

RICHARD J. JOHNS

An Interview Conducted by

Frederik Nebeker

IEEE History Center

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**Interview:** Richard Johns  
**Interviewer:** Frederik Nebeker  
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Nebeker: I see that you were born in 1925 in Pendleton, Oregon.

Johns: Yes, Pendleton, Oregon. I am a West Coast boy.

Nebeker: Can you tell me a little about your family?

Johns: My father was a businessman. He founded the Oregon Title Insurance Company. My mother and father met at the University of Oregon. I have a sister who was two years younger than I. We lived in Pendleton, which is in eastern Oregon in the foothills of the Blue Mountains. Then it was largely wheat raising; subsequently they have started raising peas. We had a summer place on the Oregon coast and we would go down there for the summer.

Nebeker: Were you interested in science as a child?

Johns: Yes. I was always interested in science.

Nebeker: So many of the people I have spoken with have tinkered with the crystal radio as a youngster.

Johns: Yes, that's right. I had a crystal radio with a coil on a Quaker Oats oatmeal carton. It is the usual story.

Nebeker: Classic.

Johns: I did a bunch of that. I was interested in what at that time in what passed for hi-fi.

Nebeker: The hi-fi movement was a post World War II phenomena. Is that the time that you were in it?

Johns: It was at the beginning of World War II.

Nebeker: You were on the leading edge of hi-fi.

Johns: This was when they first started having negative feedback from the output side of the audio transformers, which improved the frequency response around the output transformer.

Nebeker: Is this your radio that you are improving the performance for?

Johns: No, a phonograph. I was interested in some of those things, but I was also always interested in medicine. This was at the beginning of World War II when I went to the University of Oregon.

Nebeker: Had you decided that you wanted to be a doctor?

Johns: Yes. But I was also interested in science, so I was a physics major rather than a biology major. I ended up getting a degree in physics, so I never had an engineering degree.

In the summer of 1944 I came here to medical school. This was during World War II in an accelerated program, so in three years I had fulfilled all my physics requirements, but I did not have enough hours to graduate. After two years of basic sciences in medical school, they accepted those as fulfilling the course hour requirements.

Nebeker: So you were accepted into medical school without the...

Johns: Yes. Hopkins was the first medical school right from its founding to require a baccalaureate degree. But they waived that during World War II in the accelerated program. After two years of medical school, I received my baccalaureate degree. People have always wondered how I received my BS

degree in 1947 and then my MD in 1948—maybe I just had one year of medical school.

I came here in 1944. At that time, the medical school was also on an accelerated program, so we started in July rather than September. After my first year in medical school, which finished in March, they went back to the regular schedule, so I had six months off. That is when I had a job with Sam Talbot.

Nebeker: Herman Schwann talked about Sam Talbot.

Johns: Yes. Sam Talbot started biomedical engineering here at Hopkins at the same time that Herman Schwann started it at Penn. One of the sidelights on that was that they would commute between Philadelphia and Baltimore. Herman Schwann would teach some courses here and Sam would teach some courses at Penn. I got a job at the end of my first year of medical school working for Sam Talbot in his lab. At that time, he was doing some research on color vision.

Nebeker: I take it that was not a coincidence—your physics background, and you knew him.

Johns: Yes. I was looking around for something to do and I worked for him. In essence, I was doing some electronic design. This was in the days when you built your own oscilloscopes. I worked for him as a laboratory technician.

Nebeker: How many people did he have in his group?

Johns: He had about three.

Nebeker: Some graduate students?

Johns: Yes. Because he was doing vision research, he was in the Wilmer Eye Institute. In that research era there were a number of really notable researchers, including

Hubel and Wiesel, who received the Nobel Prize for their work in the optic cortex. At that time they were post-doctoral students of Steve Kuffler. So it was an active group.

The other part of it was that Sam Talbot was providing the equipment for Dr. Harvey, who was the Chairman of the Department of Medicine, and Joe Lillenthal. They were studying neuromuscular transmission in man. I was doing some of the fabrication of the equipment. Sam had gotten to know them, which plays a role in my subsequent career.

Nebeker: I would be interested in what Talbot was like to work for.

Johns: He received his Ph.D. at Harvard. He also received his Ph.D. in physics, not in engineering. He was a typical, reserved New Englander, but he had a terrific sense of humor. He was a great guy to work for. He was one those people who had a real gift for explaining things. I learned a lot from him.

Nebeker: Did you think that when you starting medical school that you would be going into research and would want to do this kind of engineering?

