became, the less interested he became. Considering what they're doing with the myosin molecule today, would be what he would have liked that kind of work. But he wasn't ready for that. And the molecular mechanics that were involved would have been his cup of tea except that at that time to--we didn't know enough about it to know what the molecular mechanics there were. We did publish a little paper on relaxation. No, in fact, I don't think we got it--even published it. It was on a method of measuring relaxation times of electrically-oriented myosin molecules. We used ultrasound and such things to see if we could elucidate some of the

properties of the myosin molecule. I've forgotten much of the detail. Dr. Bedderson was interested and came to NYU and worked with me in the at 28th Street laboratory. We felt that it was good place to meet and learn to mix the other people. We used to go to the faculty dining room and mix it up, during the lunch time. But he got so that he really didn't look forward to lunch. He said: "All these people talk about things that take my appetite away." He didn't relish discussions about the appearance of somebody's blood and guts that were obtained at autopsy. Everything that the physicist was interested in seemed to be tainted with blood and guts. Even in the catheter lab where physically interesting things were being measured, there was blood running around in places that disturbed him. And, I couldn't drag him to the catheterization laboratory any more once he saw it the first time. At any rate, the idea of being able to go out and capture a capable physicist and bring him down to the medical environment was not very adequately pursued. We didn't have physical science instrumentation or the real money that would attract somebody that really wanted to do physics research. Now, some of the equipment we built, such as that for studies of the relaxation mechanisms and the polarized light, depended pretty much on my knowledge of physics and of physical instrumentation, although the physicist knew theory, the instrumentation used in fields in which he had never worked was unfamiliar to him. So, the work really never did get off the ground. We did get another man for a very short time. He was a physicist with Sylvania

Electric Corporation and he gave it a short try. He visited informally a few times, was fascinated by the problems, but really never got his teeth into anything. Another man who was highly qualified in electronics spent some time with us. He worked in the world communications division of a large company. His involvement in pulse and coding systems for communications, led him to a lot of interest in neurophysiology. He was just entranced by our work with nerve potentials, the excitability cycle, and the ability to record signals from these fibers. Although he became very interested in our work, he was promoted to be head of the world international communications division of his company and he went on to work with pulse code modulation systems for sending transatlantic messages through cables by multiple codes and things of that sort. This was a field that was just getting active at that time and he was soon well out of our reach. But while he was able, he would come evenings and work with me on trying to figure out some methods through which he could adapt his knowledge of pulse code communications to the neurophysiology that I was studying. At any rate, that idea represented an opportunity was for me to build a program in an area where we had no money. We had some, but the Baruch Foundation was more interested in providing the where-with-all for this for someone who used to write for *The New York Times* and well known in New York as the father of physiotherapy. What was his name? Probably Howard Rusk. I don't know anything about a Baruch Foundation. Could this be Baruch (Bernard Baruch)

who may have created such a foundation. At any rate, there was a coupling of my work. I got into ultrasonics and explored its possibilities both as therapy and as a diagnostic method. I had a certain amount of rapport with the Rusk Institute which, I think; is still the big physiotherapy part of NYU. I mention the opportunity in this field since the prospects of developing it influenced my decision to stay at NYU. Thus Dr. Ikna's positive attitude about introducing more instrumentation and physics, into the practice of medicine.I was also involved in the studies of the pulsation of the arteries and the diagnostic aspects of measuring pulses and pulse pressures, phenomena which have since been well utilized in the study of cardiac dynamics. I also remember Domingo Gomez, a mathematician hired by Homer Smith, who was working on the dynamics of the circulation with a pump circulation system that he was analyzing. He was a highly regarded expert in mathematics who was close to Einstein. He and Einstein compared notes on what was going on in each of their fields; he mostly left me out of the picture. I didn't meet Einstein with him but apparently he had a close rapport with him. So, that was what was in the offing if I were to stay at NYU. And I had good backing. Dr. Tillich was quite friendly and liked my work. And, as I said before, Dr. Christiansen, who had a shop of his own in the monkey house was quite supportive. When I came from Goldwater to NYU, I brought my shop with me and installed it in the corner of my laboratory in the 28th Street building. So, I had the shop and I had Christiansen's interest. He was also

interested in doing something about organizing a shop for the whole medical school. He wanted to develop a special services facility along the lines of NIH's Division of Research Resources. It would have a shop library and such things under its control. He wanted to head up that kind of unit and got a fair sized grant from one of the commercial animal laboratories I forget its major business but at any rate, it provided money for the procurement and maintenances of animals to be used in research; and the procurement and handling of chemicals. This was an economical way of obtaining needed resources. In addition, the grant enabled us to make a machine shop facility available to facilitate research at the medical school. So, all of this was relatively attractive and Dr. Shannon knew about what was going on. I discussed things like this with him a lot. Homer Smith also knew all about it and we talked about it. I think everybody thought my prospects at NYU would be much better than at a government laboratory.

HARDEN: Were you already thinking about such a move or corresponding with Dr. Shannon?

BOWMAN: No, I don't think we had much of any correspondence, but we talked occasionally. Anyway, with this opportunity at NYU that I just indicated as an alternate, I was not very anxious to get into the Heart Institute. Although Dr. Shannon had not really made a formal offer, I think he was in a position to do so if I asked him. But, I think everybody was a little cagy about asking for fear they would get an

offer. So I held out along with quite a few people at NYU. I think Brodie and Udenfriend were anxious to go with him. They and Kennedy, were the first few that were enthusiastic about it.

HARDEN: Were these people enthusiastic because of the opportunities available at NIH-which had been here for some time-or mainly because of Shannon's vision?

BOWMAN: They were enthusiastic entirely about Shannon. I think that was the only thing that was going to be different and everybody interested in going was concerned that Shannon would stay with it. If he would go, was he going to stay? There were several people who took up the first offer and went with him immediately. Dr. Berliner was slow in acquiescing and I was mostly taking advice from Dr. Berliner who I felt was a strong person. He knew the fine points and I regarded his advice very highly.

