Edwin (Ted) Becker Interview
July 15, 2005

This is Claudia Wassmann, and I'm doing an interview with Ted Becker. Today's date is Friday July 15, 2005. Okay, so yeah, maybe you can begin with telling me when you came to NIH, what institute you started at, what work you were doing at that time.

TED BECKER: Okay, I got my PhD in chemistry at the University of California in Berkeley in 1955, and I came to NIH in about the beginning of November, 1955. I came here to work with a man named Earner Ledell [spelled phonetically] who was interested in doing studies in hydrogen bonding. This is interaction between molecules; it's important in the properties of water, and also what holds proteins and, like, acids together. He was interested in working on this with inferred spectroscopy, and I had done inferred spectroscopy at Berkeley. When I came here, I came actually as a commission officer, and knew that a couple of years as a commission officer would take care of any military requirements so I wouldn't be drafted, and at that time I was eligible to be drafted until age 35. But I expected to stay here only two or three years. Then NIH turned out to be a marvelous place to do work, so I spent my whole career here. But initially, I came to the institute which was known as the National Institute of Arthritis and Metabolic diseases, NIAMD, which later changed its name several times and is now NIDDK. Interesting, at that time, the Scientific Director was Hans Stetten [spelled phonetically], after whom the museum is named. He had been here probably only six months or so before I came. And I had no contact with him initially, and did not work with him directly. But eventually, I met him, and he was our boss, and he was interested in the work that we were doing in spectroscopy. Do you want me to say anymore about that?

CW: Oh yeah, go ahead.

EB: So you may want to know what NIH was like at the time. NIH was small, parking was very easy [laughter] until the Clinical Center was built. And that was completed in 1953, just two years before I came. NIH had consisted of these small buildings clustered around Building 1. It had no clinical component and a rather different structure from the way it later developed. They didn't have scientific directors in each institute. The institute was run by a director and almost all commissioned officers, and it was very much a health service type of hierarchy. I came as a commissioned officer, so I fit into that very well. But there were civil servants, many, many civil servants here too. And things were changing. People like Stetten and his counterparts in other institutes had come in -- as I said -- within a year before the time that I came, and started developing much more sophisticated research programs, both in the laboratory and the clinic. So it was an interesting time to be here because NIH was really changing. I remember going through Building 10, which was just the original part of building 10 without the ACRF and all the other things, and lots and lots of empty labs. People were just moving in, NIH hadn't expanded – had not expanded at that point. And it was a good time to be here from my standpoint because NIH did expand, and our lab doubled in size over a period of relatively few years. Physicians were available, and they were encouraging more people to do research, so it was interesting.

I think many of the people were not the highest level scientifically. I felt a little bit unhappy with some of my colleagues in the lab. Some were very good, but others were government scientists who had been here working for the government, doing whatever it

was they were supposed to do, for a long time. They were competent, and they were conscientious, but it was just kind of a "well, you come in and do your job" type of thing. And then after I was here for a year or two, more people began to come in with the same kind of attitude, I guess, that I had – wanting to do research, people who'd been trained recently in good universities. And it changed the atmosphere quite a bit. There was a lot of interaction between people interested in biophysics and what would later be called "molecular biology," although the term hadn't been coined by that time. And we had a number of seminars, discussions of this sort. My own work really wasn't involved much with anything biological. I was a chemist; I was interested in doing molecular structure studies, but on basically small molecules. And from the standpoint of a physical chemist, not someone looking particularly at the biological problems.

CW: Okay, so how did you get involved in NMR?

EB: Well, the two people that I work with most closely, Earner Ledell [spelled phonetically] and Fred Bracket, [spelled phonetically] who was the chief of our section, were interested in NMR. Bracket was a physicist who had become famous back many years earlier from his work on spectroscopy, and then he got into biophysics. He knew that NMR existed. NMR was discovered basically, as we know it, around the beginning of 1946. And bracket was aware of the physics that was going on, and he contemplated building an NMR spectrometer. In fact, he had a magnet. It was a small magnet, it would not have been adequate for doing NMR, but he was going to try to do something with that, but he never quite got to it. And Ledell [spelled phonetically] was a physicist who had worked in spectroscopy, principally infrared spectroscopy, in the 1930's, and then during World War II had been an administrator in the office of Naval Research, where he met my mentor at Berkeley -- and that's how I actually happened to get here, because of their acquaintanceship. But Ledell had had an occasion to learn a little bit about NMR while he was at the Office of Naval Research, just from conversations with a professor at Harvard, and actually had a short publication on hydrogen bonding by NMR. He didn't know much about the subject, really, but he knew something, and he was interested in it.