Johns: Yes. I continued to work. One of the things at Hopkins Medical School is that they really do encourage you to get involved in research with somebody or other. So I did, and I worked with Sam Talbot, Mac Harvey, and Joe Lillenthal . I became interested in doing research on neuromuscular transmission, so I worked with that group part time throughout medical school. Then I received an internship in internal medicine here at Hopkins.

This was at the time of the doctor's draft. After my internship I then spent two years in the army. Again, I was in the research lab of Harold Himwich at the Army Chemical Center. He was a neuroscientist.

Nebeker: That sounds like either a very fortunate thing or something that took some arranging.

Johns: Well, at the time of the doctor's draft, you had two alternatives. One was to do nothing and be drafted. Or you could volunteer, which is what I did. There were no guarantees as to what would happen, but you could request a given assignment. I volunteered, and I was assigned to the Chemical Corp.

Nebeker: What was your work with Himwich?

Johns: With him I studied the effects of nerve gas on the central nervous system in animals. That is what I was doing in the lab part. The other part of it was that we rotated time as the post surgeon at Dugway Proving Ground in Utah. You had better believe that Chemical Corp proving grounds are not in the garden spots of America. It was in the middle of the Great Salt Lake desert. I had one side of my car sandblasted in a windstorm.

Nebeker: Did it take much of the paint off?

Johns: I took enough of the paint off that it had a dull surface on one side of the car. (Poor me.) So, I spent two years doing that.

Nebeker: Were you involved with the instrumentation for that work?

Johns: Some of it, yes. We were recording from the brain, and it did involve electrophysiology. This was before the days of microelectrodes.

Nebeker: Did you have to open the skull and place electrodes?

Johns: Yes. You would have to put in electrodes and get them into the parts of the brain that you wanted to record from. So there was some electronics required.

When I came back from the Army, I was back on the medical house staff as an Assistant Resident. Then I spent two years as a post-doctoral fellow with Mac Harvey and Dave Grob, again working on human disorders of neuromuscular transmission.

There were a number of engineering improvements that I contributed to. One of the things was to be sure that you could get full, maximum nerve stimulation in man through the skin, and at the same time have the comfort level reasonable, which means that you had to have a very short pulse so that you were not dissipating a lot power.

Nebeker: Was it mainly a question of how much power is delivered?

Johns: It is how much current goes through the nerve.

Nebeker: Does it cause a pain response?

Johns: The current through the nerve membrane is what is stimulated. That is what you want.

Nebeker: You want to be able to do that.

Johns: You want to get a narrow, high current pulse.

Nebeker: The total power is small, but you still get the maximum current in the nerve.

Johns: Yes. That was one part of it.

The other part of it is that the action potentials that you evoke from this are in the millivolt region, so you had to have very good differential amplifiers. Otherwise, if you charged up the input capacitance of your amplifier, all you would be

looking at is the exponential decay from the stimulus artifact. You had to have very good differential amplifiers with low input capacitance to be able to do that.

Those were some of the technical parts of it.

Our research interest was in finding out the nature of the disorder of neuromuscular transmission in myasthenia gravis. There was a big controversy at that time as to whether the problem was in the nerve ending with not enough acetylcholine being released. Or, whether the disease was of the musclemotor end-plate and that a normal amount of acetylcholine being released, but it was insensitive to it.

Nebeker: Were you able to answer that question?

Johns: Yes, we answered that question. The way that we were able to answer it was by comparing the effect of externally injected acetylcholine in normal subjects and patients with myasthenia gravis, when you put the same amount in. Since acetylcholine is quickly destroyed in the blood, the trick was that you had to put it into the artery so that would go directly to the muscle very quickly before it was destroyed. That showed very clearly that there was a diminished response to injected acetylcholine. So we knew that the problem not in the nerve, but in the motor end plate, and that subsequently was shown by other methods as well. So that was my research as a post-doctoral student.

Nebeker: Was it your intention at this point to find a permanent research position?

Johns: Yes. The next step was that I was the Chief Resident in medicine for a year. Then I was given a faculty appointment here in medicine. I had my own research lab, and I continued doing some of the things that we are talking about.

Nebeker: What were the areas of interest initially for you?

Johns: Well, I continued and did further research on myasthenia gravis. Another disorder is familial periodic paralysis, which is not a common disease, but it was an important one. It is familial, so that the families knew what was going on. If they exercised and ate a big meal and then went to bed, they might wake up, in essence, paralyzed. They also would learn that if they could get started and exercise a bit, they would get over it. Now they know that it is a disorder of an ion channel in their muscles, but at that time the thing that we showed was that their serum potassium would go down and that the potassium would go into their muscles. It was called hypokalemic periodic paralysis—low potassium periodic paralysis.