HARDEN: Let me ask one more question about Dr. Shannon. What was it about him that inspired people? His management of research in terms of his confidence in the people that would be working with him? Can you put it into words how he inspired people while others might not?

BOWMAN: Well, I don't really know the answer to that except that he inspired me. I'm not sure exactly why. I think I heard him talk about what they were doing at Goldwater during the time I was away. Also I think I caught it from other people who had regarded him very highly. I regarded Homer Smith very highly and I think Homer Smith regarded Shannon very highly. Dr. Berliner, with Dr. Smith

and Dr. Shannon, were the kingpins of kidney physiology, they seemed to be doing the most interesting work around. And, the fact that they were interested in what I was doing and listened to me inspired me. If they'd listen, the work was important.

HARDEN: Now, you were saying that Dr. Berliner was advising you to come down here.

BOWMAN: Yes. So, I dragged my heels for quite a while and then Dr. Berliner who had been holding off, decided that he was going to go. When he said that, I said I wanted to go too. I said, "Well, then, that's for me, too." I had the greatest confidence in Dr. Berliner and liked to work with him. I felt that he was a good teacher and an excellent scientist. Then when we had to say what we wanted to do, I said that I wanted to make up a laboratory based a little more on the physical sciences than most laboratories. I really wanted to set up the kind laboratory I had wanted at NYU when we didn't have any money. And it sounded like we were going to have some money if we went with the Heart Institute. I outlined a little bit of what I was thinking to Dr. Shannon. He didn't seem to care just how we were going to do it but, he was, I think, convinced that there was a place for the physical scientist. Of course I really am not a physicist; I'm an instrumentor, I suppose. I guess we talked about the ability to do experiments that required hardware. I had already demonstrated in work with Berliner, Shannon, Homer Smith and other people that I could do things that would facilitate the operation of a biological laboratory. Now, I don't know whether the Laboratory of Technical

Development was made up and named after the concept that I had discussed with to Dr. Shannon, or not. But Dr. Boone was brought here by the previous temporary head, of the intramural division, who was going to get the lab going for Dr. Shannon, I think. And he brought Bert Boone, an M.D. who had been effectively working with the radiologist at the University of Pennsylvania hospital.

The radiology department there was working with hemography which was a way of dividing the heart x-ray up into a series of slits which were moved to produce a contour map of the beat of the heart. Dr. Boone worked on this procedure. With the collaboration of an electronics engineer who worked for Dr. Boone at Pennsylvania, they had also developed a kymograph. The engineer came with Dr. Boone to work on this instrumentation at the Heart Institute. Now, Dr. Boone was mainly an instrumental scientist; he was not much of a physicist but he knew a lot about what you could do with electronics. I don't think he did much himself, but the engineer, Frank Noble, was apt and very capable. He was mostly interested in radio frequency or ham-type electronics and he had communications instruments of one sort of another, a field that was his forte. He built equipment with Dr. Boone and had an electronics section with several people in it. They were capable of building more electronics than we could possibly use. Although it was good electronics, not very much of the work impinged strongly on biological problems. So, when I came I was assigned to

Dr. Boone's laboratory which was already called the Laboratory of Technical Development. Now, I don't know whether Shannon had me in mind when he let the Technical Development Laboratory develop. Dr. Boone was several years my senior and was talking about retirement relatively early in my contact with him. I got the impression that I should get a number of people together and see if we couldn't make a multiphasic approach to introducing instrumentation into the biological institute.

HARDEN: Now, let me also ask you, at this point, to characterize the NIH, the Heart Institute, at this time. Now, the Clinical Center was not here. So, there wasn't any clinical research when you first arrived. But, it was under construction. Can you talk about where your laboratory was and what the atmosphere was at the time?

BOWMAN: Well, when we came here we had facilities in Building 3. I guess we had the third floor or maybe the second and third floor. Parasitology was above us; the attic was not developed yet; and there was a preparation room in the basement where they prepared huge quantities of things like horse livers. In this tissue prep room they prepared, ground, and extracted tissue specimens. It was like a giant butcher shop. And, so, I think we had two floors in Building 3. My laboratory was the corner laboratory on the third floor which is now the library of the Laboratory of Cell Biology. I had just a few people, Dr. Boone and Mr. Noble. I guess that's about all. And it wasn't my laboratory, but I was

instrumental in seeing if I couldn't get Dr. Boone to hire the people that I was interested in and I was encouraged to do that by Dr. Shannon. And I got a couple of people together early in the game, one of whom was Murray Eden and, I guess Berger didn't come until I was chief of the laboratory.

HARDEN: '62, I think.

BOWMAN: '62? Was it that late? Well I got several people who were trying to solve problems of their own.

HARDEN: That's what I wanted you to talk about, too. Where were the other labs, like Dr. Udenfriend's and Dr. Berliner's? Were they in physical proximity to you?

BOWMAN: They were all on the same floor. Dr. Berliner, Udenfriend, and Brodie were around the corner or down the hall. Just as close as that.

HARDEN: So, you talked…

BOWMAN: And we all talked with all of them together all the time. We had continual contact with that group. I don't quite know when the various groups appeared but as they gradually drifted in, more and more of the laboratories were assigned to the Heart Institute and other people moved out as we encroached on their part of the program. And, of course, there was no Clinical Center, so a part of our activity was concerned with making plans for the Clinical Center. The problem of ordering enough equipment to equip the laboratories of the Clinical Center was a fabulous piece of work. Dr. Shannon more or less indicated that we ought to use building money-we had lots of it at the moment-and stuff this building with

all the instruments we might ever want. After all it came out of the pocket that didn't come out of research money. So, as I recall we counted the laboratories and ordered a microscope for each one, or something of that sort, and all kinds of machines. We bought instruments of one sort or another, anticipating the kind of laboratory that we would be able to work with. We bought so many centrifuges, and so many refrigerators, and so on. At this time the sales people were very interested in coming by to see us. A lot of them were anxious to show their wares and organized a symposium that accompanied an exhibition--the manufacturers' exhibition. The manufacturers' exhibition was tied to a non-NIH organization because they really wanted to keep government money separate from the money that was financing this show.