So, about a year after I came here, there was interest in "should NIH buy an NMR spectrometer?" And there was really only one company in the country that was making NMR instruments at the time, Varian Associates in California, Palo Alto. He knew some people there that I had met while I was in graduate school near by Berkeley. So it was arranged that I would go for a week to Varian and take along some samples and run spectra and NMR spectra and see what we could learn. And I did, I didn't actually run anything because the instruments were so complicated to operate, you had to have someone who was a specialist who knew just what to do. I just sort of looked over his shoulder, and we got some very interesting results, published a paper. It's pretty good, one week's work and you end up getting a paper. It took some work in interpreting the data, but there wasn't much time spent on the experiments. We did some other things, too, and I came back and reported on what NMR would do and what it wouldn't do. And at that time it really wouldn't do anything with large molecules for reasons that I won't get into about the way molecules tumble; large molecules had very broad lines, and it was not easy to disentangle them in the NMR spectra that you could get with instruments in those days. But small molecules, you could get see something, and you could get chemically important information. So after looking over a report that I had, Ledell and Bracket thought we should buy the spectrometer. They requested that this be done, and Stetten approved it.

An interesting sideline to this is that while I was in Palo Alto there was a discussion at the scientific directors meeting, and Stetten's counterpart from the Cancer Institute said, "You know, there's this new NMR, and NIH ought to do something about it," and Stetten said he was delighted to be able to say, "Our institute is doing something about it. We have Dr. Becker now at the manufacturer evaluating it." So, I think that was really kind of helpful because Stetten was probably encouraged to approve the funds to buy this because then his institute would be the first one to have NMR at NIH. And the instrument cost $30,000, and that was the most expensive instrument by far that this institute had ever purchased. So, it was not a decision that's made lightly, and it required a lot of lab space, and air conditioning, and cooling water, and all sorts of things.

And we managed to find the space in Building 2 to do this and got the instrument. It arrived in 1957. It was an instrument that operated with a field of what would now be called about one tesla and the frequency for the proton NMR was 40mhz. I mention that because we'll come back later to talk about other instruments of higher fields, and those numbers become important. So, this was a 40mhz instrument. It was good enough to do a lot of things and we started working on it, but it was very difficult to do experiments with that kind of instrument. It was just tedious, difficult to operate. And we hired a technician to work with me, Bob Bradley, who was very capable and patient –- which you needed -- and working all day when we'd sometimes only get the very little bit of data out.

CW: So, at NIH, did you then, start to improve the instrument?

EB: No, we really didn't at that point. The lab that I was in, which was in the laboratory of physical biology and later split off and our part became the laboratory of chemical physics, has always been interested in instrument development but more from the standpoint of mechanical and optical instruments than electronics. And at that time, we didn't have, really, people and sophistication and expertise to be able to do much to improve the instrument -- make little small modifications perhaps, but not much. And we had to really rely a lot on Varian, and other manufacturers who came in with various accessories to get instruments. But as time went on, then we started making changes. We learned a little bit more about it and ultimately developed expertise, and the biomedical engineering group later was very helpful. And I have collaborations with people from the then National Bureau of Standards, now National Institute of Standards and Technology. And a lot of that development was specifically on new instruments and new techniques for doing NMR. So, I spent quite a bit of time on that later. That occurred mostly, I would say, in the 1960s -- late 1960s and the early '70s.

CW: What were the first scientific problems you tackled when you had these instruments here?

EB: Mostly structures of small molecules -- steroids [?], alkaloids, compounds of not very complicated molecules -- and the studies of molecular interactions. Remember, I was interested in hydrogen bonding and the interactions between molecules, so we certainly worked on that. I guess my first NMR paper was, of course, from the work done at Varian, and then some later ones were on similar topics. One interesting early study was on porphyrins. The porphyrin ring is the basis of hemoglobin, chlorophyll, and various other things. And no one, at that time, had done any NMR on porphyrins. Now there was good reason for that. I talked with the people at NIH about porphyrins, and I knew that a number of people were working with porphyrins, and I said, "Well, if you could give me a sample, maybe we could do it." And they said, "Fine, how much material do you need?" And I said, well, we needed about 10 milligrams. And they'd say, "You

mean 10 micrograms." "No, no, I said, 10 milligrams." "Oh, well that's a thousand times more than we would have available." So nothing got done.