A technological part of this was that this occurred when it began to be possible to use microelectrodes to measure the membrane potential.

Nebeker: What year is this, roughly?

Johns: It was in 1958. So to make a long story short, we were able to measure the muscle membrane potential in man, and show that indeed that was the cause of the paralysis. The things that we first showed was that the potassium disappeared from the blood, but it did not disappear from the body. So it had to be rearranged from the extracellular fluid. The potassium had to be going into the intracellular fluid. Finding this change in muscle membrane potential indicated that it included muscle. We also did muscle biopsies and showed that the amount of potassium had gone up.

Nebeker: Do you know where that work was done in developing these microelectrodes for this kind of research?

Johns: Yes. Ralph Waldo Gerard at the University of Chicago was the person who invented the idea. The question was how can you get an insulated electrode that is fine enough that you can stick it into a cell without wrecking the cell? The other part of it is that it had to be a material that does not have a big junction potential between the conducting material and the inside of the cell. Because if you have a big junction potential, you have no clue as to what the muscle membrane potential is. If you have an electrolytic cell between those, you cannot know. He figured the whole thing out, which was to pull fine glass capillary tubes and then filled them with, at that time, saturated potassium chloride.

Nebeker: So, it is a fluid conductor?

Johns: It is a fluid conductor. You could get a half-micron tip diameter. At first they were pulled by hand, and then with a microelectrode puller. The thing that you would do is stick a chlorided silver wire inside of that, and you would have another chlorided silver wire and you would measure the tip potential as well as the tip resistance. Gerard showed that the resistance of the tip was a good measure of tip diameter.

Nebeker: Trial and error to get the size that you need.

Johns: Yes. So that is how that evolved.

Nebeker: Were you able to purchase these microelectrodes?

Johns: No, you had to make them yourself. The thing that you could purchase by the time that I got into the business were these microelectrode pullers, which had an

omega-shaped platinum loop. You stuck the capillary tube in there, and you had two adjustments. What pulled on the thing was spring loaded, and then it also had a microswitch and a great, big solenoid that would really yank it. You had two adjustments. One was the current through the platinum loop, which controlled the temperature. The other thing was how far the thing stretched before it really pulled it hard.

Nebeker: It sounds as if you operated this.

Johns: Oh, yes. Once you had it right, you would murder anybody who messed with it, because it really was trial and error. You would make a batch of them and see whether they were too fine or not fine enough by their resistance. Then when you finally got it right, you just didn't want anybody to touch anything.

Nebeker: This made a big difference in your research and a lot of other people's research by being able to do this?

Johns: Yes. That is what made it feasible. The business of pulling them by hand was just not the way to do it. They are getting better all the time. These pulled horizontally, and one of the problems was with gravity. It might sag before it cooled. Somebody turned it 90 degrees. That method is still being used. The other research interest that I developed also had some instrumentation aspects. One of my post-doctoral fellows was from England, and he was very much a rheumatologist. He was interested in joint disease. One of the great unexplained things was the nature of joint stiffness in rheumatoid arthritis. The thought was that because the joint cartilage was destroyed, that it was friction that gave them the stiffness. Nobody could understand one of the real characteristics

of rheumatoid arthritis, which is called morning stiffness. People would wake up and their hands would be very stiff, and then as the day wore on their stiffness would decrease. Well, there is no reason that the friction should change, and that did not make sense.

We developed a method of measuring joint stiffness in which with a hand-holder, which is something that mechanically moves the finger so as to have passive movement of the finger sinusoidally. We would impose a sinusoidal rotation, and it would sense the force that was required to do that.

Nebeker: It sounds like a sophisticated device to be able to move and sense the force that is encountered.

Johns: Yes. The other thing which is important is the axis of rotation. It was a cantilever bar with strain gauges on it that measured the force, and then a sinusoidal rotational drive on the thing.

Nebeker: How was the finger being moved?

Johns: It had an aluminum angle that was taped to the finger, and it had a bearing that was hooked onto the cantilever, and you aligned it so that the axis of rotation was collinear with the end. The bottom line was that the stiffness of rheumatoid arthritis is a visco-elastic stiffness and non-frictional—there is no excess friction. It was this visco-elastic stiffness that increased in the morning. It was an actual swelling in the joint capsule that produced it.

Nebeker: That increased movement in the joint reduced.

Johns: Yes. As you begin to move a little bit, you milk some of that tissue fluid away. So, my first career was really in academic medicine and in internal medicine.

Nebeker: Were you teaching?

Johns: Yes. I was an attending physician on the wards. I taught and I lectured.