HARDEN: I think we need to stop here because I think the tape is about to run out. This is the end of Tape 4, Side A. ***Note: The following remained on the tape and is assumed to be the end of the original tape 4, side A.***

BOWMAN: …dispense with the shop with this. And everybody recognized its value.

HARDEN: You talked about the profusion pump you built. Could you run through the other instruments that you worked on here.

BOWMAN: The other thing that we did was a flame photometer. Now, Dr. Berliner was measuring sodium and potassium excretion and handling in the kidney and it was really not a very good way of measuring sodium and potassium in the blood at that particular time.

HARDEN: I'm going to interrupt you one minute because I think we're going to run out of tape. This will be the end of Tape 4, Side A.

This is Tape 5, Side A Interview with Dr. Robert L. Bowman January 1992

HARDEN: Dr. Bowman is going to pick up where he left off from our mistake taping over on Tape 4. Can you finish? Can you recall?

BOWMAN: Well, I started to talk about the manufacturers' exhibits and symposium, which we organized around the Purchase and Supply Branch of NIH. It was run by an association of manufacturers a representative from NIH. I was the NIH representative. I organized a scientific symposium to accompany to the exhibit. It was primarily an instrumentation symposium at which scientists from the companies that manufactured new instruments could discuss the applications of the instruments. The scientific, rather than sales, aspect of the instruments provided the meat for the instrumentation symposium and that went on for several days, running parallel to the exhibit. The symposium was a drawing card that brought people from all over the Washington area and the east coast to NIH. Lou Heiss, who was the sales representative for American Instrument Company, represented the sales aspect of the exhibit. Mr. Heiss managed to keep this symposium working despite the challenges of a budget based on government and nongovernment funds. We used the fees for the exhibits to bring scientists from abroad or from distant parts of the U.S. to speak at the symposium. I chaired one or two of the sessions and chaired the whole session at least once. Thus I was

involved in a lot of the instrumentation brought here by the symposium. We were the first people to present some of the topics on some of the newest instrumentation. Once I got some fellow over from London who was doing gas chromatography. That year nobody showed up because nobody had ever heard of gas chromatography. The next year we repeated the presentation, and the place was jammed. Gas chromatography was one of the things we presented before it became overwhelmingly important. This was the kind of thing that was done with this association of manufacturers and the exhibit. The exhibit was the ideal place for manufacturers to exhibit their wares because it attracted people from the various Institutes who were going to order instruments. Most importantly the manufacturers' representatives were able to talk to the technicians, not to the scientists. The technicians, who were going to use the instruments, wanted to find out what they could do, and the sales people were enthusiastic to be able to show the technicians what a new instrument would enable them to do. The instrument exhibits were very successful and very popular with both the scientific groups and the sales people. I mention this especially because working on this event was how I met Lou Heiss, the chief of sales in this particular area for American Instrument Company (AMINCO). American Instrument Company had sales offices all over the world. It was not a very large company, but it sold esoteric instruments--small, inexpensive, but good instruments. AMINCO was having a certain amount of financial difficulty, I think, about that time. Lou Heiss

encouraged me to tell American Instrument Company what we were doing. He wanted American Instrument Company to manufacture our fluorometer. He told Mr. Freeman, the head of American Instrument Company, about it. Mr. Freeman was very interested in a new product at that particular time, and Lou Heiss proposed to him that spectrofluorometry was a technique that he thought was going to be very worthwhile. Mr. Freeman was convinced that I knew what I was talking about, and when they began to develop it and I saw a problem, Mr. Freeman listened to me. He responded to my concern that the engineer who was working on it was not listening to me. He assigned a new engineer to the project. In addition after I had made a prototype, I wanted to incorporate a new idea in the first commercial instrument, but I had no experience with it. Nonetheless, they built the new instrument--cast in bronze, very solidly made--utilizing front surface reflection. Unfortunately, I had made a mistake. Front surface reflection made the instrument very sensitive, but it had great difficulties with stray light. AMINCO was willing to throw out the original instrument and start over again. It gave me great confidence that they really wanted to do it right. The new engineer did do what I wanted to do. He was very receptive; he brought over new plans and let me red-line them, etc. The company was very cooperative. The important thing I want to emphasize at this point is the role of Lou Heiss. He had the insight to tell American Instrument Company, "This is a good instrument that is very much in demand and will go places." He got a lot of

credit because people tell me that American Instrument Company survived its difficulties by making the spectrofluorometer. I don't know for sure that this is true, but AMINCO apparently did very well with the spectrofluorometer. The important thing is the fact that Lou Heiss was able to convince American Instrument Company that it should make the instrument.

HARDEN: This is a continuation of the interview with Dr. Robert Bowman. This is January 7, 1992 and we're in his office.

HARDEN: Dr. Bowman, when we finished the last time, you had been talking about the American Instrument Company's involvement with the spectrophotofluorometer (SPF). I'd like to talk today about when your laboratory was in Building 3. Could you to tell me about the intellectual process of developing the instrument? Your interactions with Dr. Brodie and how you assessed the existing filter fluorometer and how you got the idea for the SPF?