And then the Board of Scientific Counselors came, headed by a man named Cecil Watson [spelled phonetically] from the University of Minnesota, and well known to Stetten, who had actually appointed this group. And I presented some things at the meeting of the scientific counselors about NMR. Watson, it turned out, was an expert on the disease called "porphyria," where porphyrins accumulate in the body. And in order to make studies of porphyrins, he had a herd of cows who accumulated porphyrins, and they would isolate the porphyrins from the waste products of the cows using presumably large amounts and getting some porphyrins. And he was able to give me ten milligrams each of about a half dozen different porphyrins. And about 1959, I think, we made the first NMR spectrum of a porphyrin, and found some very unusual things, which I won't describe, it's the way in which the spectrum looked. Subsequently, other people were able to synthesize porphyrins -- I had no ability in this line, but places -- there were not anyone at NIH doing that, but other labs people were able to synthesize porphyrins and then they went on and did a lot more work on porphyrins. And I just had these two papers on porphyrins, and then sort of got out of the field because I didn't have any more samples.

But that was interesting, and there were a lot of other people who did have problems at NIH. I remember when we got the instrument, there clearly would be only one such instrument at NIH for a long time, and Stetten, who was a very good scientific director, said to me, "I will probably send people over to you if they have problems that it looks as though you might be able to solve. And listen to them; you don't have to work on anything you don't want to, but if you think you can help them, that's fine. If not, listen to them and send them away." And this delighted me because it meant that we had an opportunity to reach out and collaborate with other people. And a number of organic chemists did come with that. The statement that a lot of people would say was, "Would that big machine of yours help me learn whether the structure is this or that?" and sometimes it was possible, sometimes it wasn't. People even came with things, there was a neuroscientist here named Tasaki [spelled phonetically], who I think died recently, very old, who used to come over to see whether nerve excitation, whether it would change the NMR spectrum of water. And he never got much out of it, but we had all kinds of different things. But it's interesting, I learned about a lot of other problems, and some of the things that didn't materialize, didn't work out from the NMR standpoint.

But many things did, and we published papers, and sometimes we collaborated; most of the time we just gave them the spectra and got an acknowledgement of it. There's no reason to -- I never wanted to get my name on somebody else's paper. But it also meant that sometimes I met people who were then able to synthesize molecules that I was specifically interested in, who would help me solve some NMR problems. And that worked out very well. So it was quite a good atmosphere, and I think a very good way of starting.

CW: Would you have predicted the enormous success of the technology?

EB: No. I wish I could say, "Oh yes, I saw this right from the beginning," but absolutely not. When I first heard about NMR while I was a graduate student at Berkeley it seemed interesting. But the examples that I saw being done at that time were not molecules that seemed like anything very interesting. And when we first obtained the NMR spectrometer at 40mhz, we really were limited, both because of the way the spectra were

obtained at relatively low field -- now we're dealing with fields that are 20 times as high, and the spectra are spread out by a factor of 20 so that you get much more information. Moreover, the sensitivity of NMR is relatively low. And now it's factors of 100,000 or more greater than it was then. So at that time, you'd need a large sample, and you got a very limited amount of information. And I would have said, at that time, that it would be essentially hopeless to be able to study proteins. I was wrong. That has now become a major aspect of NMR. I think there were -- clearly though from the very beginning, there were some people like Russel Varian, who founded Varian and Associates, and who arranged with one of the inventors of NMR to patent that technique and assign that patent to Varian Russel Varian didn't know what it was going to do but was sufficiently convinced that there would be some applications that would be commercially valuable --- that it was worth getting the patent, and worth it for them to put a lot of money into developing the technique -- which they did over a period of many years. And then eventually other companies came in, particularly after the Varian patent expired in 1963. But so some people certainly have always been able to see that there's something more to be done. But I don't think anyone really would have predicted the enormous applications in this field.

CW: So was it then -– what was it then? The costs of breakthrough sort of thing?