Then in 1965, two things happened. At that time, biomedical engineering was a division of the Department of Medicine, and Sam Talbot was the head of that. By that time I was the head of another division in the Department of Medicine. Sam Talbot decided that he was going to go to the University of Alabama to start a new biomedical engineering program there. So, the Medical School needed to decide what to do about biomedical engineering. The Advisory Board of the Medical Faculty, which is the executive part of the Medical School, appointed a committee to figure out what they wanted to do with Sam Talbot's departure. That committee was chaired by Vernon Mountcastle, who was the Chairman of Physiology. It had on this committee Mac Harvey, who was the Chairman of the Department of Medicine; and had Russ Morgan, who was the Chairman of the Department of Radiology.

Nebeker: Were they deciding whether to shut it down?

Johns: I suppose they could have, but their real interest was in how to expand it. Mac Harvey was convinced of its merit, because he started it when he became the Chairman of the Department. Vernon Mountcastle, in Physiology, used a lot of the instrumentation for his work in neuroscience. Russ Morgan was the person who introduced video amplification into clinical radiology.

Nebeker: They had the right people on this committee.

Johns: They were the beneficiaries of engineering science and technology in all of this. Another person who was in biomedical engineering was David Robinson. He was

an engineer who used physiological modeling for contraction of the heart, and then he went on to become interested in eye movement control as a physiological control system. He devoted the rest of his life to that, and he is one of the world leaders in that area. He is the one who thought that people had a real problem measuring what the eye was doing in the experimental realm. He was the one who thought of the idea of implanting a coil under the conjunctiva of the eye and sticking the monkey's head into three pairs of Helmholtz coils. By sensing the vector of the signal in the eye coil, you could tell with great precision where the monkey was looking in 3-D. You could measure what was going on in the nervous system; it was in the output of the plant that you could not get.

Nebeker: You could take a movie of the eye and try to reconstruct it.

Johns: That is exactly what people had done. The other was a corneal reflection—a reflection off the cornea to see what the eye did. None of those really had the precision of this. He went on to get a contact lens manufacturer to put a coil in a contact lens and so forth. So that was another thing that was going on at this time.

Nebeker: So, there was no question of ending the department division wise?

Johns: No. It was in growing the thing. To make a long story short, the committee decided that Hopkins Medical School really ought to get a department in place.

Nebeker: Was it a division while under Talbot?

Johns: It was a division in the Department of Medicine, and this was to make it a department in the Medical School. The first step was to make it a subdepartment of the Department of Medicine, which gives you everything except ownership of the space.

So that happened, and the advisory board said, “That sounds good.” So the same people were then appointed as a search committee to find somebody to do that.

At that time, I happened to have an offer to become the chairman of a department of medicine somewhere else. They asked me if I would be the Chairmen of this new venture. I thought about it, and all I could say was that this was something much more exciting to me.

Nebeker: Was it seen as a strength to have it as part of the School of Medicine?

Johns: That was not an issue here, because shortly before all this, the School of Engineering at Hopkins merged with the Faculty of Philosophy to become the Faculty of Arts and Sciences. So the Engineering School disappeared at about this same time. This was the heyday of engineering science. So at the same time that biomedical engineering was being established, the School of Engineering was being merged into the Faculty of Arts and Sciences.

Sam Talbot had recruited Moise Goldstein from MIT shortly before this time to run the Ph.D. program in biomedical engineering. Moise came and began this program. That was in 1963. One of the unbelievable facts is that this Ph.D. program has been constantly funded by the NIH since 1963 by the National Institute of General Medical Sciences. There were three programs that were all started in 1963: this one, the one at Penn, and the one at Rochester. This is the only one that has been constantly funded. It is more good luck than anything else, because it was funded for four-year chunks, and our timing was just right because there were times when Ph.D. programs were on their way out and we would still have two more years to go. Then by the time it was time for us to renew, they

were back in again. There were some excellent programs. For example, Case Western Reserve happened to hit it wrong. When it was time for them to be renewed, people were all against Ph.D. programs in biomedical engineering. So the Ph.D. program had already started, and it was already very good and had very good people in it. For example, one of the early students is now the Director of the Mind-Brain Institute here at Hopkins. You may meet Eric Young, who is the head of the Hearing Sciences Center. They are just terrific folks. So, that is how the Department started. It has had strong institutional support. With institutional support, we were able to get some substantial foundation grants to get things going.

Nebeker: Was it your intention from the beginning to turn this into a full department?

Johns: Yes.

Nebeker: It was seen as an interim...?

Johns: Yes, that's right, to get it started and have it become a full department. The other aspect of the enterprise, right from the beginning, was to serve as a liaison with the Applied Physics Laboratory at Johns Hopkins. The proximity fuze of World War II was their first claim to fame. Satellite navigation is one of second big things that they did.