BOWMAN: The initial discussions about making a spectrofluorometer with Dr. Brodie actually took place while we were at Goldwater Memorial Hospital. Dr. Brodie's group had developed a number of methods that used fluorescence as a measure of the amount of compound in solution. The measurements were made with a Coleman filter fluorometer, a very simple instrument. It consisted of a mercury arc lamp, a medium pressure arc characterized by the bright lines of the mercury spectrum. They're much brighter than the background, so it's very easy to separate the lines and to get specific wave lengths of fluorescence out if you

merely add a filter to enhance the separation of those lines from the rest of the spectrum and to exclude the visible light. The mercury lines are well known and conspicuous in the spectrum. The very prominent lines appear at 350 to 360 micrometers. Furthermore, the cause of most of the interference is a very bright green line in the visible spectrum that is easily filtered out. The fluorometer was easily made and used only glass filters, which were usually made for production of what they call "black light". The instrument had only about three lines available in the ultraviolet. Some of the instruments had broad spectrum filters, which allowed several lines in and did not provide discrete these to separated lines. There were lines in the far ultraviolet that could also be obtained by this method, but the filter material was more expensive, and it was more difficult to get large amounts of this bright light out. At any rate, the general concept of the instrument involved a lamp that emitted specific wave lengths that could be separated from the rest of the spectrum easily. The instrument consisted of nothing but a mercury lamp and a couple of filters, whose purpose was merely to separate the visible light, which would just be scattered, from the light which produced the fluorescence. To measure the fluorescence, it used a phototube system, a relatively simple type of photo-electric cell. There was no enhancement of the intensity of the fluorescence by amplification. It was a simple instrument. It was a very serviceable instrument, and it was used largely for vitamin analysis in foodstuff. The thiamines, for example, were fluorescent,

and they were mostly determined with the fluorometric method.

HARDEN: Did it provide quantitative determination or just qualitative?

BOWMAN: Quantitative determination. Brodie and his group had used the fluorometer for determining compounds to which they had coupled fluorescent molecules. They used them as indicators of how much of the coupled substance was present. When I started work with some of the quinine compounds, which are all fluorescent, it occurred to me that the existing fluorometers might be missing some of the fluorescence that could be used in research if only we had a wider selection of wave lengths to excite the fluorescence in the ultraviolet. *(need to verify Pringtimes') book about fluorescence indicated that a great number of organic compounds fluoresced in the ultraviolet. Not only was their fluorescence stimulated by the ultraviolet but it also appeared in the ultraviolet and was, therefore, invisible to the eye. I mentioned it to Dr. Brodie that there were a lot of things that were fluorescent that we weren't aware of and I made something of a mistake in using the idea that solid state fluorescence was also interpretable in the sense of the widespread fluorescence that was present in nature in the sense that there were a lot of minerals that require them to be in the solid state and can't be done in solution. When I mentioned that, Dr. Brodie immediately said that this would be a great thing to do. I said, “We could make up an instrument just to survey how many substances were fluorescent in the ultraviolet.” I did a literature search, and I came across some investigations that were done in the

Esso oil company laboratories. I don't know whether this report was published externally; I think it was just an internal report known as the *Esso Report*. It was very negative about the use of fluorescence for analysis of fluorescent material. The reason for this was that the crude oil contained hundreds of compounds, poly-aromatic compounds made up of multiple benzene rings. A benzene ring is a resonating structure, which is usually fluorescent and can be excited with various wave lengths. The spectrum of each of these polycyclic rings had characteristic fluorescence and multiple peaks and so that each material in the solution would contribute multiple peaks and in a mixture of compounds obtained from crude oil, you would get so many fluorescent materials that it would overwhelm you. And they had concluded that it wasn't particularly useful to Esso to do much with the fluorescence for analysis of the fractions that were present in the oil which had commercial value that they were interested in. And the *Esso Report* took a dim view of spectrofluorimetry as being a great thing, but it did indicate that there were a lot of things that had fluorescence in the part of the spectrum that we couldn't reach because of the special properties of the mercury lamp. But I showed that to Dr. Brodie and told him that about the situation but he said that we didn't use a mixture of compounds and we just wanted to have a single one and if they were all these--all the fluorescent compounds, it seemed like it would be just as useful. I don't know if he said that, or if I convinced him of it, or one thing and another. It was a report that said that

it wasn't really very useful. But, in such a way that it said that there were too many materials and mixtures that made it confusing. But, we had a different requirement and in spite of this, when we considered doing the fluorescence, I think the first approach was to--So, it's a little bit foggy in my mind just what we did about this right away and I believe that one of the things we did right away was talk to the Farrand Optical Company about it and Mr. Farrand was very sympathetic and interested in medical problems and he had contributed to medical instrumentation by making an automatic blood pressure reading machine which was stimulated by the fact that his wife had severe hypertension and needed some way of recording--keeping track of her blood pressure. He made a useful machine and was benevolently disposed toward medical research, in general, and the Farrand Optical Company made one of the fluorometers which was available and I think the first thing that happened on it is that I had suggested that we get a monochrometer which would allow us to select wave lengths of light without regard to the mercury lines and would use--these are an incandescent source or a arc and disperse that and see what kind of fluorescence we could get from it. I do not believe that I built anything prior to that time, except for some relatively simple experiments that I did either at home or in the laboratory to just confirm that there was a reasonable amount of ultraviolet in the therapeutic carbon arcs. Now, the therapeutic carbon arcs were carbon rods that were dipped with metallic materials and they had a core in the middle of them so that there was iron and

various other metals present that would burn in the arc and would emit a broad spectrum of overlapping lines of the various elements and, I believe, I made a very simple device which consisted of an open carbon arc, a slit cut in cardboard or metal, or something like that and a dispersing element of a prism which I had had for many years in my laboratory that was actually a quartz prism that allowed me to see how intense you could get fluorescence. Now, that was never brought to any state that we could use it in the laboratory but was just a mock-up version on ring stands, and things of that sort, which, only indicated that it was a lot of material to work with. Now, in the literature, there was a lot of reports of various kinds of compounds as being fluorescent but these were all in books of organic chemistry or something of this sort in which the fluorescence was used more or less only to confirm the possibility--the resonance phenomena in a particular compound as being a proof of that kind of a structure. And the physical chemist and the physicist who looked at the subject knew about fluorescence and had written about it so there was no new discoveries involved but just a possibility of utilizing what was available in nature and the idea that if we could get an instrument that would let us have a selection of wave lengths available we could do a better job. And I believe at this time we wanted to--we looked at a few things and Mr. Ferrand was so anxious to do something about it he said that they would build us a fluorometer. And they started to make a fluorometer which was put in one of the little rooms in the Farrand Optical Company which is a