EB: It's a series of things. The companies, beginning with Varian, always tried to push for higher magnetic field and improve sensitivity. And those things have come along with advances in electronics. That's okay, that's okay. So they have really pushed, and of course there have been a lot of academic people interested in doing things, too. But mostly it's been the instrument companies. Then – and as more companies came into being, then there was some competition – and there still is – and that of course helps. So it's the improvement of the technology, and this has been driven by a need to do it. I mean, obviously there are important biological problems particularly that need to be done, and so there's an incentive for companies to put sufficient resources in if it is technically feasible. And over time there have been many improvements, there have been some really substantial breakthroughs that occurred in the late 1960s. I know in obtaining biographical information from people for the encyclopedia of NMR, there were several people who said they were advised by people around 1968 to get out of NMR. All the important work had been done. And then suddenly, these new concepts and new instruments became available, which really changed things dramatically and made it much, much more exciting than it had been earlier.

CW: So what role did the computer play?

EB: It was extremely important because there's a technique know as Fourier Transform NMR, which the principles of which were known in the 1950s, but this could not really be applied. By 1965, two people at Varian, Richard Ernst [spelled phonetically] and Wess Anderson [spelled phonetically], published a paper showing that you could use this technique, and in principle, improve the sensitivity of the NMR spectrometer by obtaining the information more rapidly. So that in a given time, you could go back and repeat the measurement and build up the signal, whereas the noise is random and tends to cancel. And their first experiment in this line required that they obtain the data on paper tape or something like that, take it across the city to a computer center where the spectra, where the data were processed, and eventually a spectra would appear. Now this actually was no time saving because it took them a day instead of an hour to get the results. But they demonstrated the principle.

Later in the late 1960s/early 1970s, my colleague, Tom Ferrar [spelled phonetically], at the then National Bureau of Standards, and I worked with some other people to develop carbon 13 NMR by Fourier Transform and we did the first carbon 13, basically imitating what Ernst and Anderson had done years earlier with protons. Carbon 13 is about 100 times harder to study, which is why it took so long. And we had exactly the same experience. Tom obtained data at the Bureau of Standards, put it onto I think it was magnetic tape. Jim Feretti [spelled phonetically], who has just now retired from NIH, brought it to the computer center at NIH, and overnight, had to convert this magnetic tape to paper tape, going to another computer, doing the Fourier Transform, doing various other processes, and got the spectra. But this stimulated the companies to develop special purpose computers. This was long before the era of personal computers, and most computers were big mainframe computers in computer centers. But the work really did stimulate companies to develop special purpose computers, which were designed to obtain NMR data and do all the processing within the computer. And there was one company called Nicolae [spelled phonetically], and we got one of the first instruments that they produced -- well we didn't, the Bureau of Standards did because we were collaborating and the work was that particular kind of project was being done there. And then Varian developed computer capabilities and modified some of their instruments.

So the computer really was critical. None of this could be done without special purpose computers. And it's interesting, the computers were very limited at that time -- we had something like, oh maybe 4,000 bytes of information. Well, right now you talk about everything you do in megabytes, right? These weren't megabytes, these are kilobytes. This was a factor of a thousand less. And that's all you had, and all the programs have to be put in there, as well as the data. So it was a very different era.

CW: And then the moment when there was a personal computer and it was a graphics-enabled computer, and you could create pictures of these molecules, did that have an impact for the scientists, or is it more for the presentation of the scientists?

EB: Oh no, it has a great impact for the scientists because first the spectrometers are now all run by computers, you don't see any knobs or anything, everything is done with software. And you obtain data in various ways before it's presented as a nice picture. There are data presented, obtained, and analyzed by the computers in ways -- again, without going into the technical aspects –- one refers to as multidimensional NMR. Instead of displaying the things along one axis there are multiple axes, and it's basically essentially a multi-dimensional space if you can picture that, which is hard to picture, but the computer can deal with all of it. And then it extracts information that one needs, and then you can display something that tells you what happens in one particular or two particular dimensions, and then interpret those results.