So the Medical School offered this new enterprise to form a liaison with the Applied Physics Lab and bring their talents to bear on the solution of problems. At that time, Ned Gibson was the Director of the Applied Physics Lab, and the Head of the Research Center was Frank McClure. They were very enthusiastic about it. Frank McClure was one of the discoverers of satellite navigation. He

was the one who saw the navigational possibilities. APL was big on precision radar at that time, and that was when the Soviets launched Sputnik. Frank McClure told Bill Guier and George Wiffenbach, “Okay, if you guys are so good, can you track this Soviet satellite?” And they did. Bill Guier noticed the Doppler shift between when it was approaching and leaving. Bill Guier said, “Since I know its track and since I know its velocity, then I bet I can calculate its orbit.” And he did.

So they went to tell their boss, Frank McClure, about this great triumph, and Frank McClure said, “Well, since you know where you are and you know its track and its velocity, you can calculate its orbit. But now that you know its orbit, if you don’t know where you are, you can reverse the calculation.” So that was the beginning of the Transit satellite. One of the things was that APL put some of their best people on problems that we had here. Bill Guier was one of the people who worked on those.

Nebeker: So there was some instrumentation that they had done that could be applied?

Johns: Well, it worked the other way. Rather than a hammer in search of a nail, what we would do is that we would put together a whole bunch of biomedical problems that needed a solution. We would describe the nature of the problem in a half-page of plain English. Then we would go down to APL and we would distribute that fairly widely for anybody who had an idea as to how we could solve this. Problems that nobody had a clue as to what to do, we would throw that in the wastebasket. We would then have a meeting at APL with the people who owned

the problem with all of these APL folks, and a number of very good things came out of it.

To give you one example, Arnold Patz , who was the head of the Retina Clinic, was disturbed that using incandescent or ruby laser light to photocoagulate leaky blood vessels in diabetics was causing a destruction of the retina that was almost as bad as the hemorrhage. He was down there explaining what this problem was, and one of the physicists in the audience, Bill Liben said, “Wait a minute. You’re using incandescent light or a ruby laser to coagulate red blood vessels? That’s stupid. You should be using green light, a complementary color. You must just be cooking the black retina there and not the blood vessel. You need to use green light.” That was the beginning of the helium-argon laser for diabetic photocoagulation that was developed, and has really changed the outcome of the effect of diabetes on vision.

Another kind of example was at the time that Hopkins was one of the five places that had a myocardial infarction research unit. This was before the days of coronary care units. Indeed, almost everything that is done in coronary care units today grew from these five myocardial infarction research units. Bill Guier , the man who figured out the satellite orbit, went to work with the folks who were establishing the myocardial infarction research unit. One of the things they did was they developed the algorithms for the computer recognition of cardiac arrhythmias. That became the basis of Hewlett-Packard’s first arrhythmia detection monitor. There were a whole bunch of those kinds of things.

Nebeker: How big were the subdepartment and the fledgling departments?

Johns: Well it started off with five lead positions, and grew from there.

Nebeker: How many graduate students and post-docs would there be typically?

Johns: There were probably initially fifteen graduate students. It was good size. One other thing that we did was that we taught the Medical School curriculum too, in physiology. And we taught quantitative systems physiology to the medical students as a part of the regular physiology.

Nebeker: Were the graduate students you got typically M.D.s interested in going into research here, or people going for Ph.D.s?

Johns: No, they were mostly Ph.D.s, although there were some MDs. Let's see if we have a list. Here is a picture of the helium-argon laser.

Nebeker: I was wondering how different your program was here because it was in a medical school as compared to the standards in the other programs.

Johns: I think there were a couple of things that were different about the Ph.D. program. At that time, the only program at Hopkins that could award Ph.D. degrees was the Graduate Board of the Faculty of Philosophy. It did not make any difference where the program was, they were the degree-granting outfit. To this very day, a Ph.D. from Hopkins just says Ph.D.—it does not say “in” or anything else. From the very start, the Ph.D. program was an interdisciplinary Ph.D. program, and was run by a committee. That worked very well because the majority of the people who were on the Ph.D. committee were in the department, and the director was always in the department. When the budding School of Engineering came back, it had people from the Engineering Department on it as well, and had some people from Physiology on it and so forth, and their graduate students could work

with anybody—they did not have to work with somebody in the department. That did a lot of good things for attracting excellent students. This [showing something on paper] has a lot of the early graduate students. The current president of the Biomedical Society is Herb Voigt. He is one of the Department graduates. We have had a bunch of really terrific graduate students.