good-sized company that made not only high grade ultraviolet instruments that contained quartz prisms and there was a particular high aperture monochrometer available from Farrand for something like $5,000, and that seemed to be a very expensive instrument at that particular time. And they said that they would put it at our disposal and they had indicated that they were running experiments on the system and that they were interested in making progress. I visited them with Dr. Brodie on one occasion and they enthusiastically described their efforts of getting together a monochrometer and a system to find out what they could do and when we went there they didn't quite know what room it was in. And, so, their very special high activity was not very apparent. And we finally found a little room which had one of these monochrometers set up so that they could isolate the ultraviolet and you could hold the test tube up in front of it and see fluorescence. And there was nothing but the monochrometer itself present and very little else of any use and I think he had some quartz cuvettes so that you could utilize the quartz ultraviolet and see some fluorescence. There was some intimation that maybe they would let us--let me have one of their monochrometers and see what I could do with it. It's incidentally true that the man at Farrand had chosen to put on the project was Mr. Farrand's son. Now, Mr. Farrand was very proud of the fact that he was a self-made man and was now the head of a large optical scientific business without any formal education. I think he had nothing but a high school education and was proud of it and this rubbed off on his son a little

bit and he, also, had left school early and had very little to offer in terms of knowledge of the physics of what was going on. And, he, also, was going to be a self-made man. But, he didn't seem to me that he was quite finished in being made, yet, and he offered little contribution to the whole idea and we also met Mr. Noland who was the chief engineer of the optical department of Farrand Instrument Company who designed very high quality instruments and more specifically infrared instruments that were used in the wartime activity of Farrand Optical Company. They had a large secret portion of the plant which they had all kinds of things that they couldn't tell us about. At any rate, Mr. Noland said, "Sure, he would know how to make a fluorometer and then if you just wanted one, you'd just tell him what you want and he would make it." But about that time, we were considering coming down to NIH, and I was anxious to do something more with this and I had done nothing but the preliminary experiments with carbon arcs and I think at home instead of at the laboratory. Now, it's vague to me just how some of these things went along but I had proposed that we might explore the field of organic fluorescence by using two monochrometers and these two monochrometers would be something of $5,000 or $6,000 a piece and they could be used to isolate both the ultraviolet exciting light and also the ultraviolet emission and it seemed to me that Mr. Farrand had more or less said that, "Yes, they would lend them to me." And, when we got down here to Bethesda, we were now in a government laboratory and before we were poor relations that were

relatively unfunded and had no great amount of funds available to buy $5,000 monochrometers just to see what they would do and it seemed to me that they started dragging their feet immediately about sending the monochrometers because my impression was that they felt that now the government had lots of money and they ought to be buying them rather than giving them to us and I don't quite know how Dr. Shannon had felt about buying $10,000 worth of monochrometers at that particular time and I wasn't particularly anxious about getting such fancy equipment because I felt that it might be in excess of what was needed. These were very high output; very excellent monochrometers, one of which I have in my laboratory at the present time that was obtained from somebody's ultraviolet microscope that had been purchased later--much later than the fluorometer and had become surplus and I picked it up surplus at NIH. At any rate, at this point, I had been anxious to do something with this and I had the frustration of having these monochrometers, dangling just out of reach, more or less, and I said something to Dr. Shannon that I'd really like to get some kind of monochrometers or stuff and he mentioned that there was a lot of captured optical material from the war that was just over and that there were things listed--a list of things that were available from the various optical companies in Germany that had been confiscated in the settlement, and, what do they call it, the War something-or-other board--scientific board had a fair number of instruments listed one of which was a Steinheil--made by the Steinheil Company, a small

spectrometer which had a very tiny coarse prism in it and only a very short focal length so that the prism only dispersed a small amount of the spectrum but it was quartz prism and a prism disperses the ultraviolet more than the visible light so that it had a fairly reasonable dispersion in the region that we were interested in. So, we were looking for ultraviolet fluorescence so there was an opportunity to have a nice instrument that was calibrated that you could do something with and Dr. Shannon had it set up so that I got the Steinheil instrument from, I guess you'd call it, War Claims--or something or other--War something or other Board that took care of this sort of thing and analyzed instruments that were captured, or whatever it was--settlement, and, so, this particular instrument was very convenient for dispersing a spectrum; however, there were no facilities for dispersing a spectrum in such a fashion that you would be able to record it on calibrated material so that you could plot a curve so I built around this Steinheil spectrometer and converted it to a monochrometer that would allow the spectrum to be dispersed and plotted--intensity plotted against wave lengths so we could get a curve of the shape of the spectrum of the emitted light. In experiments and sources for dispersed light, I set up again my own quartz prism and waxed it on to the table top so as to just make it stable and that looked into this spectrometer which was converted into the monochrometer by having the monochrometer traverse a--let's see, how would I best describe that? I don't know. I fixed it so that the spectrometer projected a spectrum in space and fixed it so that a photo

tube with a slit in front of it would move in a--according to a dispersion curve of the prism so as to make a more or less linear presentation of the intensity and this was all made by mounting a phototube on a flexiframe bridgework or structure which was moved by a cam which I cut out of brass and filed to proper shape to get the conversion from the nonlinear wave length of the prison into the linear wave length presentation of--the usual presentation--a wave length against intensity and I used a photomultiplier to get high sensitivity so I could measure the small, very small fluorescence of relatively small samples. The problem of selecting the arc, or the source, and dispersing the source was the next big problem. And, at this point, the selection of--now, this is already at NIH--I started to build up this particular instrument here after I got to Building 3.

HARDEN: Maybe we'd better stop and turn the tape over; we're about to run out of tape.

HARDEN: **This is the end of Tape 5, Side A. This is the beginning of Tape 5, Side B of the interview with Dr. Robert L. Bowman**

HARDEN: Dr. Bowman, I interrupted you to turn over the tape. Go ahead and continue.