So the computer is extremely important in doing these things as well as making the displays so that all of them –- when you get the structure of a protein or something like that, you have a very good three-dimensional type of display. And some of that early work on the display was done at NIH in the computer division. Back in the late 1970s we worked with people there on structures of molecules determined by NMR -- simpler, not proteins at that stage, smaller molecules, molecules that aggregated and made sort of complexes, the question was how did they fit together. And one could look at this with the computer graphics techniques that were being developed at NIH, and NIH was the leader in the computer division. And you could get space filling models, which -- not available in most places. And it was very helpful to interpret the results, published several papers and things like that.

CW: So for how long did you continue doing this kind of work?

EB: Well, the NMR has really been something that I've been involved in a lot, for a long time. I'm still involved in it a little bit. I don't really do much research, but I'm dealing with a few things on how to present data, and dealing with international agreement on the best way to present the results and recommendations for simplifying what one can do, and some experiments on little things about temperature dependence of certain – NMR resonances and so on. So I've been involved in it, some very actively for a long time. When I went into administration in the end of the 1970s, my active lab work really almost ended – I had a very little bit, but not much. And then when I returned to the lab at the end of the 1980s I really did not personally get back into doing lab things again. And meanwhile our laboratory had recruited some very good people while I was away. Ad Vax [spelled phonetically] is one of the leading experts at NMR, came just after a post-doc, came here in I think it was about 1983, and came to our lab and has developed a big program in NMR. And then we decided that……more came and developed more programs in that line. And meanwhile, there were lots of other people of NIH doing work beginning with simpler molecules and then going to more complicated molecules in NMR, Dennis Torschia [spelled phonetically] for instance, in the Dental Institute, is one who has done an enormous amount of very good work. He came here from the Bureau of Standards after working there a couple years. And when he came, I was called by his lab chief, saying, "What kind of instrument do we have to buy in order to get him to come here?" And then Dennis came to me and said, "Instead of buying a simple instrument for our laboratory at the Dental Institute, why don't we put some money into your instrument in Building 2 and I can work there and do some of my things and then we'll all benefit." And that was the beginning of what has become a very good collaborative program at NIH and still exists with all the protein studies and so on, that different institutes have put money into buying instruments that we can all use. And it's been a great cooperation. So even if in areas where I have not been personally involved with doing NMR research, I've sometimes been able to facilitate getting these cooperative arrangements made for NMR, and chemical NMR, you might say.

CW: And so you had a lot of experience working at NIH, a lot of experience in collaboration, experience in NMR, and all of that brought you into a good position to become the architect of the in vivo NMR center.

EB: Yes, it turned out I was. As you say, I had a lot of experience, and there were many people at NIH during the 1970s and early 1980s who were doing biological applications of NMR with real in vivo systems. Either some of them were with -- first with just cells, then with excised tissues, and then with small animals, and eventually, as it became possible, with humans. And people in different laboratories were working on these things. And there were some experiments with animals but the instruments available were really still very primitive. And we needed larger instruments both to study animals, and particularly for humans. And at the time, in about 1984, all of this sort of came together with a number of my colleagues coming to me – and this was while I was an associate director of NIH and was spending most of my time in administrative work in Building 1. But obviously I still had a lot of knowledge of NMR, I was interested in it, I went to NMR seminars and so on. And my colleagues came to me and said, "We really need to get together and do for the in vivo area, what has been done in other places. But we need new instruments and we need to figure out where we're going to put them." One of the things that was particularly a driving force, was to be able to get a whole body – human body – instrument, one meter bore instrument into the clinical center. The

diagnostic radiology department had one at low field, but it needed an instrument of one and a half tesla, whereas what it had was a half tesla, could barely be operated there because of all the steel and so on around it. And one of my colleagues, David Holt [spelled phonetically], who's a superb instrumentalist and who was then at the biomedical engineering branch, looked for all through the Clinical Center, was there a place that this instrument be located and be accessible to patients? He and I sat, looked over the plans, and there was always an elevator around or there were cars going through the garage or there was something around that prevented our being able to site an instrument there. So we ultimately decided that the best thing to do would be to have another building, a little addition to Building 10, connected to it so that patients could be brought there easily from the radiology department. But this would house an instrument, a one and a half tesla instrument that would be used for patients and for research subjects doing volunteers, and we would also have two instruments that would deal with animals.