Nebeker: How long did you head the department?

Johns: It was from 1965 until 1991 when Murray Sachs was appointed as my successor. [tape cut—voice activated mode began at this point.] One of the good things about the Medical School is that when you reach age 65, you stop being the Director of a Department. The current incumbent meets with the committee for an hour. The Search Committee does that. It is very good in that the Search Committee assesses the Department and the institution. I was just delighted that they picked Murray, because by and large, they go outside.

One of the reasons that I think things went so well is that the Chairman of the Search Committee was the then Director of Radiology, Bill Brody, who is now the President of this University. Bill Brody received a BS and a MS in electrical engineering at MIT and then went to Stanford Medical School. Without giving his whole life history, one of the reasons that he wanted to come here to be the Director of Radiology was that he wanted his department to be associated with a Ph.D. program. At the same time, we were having a lot of graduate students who were interested in biomedical imaging and imaging science. We had interacted with them often. Russ Morgan had stepped down, and the then head of Radiology was really primarily interested in clinical research. With Bill Brody's advent, our

imaging science part of the program really expanded. So, Bill understood engineering; and number two, he understood the department and what it was doing, and he did an excellent job running the search.

Nebeker: What were the areas of strength of the Department in the years that you were there?

Johns: One of the strengths at all times was quantitative systems physiology or biology, in the sense of really understanding how things work in a quantitative basis. For example, Kiichi Sagawa was in the cardiovascular area, and he really raised a lot of controversy about how to categorize and assess myocardial contractility. He figured out *the* way to assess myocardial contractility, which contributed to the fundamental understanding. But, it also changed the practice of clinical cardiology in allowing clinicians to figure out in somebody whose heart was not pumping right, whether it was poor contractility of the muscle, or improper filling of the heart, or too big a load that it was pumping into. Any one of the three can make the patient look the same. The trick is how to figure out which of these things it is, because you treat them differently. So that is one example of the kinds of things that were done in a number of areas.

Another big area of understanding was in hearing science, in which Moise Goldstein was the first person in that area. Murray Sachs was one of his post-docs. Murray Sachs' successor, Eric Young, was a graduate student who went away and then came back.

Then we have Nitish Thakor who was interested in clinical instrumentation. The faculty saw that this was something that really was important, and we didn't have

anybody who was doing that. John Webster was the guru of instrumentation, and Nitish was his brightest graduate student that area.

I think the thing to do is when you talk to Murray, if you can talk about exactly what is going on.

Nebeker: Developing instrumentation to better understand a physiological system, there seems to be a science.

Johns: Well, what you say is true. Nitish did straightforward, innovative electrical engineering signal processing to solve a problem. The first thing that Nitish did that received real recognition was problem about how neurosurgeons, when they're working around close to where you live, stimulate your toe at once per second and record from the cortex the evoked potential. They do it to make sure that the messages get through and that they aren't screwing things up. Now the problem is that you can only stimulate the toe at about one per second, and the bad news is that the signal-to-noise ratio is such that it takes about four minutes of averaging to see that the potential hasn't changed. Putting it another way, you get the bad news that four minutes ago you were really screwing things up.

Nitish said that averaging isn't the only way of signal recognition and getting signals out of the noise. What he did was that he used template matching. What he said is that before you begin messing with the person, and you get a template of what that potential looks like. From then on, you do not do averaging, you do template matching. When the signal departs from its template, you had better quit doing whatever it is that you are doing and let it get back to where it was. So, that is the way things are done these days.

Nebeker: I know it is impossible to draw a line.

Johns: You are absolutely right. One of the other parts of it is that a lot of the straightforward engineering comes from the Applied Physics Lab.

Nebeker: So there continues to be a close connection?

Johns: Yes. It continues. The person who was talking to me when you came in is the person who is the head of that down at the Applied Physics Lab.

Another aspect of the whole thing is clinical information systems had its home in this department. The first person was Don Simborg, who is I believe the only multi-millionaire of our faculty. He developed a ward information management system here. The Chief Financial Officer of the hospital didn't understand that it might be worth spending a dollar to save a hundred—which is what it did. It improved the quality of care and reduced costs. But it cost money, and he was opposed to that.

Nebeker: Tell me a little bit about the early programs of this sort and if there were huge problems with them and doctors were often opposed to them.

Johns: Don Simborg was an M.D. and the doctors and the nurses loved him. He understood what folks wanted. It was hospital administration that was the problem. Don said, "Screw this," and went to UCSF where the hospital director actually recruited him. One of the things that he did was he developed an intranet before anybody had a word for intranet. He got the incompatible hospital computers talking to each other and so forth. Don Simborg then started a company that did this in Silicon Valley and built that thing up. He sold it to Bell Atlantic for many millions. He now has some more companies going.