BOWMAN: O.K. So, about this time I had the dispersing mechanism and the light sensing part of it made up so that it was quite sensitive to receive the ultraviolet emission. Now, to get an adequate excitation you had to have a relatively bright source of ultraviolet which would be, again, dispersed so that the spectrum would be swept from one end to the other so that you can select a wave length of excitation and then measure the curve of the fluorescence. Well, about this time it became

obvious that what we wanted to know is what was fluorescent at what wave length, and what wave lengths were. So, it looked like it was a good idea to make the instrument a nice survey device in which you would be able to stop one and scan the other. In other words, you could select a particular wave length where you got a lot of emission and then sweep the emission spectrum and plot a wave length against intensity curve for that and then we could select another wave length and then select another exciting wave length and then scan the emission so it looked like that would be an easy way to make it. That was how the idea of being able to make emission and excitation curves separate from one another and that you could then have a emission spectrum obtained from excitation at 350 or 280 or whatever you had to--excitation spectrum you used. So, the invention of the idea of making the two spectra one on top of the other so that you had these two curves both of which would contribute something to identification was born out of more or less obvious requirements. That's what you wanted to know. About this time it became obvious that Bausch and Lomb was beginning to come out with some grating spectrometers which were devised and made as microscope illuminators. Now, they had made a light source in which they had a diffraction grating fixed so that they would be able to select the wave length of light that you used to illuminate your microscope with.

HARDEN: Could I get you to stop and I don't think you have done this on tape, yet, but to explain very briefly the difference between a grating--how does a grating work, as

compared to all of this?

BOWMAN: O.K. Now, the selection of the spectrum dispersing device was either prism or diffraction grating. Now, a prism disperses the light depending on refractive index change. There's a difference in refractive index that's related to the wave lengths and in the prism the spectrum is dispersed because the different wave lengths of light are bent to different degrees and so that the ultraviolet appears to be bent much more than to the visible light and the spectrum is then available--if you present the light in the form of a beam that's shaped like a slit; then each individual wave length will be presented in the field of light that comes out of the prism as lines which are formed by the image of the slit. Now, the advantage of this is that the quartz is transparent and has a varying dispersion depending on wave lengths. The other alternative available which seems to be a simple way of doing things is to use the interference of light that's produced by the phenomenon of grating spectrum. Now, grating is nothing more than a piece of optical reflecting surface that's got very finely engraved with lines which cause the light that's reflected from the surface to form an interference pattern which may be likened to the fact that each of these lines acts like an individual tiny mirror so that the white light that strikes it is reflected from a discrete line. Now, you put a line spectrum into it--a line source into it as well that's parallel to these lines so that you really are putting in a beam of light into a number of very finely spaced mirrors, and each of these mirrors being a fraction of a micron in width, produces

a reflected portion of the light that you put onto it; but each way that the light is separated from its neighbor by another line, so that what you have is as though you had a number of mirrors that are very closely spaced and within a few orders of magnitude of the wave length of light, not orders of magnitude, actually, in the range of the wave lengths of light, you get an effect which is called the interference pattern in which the wave lengths of light are reflected from each mirror cause reinforcements and attenuation of the intensity of the various wave lengths because of the fact that the light is coming in and leaving at a different angle. Now, each little mirror acts as a separate source and each separate source has a spatial relationship to the next source. And, if they're spaced uniformly, each of the portions of the light that's reflected interfere in such a fashion as some wave lengths are canceled by being interfered out of existence; in fact, the crest of one wave meets the trough of another wave and that cancels that particular wave length. Whereas another one, the two crests correspond and that puts intensity of that light brighter. And it's geometrically arranged around the sources in the sense that if you shine the light directly on this slit that you will be presented with a series of spectra, each spectrum overlapping a bit of the next spectrum as you look at the light returning from the grating you will see individual spectra as you go around the point of illumination and these spectra overlap one another and are spatially separated according to the diffraction grating theory that will indicate that each multiple of spectra overlap one another. I don't know how quite well to

express that, but it produces a number of spectra and they're numbered as zero order, it means a spectrum that comes back right at 90 degrees to what the light shines and then the first order is the first spectrum that is presented off to the one side; there'll be one to each side of the 90 degree position, which represents 90 degrees from the lights going in back of the mirror and these spectra overlap because the wave length multiples that overlap are related to the number of lines on the grating and the wave length so that you would get an enhancement of the light. If you got the enhancement of the light at 200 millimicrons, again, I'm using the old terminology, I should be speaking in micrometers, that the light at 400--I guess we got to talk nanometers, or whatever they're going to be--the multiples of the light--the half wave length light and the double wave length of the same light all overlap one another. So, that there is some confusion in the diffraction grating spectrum. Now, the other thing wrong with the diffraction grating is the fact that all of the light that goes into the mirror is dispersed over a whole plane of illumination that comes back out of the mirrored surface. Now, it's dispersed over the full 180 degrees that's in front of the mirror, so the light is diminished by the fact that it's just distributed over this wide area instead of being all concentrated in one place. Whereas, in the prism, it's just one line is dispersed and not overlapping and none of it is wasted. But a new method of making gratings which requires a shaped line to be put onto the plate was invented by, I can't remember but there's a system of producing a blaze; now, a blaze means that

you fixed it so that instead of using little mirrors that are able to just reflect the light right straight back and allow the interference all over the--at all angles, the blaze is fixed so that the mirror at the line has a preferred direction of scattering so that the line is no longer just a single scratch but a shiny surface which actually has an angular orientation with respect to the surface of the grating. Now, this angular orientation produces more light to be reflected at one angle than another and the result is that you can see when you look at this diffraction grating that as you look at it and rock it, so that you can see the light flashing from different portions of the grating that you will see that there is a particular region of the--a particular angle at which if you look at this grating that it seems to blaze out. And it says that it is blazed, so that the result is that if you look at diffraction grating, and you will see spectra all over the grating, no matter how you look at it, but at one angle it seems to blaze out much brighter. And that's why they call it blazing.

HARDEN: And this was developed when, approximately?

BOWMAN: In what?

HARDEN: What time period was this particular fraction spreading developed? Late '40s, early '50s? Earlier?