And so we began discussing this in the summer of 1984, and we concluded that we should have a building, we should buy instruments from a company and get them to construct the building because the building had to be done in such a way that the steel would not interfere with the magnets. So it was going to be a so-called turn key operation. And in the fall we made a proposal for an NMR research center, got several responses, made visits to manufacturers of clinical and laboratory animal instruments. And by the March of 1985, we made a proposal to the deputy director for intramural research and to the --- ultimately the scientific directors to create an NMR center. So that was a very, very interesting thing, and I was really in a good position because I knew all the scientific directors and institute directors, I had attended all their meetings in my administrative capacity, and I knew all the NMR people because of a history of dealing with that. And it was really something that was worthwhile I think, to bring all of these people together. And the time was right, the scientific directors were very receptive. Ed Raul [spelled phonetically], who was then the Deputy Director for Intramural Research, he had been Hans Stetten's successor as Scientific Director in NIDDK, and then later became the Deputy Director for Intramural Research, Ed Raul had known about NMR from the very earliest time. He was an investigator at – well, it was then NIAMD -- when we got our first NMR spectrometer. So he had sort of followed this along – knew what I was doing, and my other people were doing. So he was very much on our side. Of course he presides the meeting of Scientific Directors, and the scientific directors heard from scientists in their institute that a center of this sort would be useful. John Dotman [spelled phonetically], who was head of diagnostic radiology, felt that it was absolutely essential that we have something of this sort.

So with all of this we put together the resources. We got very enthusiastic support from the scientific directors, and it fell to me to go to the scientific directors of each institute and ask them for money. And I kind of drew up a list of how much each institute should contribute based on the size of the institute and how much we expected them to be using the center. And every institute contributed, even those that really couldn't see any use of it, probably never have used it, they still felt that it was worthwhile having it and they thought that they wanted to be kind of included in it. And that set the stage for how the NMR center has operated ever since. We got the money on a voluntary basis from the scientific directors. We got enough money to be able to construct the building, buy the instruments and operate it for a few years. And then, once it got going, then we set up a mechanism by which the users of the facility would retrospectively be charged on the basis of how much they had used it over the preceding year or so for three quarters the cost of the operations center, and all institutes would pay the other one quarter based on

the size of the institute. So that means that everyone still sort of has a stake in it. Those that don't use it don't pay much, but they do pay something.

And that procedure has continued to mature. There have been changes in it and the administration of it, which started out in my office when I was in Building 1. We moved to the biomedical engineering branch because I thought that was a better place for it. And then subsequently, in the 1990s, the scientific directors concluded that it would be better to put this in an institute and it has gone to NINDS, but with a charter that I largely wrote in 1997 that ensures that center will still operate in a sort of cooperative mode.

And I should say that that was the beginning of the NMR center, but it has really grown. If you look at the outline of the NMR center and what it was initially, and then look at what has happened –- I will give you some illustration of this -- and look at what happened as institutes decided to build their portion of the center, the first one being the Heart Institute, and Bob Balaban in particular, was able to get a very high field instrument, a four tesla whole body instrument. They were able to get money from Congress – the Heart Institute was able to get money from Congress – to build the big expansion of the NMR center. And that's their space, but it's all intermingled, and the people work together regardless of who officially owns the space. And the Mental Health Institute and the Neurology Institute have added additions to the building for new instruments.

So there have been a lot instruments put into the center which are technically belong to one institute or another, as well as those that belong to the NMR center, the shared center, per se. But really, all the instruments get used by everyone. And it's been one of the principles that when a new instrument has been put into the center -- purchased by, say the Neurology Center -- that a certain amount of time is to be made available to anyone who wants it outside of their institute. And that has fostered a spirit of collegiality, and it has meant that people do, indeed, cooperate. The institutes have control, they spend their money for things they want to do, but there's always a feeling that people work together.

We set up a steering committee when the center was first constructed. We talked about how would we govern all of this, and decided that we'd have what we called a steering committee with a representative from each of the institutes that was involved and would like to be there. And these were not administrators from the institutes or directors or something, these were working scientists: people who were NMR people, who worked there, but who could serve with their scientific director's approval as kind of the representative of that institute and speak for the institute. And we had set up a budget that would permit the senator to continue to improve its instruments, that's been extremely important. You have to upgrade -- you don't buy these things and say, "Now we have the instruments, now let's just work." This field developed so rapidly that you need hundreds of thousands of dollars every year just to be able to get so-called "upgrades" without buying a new instruments, just to change because the software changes, the hardware changes, and you get out of date very quickly.