Another one of the people was Bruce Blum, who was from the Applied Physics Lab, and actually came up here. He developed the oncology information system that is a state-of-the-art so called “smart system.” If you are a patient over in oncology and you run a fever, it comes up with a list of things that you had better consider, such as is the guy’s white count low, does he have an infection, is it drug sensitivity?

Nebeker: Has this been widely adopted?

Johns: It has in cancer centers.

Nebeker: One things that we are looking at is the history of the IEEE Biomedical Engineering Society itself, and we are interested in other professional organizations. I see that you were associated with IEEE and you have also been president of the Biomedical Engineering Society.

Johns: Yes. And I was the national President of the IEEE Group, too. It was then called the Group on Engineering in Medicine and Biology, and in 1973 and 1974 I was the chairman and president of that.

Nebeker: I take it that you found its publications and activities interesting?

Johns: Yes, and that is where I first knew Thelma Estrin. She also was president.

Nebeker: Of the IEEE EMB Society.

Johns: The IEEE Society on Engineering in Medicine and Biology were primarily the electrical engineers in biomedical engineering. They were a fairly diverse group. They weren’t all circuit design for biomedicine people, and there were many the Dave Robinson types (the guy who was studying control of eye movement who

was an electrical engineer). They didn't think much of chemical engineers and the kind of stuff that they did. I was okay, coming from medicine.

Nebeker: So an M.D. had no trouble.

Johns: Yes. But a chemical engineer, they didn't quite see what they were doing in that. Then the Biomedical Engineering Society was primarily people in the systems physiology business. They couldn't understand what these people who made these keen devices were all about. I think it's sort of stupid. But that was the difference in focus.

Nebeker: But there is also the Instrumentation Society of America.

Johns: Yes. I was once belonged to that.

Nebeker: It doesn't treat your area very well?

Johns: No. I had patented some instruments, but I did not find that that was very much in the biomedical thing. I mean there is lots of instrumentation stuff. But the IEEE and the BMES have had two different views. I think their center of gravity are in two different places.

Nebeker: Since you have on a Society board, how is the IEEE doing?

Johns: I think they have done a good job.

Nebeker: It is such a confusing picture with the sciences study in so many different areas of this field.

Johns: I think one of the redeeming features is that they have good meetings, and they are well-attended.

Another part of this is epitomized by Kam Leong, who is just terrific in biomaterials at Johns Hopkins. He is active in the Biomaterials Society and does

not have much interested in BMES or in IEEE. He is a different kind of a guy. He is in polymer science, and his thing is in making bioerodable polymers. When Kam was at MIT, he got together with Henry Brem, a neurosurgeon here who was then at Mass General, and Bob Langer, and they made some little wafers that contained a cancer chemotherapy product. In neurosurgery, glioblastoma is a bad brain tumor, because when you take out everything that you can see—and you obviously do not want to take out any more brain than you have to—you always leave some tumor behind. Doing that kind of therapy didn't really do it. Chemotherapy is bad because some of the best drugs do not get through the blood-brain barrier. So they had the idea to put it on a bioerodable wafer, and when you take the tumor out put these wafers in there, and these wafers will release over time the chemotherapeutic stuff, and the blood-brain barrier works in your favor. It does not get to your bone marrow and into your GI tract. It has really been a big boon to treatment. So that is who he is, and you can see that his professional interests are different. He is interested in what the other tissue engineers are doing.

Nebeker: Was Hopkins important in getting biomedical engineering going?

Johns: It is important in at least two ways. One of the things that is unique about Hopkins is that people are willing to quit doing what they are doing and help you do what you want to do., Instead of saying, "I'm busy. I really don't have time to talk to you about that." One of the really good things is that these people are willing to stop and talk to you and tell you what is wrong with what you are doing, or tell you what some opportunities are that you might do. If there is an

intersection of their interests and your interests, they are willing to move ahead on it.

We were talking about myocardial contractility and how to measure it. Some of the myocardial contractility people and the MRI people and the image processing people came together, and what you can do now is with magnetic resonance saturation, in essence, put a whole bunch of planes through the heart, and then watch those lines, those planes, move as the heart beats. They said that we could see if there are some areas of the myocardium that are not contracting. Then the imaging folks figured out how to create a pseudo-color display, such that the less it contracts the bluer it is, and so forth. Then they get the cardiologist in on it doing thallium scans to see if that is where the thallium uptake is wrong. It does make a good environment for doing collaborative stuff.