BOWMAN: No, it was discovered quite a long while ago and I think Strong in Chicago made blaze gratings and I'm not absolutely sure of that. But, the cutting these lines themselves was quite a job since they have to be micro dimensions and the usual

grating is in the order of 500 lines per millimeter, and so that the ability to rule these lines uniformly and properly spaced is a big enough job by itself but to rule them so that they have a prismatic surface, that it has angles on each side of the scratch was something of a specialty and not only did they fix them so that they were very smooth and properly reflecting but with these very fine lines but they also aluminized the surface to make it highly reflecting. And there's an opportunity in how you put the aluminum on by producing some shadowing effect of having the source on one side versus the other side of the grating when you evaporate the aluminum onto it. You have several techniques which you can use to concentrate the light in one place. Now, these new gratings that came out from Bausch and Lomb were devised by making replica gratings. What you did was you engraved a glass sheet with very carefully cut lines and these lines were made so that they were exactly lined up and spaced and resulted in a blaze that would be selected by wave lengths--be selective of particular wave lengths--and a particular grating could be made so that something like 80 percent or 90 percent of the light at 4000 nanometers would be, I'm talking three different--let me stick to millimicrons. I don't know what I should--what angstroms, or whatever. I was most familiar with angstroms and I spoke that language the best before the International Physical Society decided that we would no longer talk angstroms, but would be now called nanometers--that's 10 to the minus 9 meters. And, it's an awkward enough figure to think of and the wave length that you were selecting

if you wanted--if you had a material that had--you wanted the diffraction grating to produce a good spectrum at 4000 angstroms--we'll translate that later, you could have the grating blazed such that 80 percent of the light that struck the diffraction grating would come back in that area that was illuminated by 4000 angstroms.

HARDEN: One reason that I asked you that is that I know you at some point when you began to work on this instrument you talked to Dr. Brackett over in Building 2, and if my reading is correct, he said that gratings were sort of out of the running for this instrument; recommended a prism and I just wondered if he was not accustomed to working with the new kinds of things.

BOWMAN: Well, that's right. Now, the problem with selecting diffraction gratings was that a quality diffraction grating was the sort of thing that if you made one successfully it would have excellent properties; you might get a Nobel Prize as a result of this. And, Dr. Strong at Chicago Laboratory--I guess, University of Chicago, was able to produce these very accurately made diffraction gratings, which were feats of success which made them extremely expensive and the replicas that were made at that time were cheap but not very useful and the Bausch and Lomb people started making quality replications of their gratings such that they were high quality gratings that were replicas so that the money--the cost of these things--were now down to a few hundred dollars instead of many thousands of dollars per instrument. And Bausch and Lomb first came out using

their diffraction--the way to get a diffraction grating from Bausch and Lomb was to buy a monochrometer type of microscope illuminator which was made for the purpose of seeing fluorescence substances under the microscope. Now, they were particularly concerned with fluorescent minerals, which were extremely brightly fluorescent and you could do studies on rocks that had tiny little inclusions under the microscope with this fluorescence illuminator. And the fluorescence illuminator was also used to some extent in seeing contrast of unstained specimens because of the particular absorption of DNA as differing from other parts of the spectrum you could see there was some contrast produced by absorption of the DNA and also there was the possibility of fluorescence; however, this monochrometer was available and it had a wide, high aperture which means that a lot of the light went through it and you could get it with the blazed grating and it seemed to me that was just what I wanted because I wanted to be able to have as much ultraviolet as possible reach through to the specimen to get a good excitation and it had a knob on it that read in angstroms or nanometers so that you could select what wave length you were concerned with. So, I suppose I'm a little bit ahead of myself in terms of actual development of the instrument in the sense that I actually did get a diffraction grating before I got the monochrometer. And I wanted to see what they were like and how they were only about $100 a piece at this point and I bought one or two of these diffraction gratings with a special blaze to select the wave lengths that we wanted and

produced a mock-up monochrometer which was nothing more than a stone table top. A piece of stone table top, in which the pieces were waxed into place with beeswax which is a very nice way of mounting something that if you want to have a very precise location that has to look in a particular angle and a particular place you can slowly lean on them and ooze them into a new position which as long as the weather stays cool, it stays in the same place. And so I waxed up a couple of systems and was amazed at the effectiveness of the grating. Now, about this same time, I was trying to get the proper light source. To get the proper light source, there was nothing available that would really put out high ultraviolet output except something like what's called an iron arc and the other one was the super-high intensity mercury arc lamp in which the mercury was confined to the capillary about three millimeters in diameter and about two inches long and energized by about a thousand watts of power. So this tiny little tube was radiating 1,000 watts or so, which meant that it would melt in a very short time if it wasn't very effectively cooled. It was put up in a system in which cold running water was rapidly flowed around the tube at a high rate so that you could cool this quartz tube enough so that you could put 1,000 watts of energy into this without melting everything. The arc was extremely bright and the interesting properties of this particular arc is the fact that mercury arc forms a spectrum which has a few lines if the mercury is under relatively low pressure and the lines are discrete so that you can separate them and that was the advantage of the earlier Coleman

spectrofluorometer and the fact that you had isolated lines. But, if you raised the pressure in the mercury, the lines broaden, and they broaden out enough so that they fill in some of the space between the lines and you can get ultraviolet light at other than the specific lines of the spectrum if the pressure is high enough. This was the super pressure quartz arc lamp which had a very high degree of broadening and was an excellent source of ultraviolet light in the vicinity of the lines that were being broadened. But, there was quite a bumpy spectrum if you plotted it. And the 1,000 watt arc is a very cumbersome thing to use and it requires a high flow of several gallons per minute of water through a concentric tube that holds the quartz tube and, of course, all of these tubes that convey the water and the water is also important because the light that's coming out of the arc is being filtered by the water and the quartz tube that confines it. It had to be a special quartz tube and a special device of one sort or another which we rigged up and ran in the laboratory at great nuisance; it was just a terrible problem and the output of this lamp was so high that it was almost unmanageable; it was like trying to work with a laser or something of the sort which you had to be very careful with getting your eyes exposed to this high intensity ultraviolet which we had specifically let out of the--let the cat out of the bag, so to speak, to be able to work with it. And, as I recall, some of the intensity was quite startling. I had put the arc in a black crackle finish container about six inches square so that the lamp was several inches away from the surface of the paint, and blackened inside