So we're very fortunate that some of the advisors at the beginning, and administrators from the Clinical Center, recognized how much money needed to be set aside. So the budget has been built with that. And the steering committee decides how that money is being spent. And they work it out together, and I've never heard any significant arguments. They always seem to do very well, and it's obvious to them because they're all NMR people, what is that's needed.

CW: That's great. Was there, at the beginning when you decided to make it really a research facility, was there a competition between clinical research and more basic science?

EB: To some extent, yes, there was. The diagnostic radiology department really had a great need for instruments. And they would very much have liked, at that time, to have their one and a half tesla instrument in the radiology department. But it was physically impossible. Subsequently, instruments have improved. The reason we couldn't do it initially is that the magnetic fields of fringe, they spread out, and many, many feet away from the magnet you still have the magnetic field, so that any metal moving through that field will completely disrupt the instrument. Well now, what has happened in more recent years is that the instruments are shielded, they are so-called "self shielded," so there is a field fringing out but not very much. So it is possible to put instruments into the radiology department, and they have, I believe, three one and a half tesla instruments there.

But in the beginning, there was indeed, some competition for time. The radiologists really didn't like having to take their patients down to the NMR center, which is located on the B1 level behind the Clinical Center, but there wasn't any choice. And what we did was give them two days a week for bringing patients in, and the rest of the time, the instrument was to be used for research. Now, it was research on humans but there was not just routine diagnostic radiology; rather they were studies of multiple sclerosis or some particular kind of disease that one of the investigators wanted to bring in patients or bring in normal volunteers to be able to study things. And many people wanted to develop new techniques. So great competition for time on the instrument.

But we always arranged that if Radiology needed the instrument at a time that it was not theirs, that other people could step aside, because obviously there are some important clinical needs; someone suddenly comes down something and you have to have MRI scan, at that time there really wasn't any other instrument of one and a half tesla. There was one, and then later, two lower field instruments that could be used for some routine things but when they needed the one and a half tesla, I think they always got it. But nevertheless, they would like to have had it all the time. And so there was competition, but I think, friendly competition, and I think we all kind of understood it. As soon as they could get -- as soon as the techniques improved so that they could get instruments in the radiology department, obviously they did. And meanwhile, the instruments in the NMR center now have gone to a higher field. I don't think we have any one in a half tesla instruments anymore, everything is now three tesla and some of them are higher – up to seven tesla, which is a big instrument with an enormous amount of shielding. But these are for our research purposes.

CW: When the decision was made that NINDS would take -- when it was moved to NINDS, why was that?

EB: There was a discussion of the scientific directors, and I really was not part of that, because I had long since left Building 1, and didn't go to scientific directors meetings anymore. But there was a feeling that the service activity –- the biomedical engineering branch —- was not really geared toward doing research, and that this shouldn't be put into an institute that had a real research focus. And that's probably correct. I think what we did initially was the right thing to do because initially everyone was worried about one institute sort of taking over the center and ultimately sort of squeezing everyone else out. Well, what happened under the system that we set up with the steering committee and so on and with these additions that were put up by the heart institute and so on, and

Bob Baloban is very important in doing this, played a critical role in setting the right tone. What happened as a result of that was there was some confidence building. I think people recognized that everyone did get along, that instruments could be shared. And the idea of having this within an institute with the appropriate scientific leadership and the review of the science by a board of scientific counselors, it was felt by the scientific directors -– and I agree with them -– to be the right way to go. I think it would not have worked back ten years earlier, but after we had some ten years of experience in operating – and it was just about ten years after the center opened that this change was made, and it went to the neurology institute. But at the time Michael Gottesman asked me to write a charter for it because they wanted to be sure that the practices that we had put in place were now written down. Obviously it's all these people that make the difference, but it's good to have some document that says, "This is the way it has operated," and we wrote into that a lot of history: this is the way things developed, this is why we're doing what we're doing, and this is the way it's supposed to stay, but obviously with changes made as needed. And so far as I know, that's working quite well. It continues in the same manner that it has before.

CW: Very good. I could go on asking more but I don't know if you…

EB: It's up to you, do you want to take a break for a minute?

CW: Yeah, we can do that.

End of Transcript