There are a number of places where people are helpful, and there are a number of academic institutions where people are not helpful and are somewhat paranoid in thinking that somebody is trying to steal their ideas if you talk to them.

Nebeker: I get feeling that [tape cut] the Hopkins program and so on, it didn't mention some of the things that you have worked on over the years.

Johns: Yes. One of the other things is that I had the idea with Dick Sheppard (who was in pulmonary physiology and was also the head of the Computer Center here in the Medical School for a number years) that maybe it would be possible to measure the level of carbon dioxide through the skin. The best way to check pulmonary ventilation is to measure the arterial  $PCO_2$ , but that means that you have to stick a needle in the artery. So we wondered if we could measure it

through the skin and sense it with a CO<sub>2</sub> electrode. The thing that we discovered was that the reason that you are not leaking CO<sub>2</sub> through your skin and also the reason that you are not leaking water through your skin and drying up like a raisin is the so-called barrier layer that is made of up dead keratin cells (dead skin). If you stick and remove scotch tape on the skin about twenty times it will come up clean because scotch tape will not stick to something that is wet. If you keep putting scotch tape on the skin until it comes off clean, the reason it comes off clean is because you are right on living skin cells. If you put a CO<sub>2</sub> sensor over that window in your skin, you will track tissue CO<sub>2</sub>.

Nebeker: Does that closely track the blood CO<sub>2</sub>?

Johns: Unless you are in shock, number one, and have shut down the perfusion to the skin in which the CO<sub>2</sub> goes up. The other exception is if you are very cold and vasoconstricted. If you get a high reading, something is wrong. The patient may be cold, or may be in shock, or may not be breathing enough.

The interesting thing about that was that it took many years before anybody believed that. It was not until they started measuring transcutaneous oxygen in neonatal intensive care units that anybody believed that this thing would work.

Nebeker: That is interesting. Is that something that bothered you at the time?

Johns: Yes, it bothered us a lot. Because we were trying to get somebody to make and market it because it is a good idea.

Nebeker: Medical manufacturers weren't interested?

Johns: We found that that the medical manufacturers would do is that they would go ask an individual, and that individual would say, "I don't think that would work." So

they would come back and say, “We don’t think that will work.” We would say, “See, here is the transcutaneous PCO<sub>2</sub> and here is the arterial PCO<sub>2</sub>.” They would say, “Well, this guy says it won’t work.” Then a number of folks starting making them, but they neglected to obtain a license. So this was the first time that Hopkins ever sued patent infringers.

Nebeker: So you and Sheppard had gotten a patent?

Johns: Yes, it was patented. We had a patent.

Nebeker: Did Hopkins hold the patent?

Johns: Hopkins held the patent. We were the inventors. Actually, we finally did license it to a company, and they were making them in Germany—they were making and marketing them in Europe because they didn’t want the hassle of FDA approval and all that stuff. A part of their license agreement was that they were supposed to defend against infringers. So we found these infringers, and we told that company that they had better go after them. The infringers said that the patent was invalid, and the licensee decided that maybe they are right, so they abandoned the license and joined the infringers, which really ticked me off. Then I told our General Counsel that they ought to go sue these folks. They did not want to sue them, so I said, “Okay. If you are not going to sue them, you issue the patent back to me and I will go sue them.” I wasn’t about to go sue them. So they said, “Well, maybe we will.” So they did, and they won.

Nebeker: That’s a good end to the story.

Johns: Well, not really, because the seventeen years was about out and the patent attorneys got most of the money.

Then I was personally interested in the clinical information system business and was involved with those. In one piece of that, Don Simborg and I were interested in seeing about using information to save costs. It turns out that the biggest wastes of money in terms of laboratory and ancillary services are not the big-ticket items, but are often the very routine items. At that time, the most expensive thing was CT scans, and the cheapest was x-ray. If you could reduce your chest x-rays by ten percent you would save more money than if you eliminated CT scans entirely because of the tremendous volume. We tested to see how hard would it be to save ten percent in that. It turns out I think three percent of all the x-rays were taken of the same person on the same day.

Nebeker: Two different doctors had requested?

Johns: The commonest cause was when a person was getting admitted to the hospital, they say, "Well, let's get a chest x-ray on the way up and that will save an extra trip." Then the next person up on the unit would not know that they had done that, and wouldn't ask, and would do it again, and so forth.

Nebeker: That is three percent right there.

Johns: Yes. Some of them were absolutely justified. They drained some fluid out of somebody's lung and wanted an "after" film. So that was another thing.

Nebeker: Is there anything that you wanted to comment on that we haven't covered?

Johns: No.

Nebeker: Thank you very much.