so as to not have too much of the light scattered around in all directions. After warming it up and running it for a little time, I was surprised at how much scattered light was getting all directions and I looked inside and the black paint had been bleached through white paint by the intense ultraviolet radiation in the inside. So, that there was no dearth of ultraviolet, except that it was bunched in a relatively awkward place and that was also a terrible nuisance to have to keep this cold water supply and the power and so on which required a huge transformer that was also a great nuisance. Of course, the lamps would occasionally break and you'd have to take it all apart and replace these lamps and clean out the broken quartz and stuff from the capillaries. They were not very expensive, they were replaceable but they were certainly a terrible nuisance. So, I was not enthralled by the high pressure mercury lamp and I had hoped that we could get something better than that. An alternative to this was the iron arc or the iron charged spectrographic arc which was a carbon arc in which these metals were included in the core of the carbon so they produced a kind of a what was called a flaming arc in which the arc was sustained through volatilizing these metals into the electric field of the arc and making bright emissions and, of course, this has to be operated in a kind of an open arrangement so as to let the fumes and smoke out of the chimney conveyed away; otherwise you would be breathing these fumes of these metals that were being volatilized in the arc. The other thing with the arc was something that it had very little stability because of the convection currents

produced by what was almost a flame. Now, other arcs were iron arcs, and these were high intensity sparks that were produced by condenser discharge that would discharge across the ends of an iron electrode which--an iron has a tremendous number of lines, very close together. So, that the iron arc is a nice source of ultraviolet but it's in lines which are not broadened but they're discrete lines and you can see them beautifully and it's nice because some of the lines of geometric spacings are such that you can recognize the wave length of those particular line groups in a kind of self-calibrating way. I made an iron arc and the iron arc is a very nice device as far as the ultraviolet was concerned and had great intensity but the operation of the iron arc is produced by condenser discharge of many jewels of energy produced by each spark and each spark makes a noise and then they're run at a frequency which is--well, we run them at 60 cycles, or thereabouts, and the discharge may be that frequency or a little bit higher and the noise is deafening, to say the least. It is absolutely a screaming Mimi of a thing; when you turn it on down in one end of the hall, everybody opens their door and looks out and says, "What was that"? or something. So, it was certainly not a device to be desired. And, of course, the condensed spark consumes the metal as it continues and, of course, the ability to make a uniform output from something that's consuming itself is not very easily done. So, the iron arc was something I learned to hate, as well. It was not very desirable what you could do with it. Then, I heard about a lamp which was made by Hanovia--no, it was first made by

Osram in Germany, I guess. But it was introduced here in the United States through Hanovia and Hanovia lamps were being developed for the motion picture photographic industry. They wanted lights that would produce the equivalent of sunlight, quality light in high outputs and the reason for putting them in quartz envelopes was merely because they wanted to make the lamps so that they wouldn't break and so that they would produce the high intensity. You had to have an electrode that was able to handle the arc and it was a tungsten electrode with an electric arc through the tungsten and the emission was produced by the presence of xenon in the lamps. Now, xenon is a high atomic, very high condensed molecule that has a lot of orbitals so that the excitation of the arc of xenon has many lines but they are broadened by the very high pressure so that it makes almost a continuous spectrum. Now, the xenon lamp was available in a 100 watt version and was available only experimentally. And they were available and they were not particularly disposed to the scientific market because they were very anxious to get a lot of these lamps out to Hollywood and the area, and the movie industry was the main place they were making them for. And I had communicated with them and gotten not very far for opportunities to try these lamps and they were not priced yet and they were not really available yet; they were developmental. They were pretty cagy about the construction of them; they really didn't want me to see what they were doing. And, it was obvious that they were not interested in the market of fluorescence which was non-existent,

anyway, at the time. But, I finally communicated with one of the engineers who was relatively sympathetic. And he said, "Well, they've got lamps there that they manufactured but I've got lamps here that we're manufacturing for our internal tests. I'll send you one." And he said, "Don't tell the boss; we'll take care of it." Or something like that. And he said, "I'll ask you for your evaluation, or something like that, as a reason for doing it." So, he sent me a xenon lamp and I had the data on the lamp and what they had been able to do with it and it sounded like it was just ideal for what we wanted. And I took the xenon lamp and hooked it up with my particular substance thing. One of the reasons for not sending me the lamp was the fact that the starting equipment was not available. It required a very high voltage spark to start the arc and then a continued high current low voltage to sustain the arc. And this had to be produced by a relatively large reactor that would be able to handle the high current at the low voltage and a very high voltage starting spark. And I had it all hooked up and he gave me the diagram of what he thought was the best way to make it and what values to hook it up and I hooked it up and blew it up immediately. And, it's a high intensity, high pressure xenon lamp and it was about an inch in diameter and made of fused quartz about a quarter of an inch thick. And it popped with quite a remarkable explosion and I thought, "Lord, after all the trouble of getting this lamp, the first thing I do is blow it up." And, I talked to the engineer--the physicist, I guess, he was, at the time. And, he said, "Well, that happens occasionally. We're trying

to do something about that. It was probably a defective seal, or something of the sort." And he thought nothing of it and sent me another one. So, I was very happy with that. And the next one worked first class. There was nothing wrong with it; it was a globular shape of about an inch and a quarter in diameter and had very heavy tungsten electrodes inside of it and a system of conductively cooling the electrodes so that they didn't melt. They had a wire wrapped around the electrode so as to make a kind of a heat sink that would radiate out so that they wouldn't break them. And they had a very special seal on them which consisted of a molybdenum strip flattened into the end of the tube which was able to sustain these high pressures and it was really quite a nice little lamp. And so I installed this lamp on a Bausch and Lomb monochrometer so that it now gave me a xenon source with the monochrometer available to disperse the light into the specimen and select the wave lengths.

HARDEN: I think we're just about at the end of this tape: Tape 5, Side B.