Interviewee: Jozef Zwislocki By: Dr. Jont B. Allen

Place: in Car Date: December 8, 2008

ZWISLOCKI: *(Answering Dr. Allen's question on his personal history)*
I was born in 1922 in the Polish city of Lwow, which, for centuries served as a bastion of West-European culture toward the east (at the end of WWII, Lwow was gifted by Stalin to Ucraine and renamed Lwiw. My father was a Dr. of physical chemistry and built the key Polish Nitro-chemical plant shortly after World War I. My grandfather was also a physical chemist. Working in Switzerland, he invented an electro-chemical method of obtaining Nitrogen from air and to use it for manufacture of fertilizers and other Nitrogen containing products. Still before WWI, He was elected Professor of the Lwow Polytechnic, subsequently also of Warsaw Polytechnic. He rebuild the Polish chemical industry destroyed during the war and was elected President of Poland twice, so that he kept this office until WWII. When the Hitler Forces and Stalin forces occupied Poland, he and his closest family escaped to Rumania, then traveled to Switzerland, where my grandfather had a dual citizenship. We were able to stay there during the war.

ALLEN: What was your grandfather's name?

ZWISLOCKI: My grandfather's last name was Moscicki, and his first name was Ignatius. In Switzerland, I was able to attend the Federal Technical Institute, called in German "Eidgenossische Technische Hochschule", for short: ETH.

ALLEN: I've been there several times. I gave a talk there.

ZWISLOCKI: What was the talk on?

ALLEN: There's a Department of Information Theory, and in that department. George Moschwitz, who worked at Bell Labs for many years, and then he retired from Bell Labs and worked at ETH as a professor. He was Swiss and worked in the US for many years, but then went back to his mother country. He was in the Electrical Engineering Department where they taught information theory. He worked on hearing aids.

ZWISLOCKI: There was a full Communication Department that was mainly electronic engineering when I was there, and these things that we are talking about are newer, they didn't exist when I was there and I studied Electrical Engineering. There were some interesting things that happened then. I developed an RC oscillator with only one vacuum tube (we were working with vacuum tubes then) in the range of audible sound frequencies but could also be used for higher frequencies. Ordinarily, such oscillators required two tubes. It was able to generate various waveforms, in particular square waves and narrow impulses. In a different course, I was working with high frequencies, radio frequencies, producing wavelength on the order of 10 meters.

ALLEN: 30 megahertz?

ZWISLOCKI: Yes, like that.

ALLEN: So this was basically like a transmitter, then.

ZWISLOCKI: Yes, for my master thesis, I had to design together with a colleague a high-frequency communication system (about 10 meters wavelength) resistant to electromagnetic noise. The system was based on the high-frequency carrier modulated by narrow, rate-modulated impulses. I learned later from Philips Company in Netherlands that US Navy developed and patented a similar system and that my theses preceded their patent and would have invalidated it were it regarded as a publication, but it wasn't.

<u>Early Years:</u> *(Looking at pictures in the living room.)*

ZWISLOCKI: There's a picture of my wife.

ALLEN: I believe I've met your wife. I don't remember exactly where, maybe she came to an Acoustical Society meeting?

ZWISLOCKI: Yes, the same one.

ALLEN: That's right, when you got that nice award at AAS (Bekesy Medal). *(Looking at the medal.)*

ZWISLOCKI: I don't know if you saw this one. *(Looking at Joe's book.)*

ALLEN: You have two books, right? Is this the new one?

ZWISLOCKI: This is the only one.

ALLEN: 2002, yes I have this. So we have quite a few topics to talk about. I have some notes here I want to look at.

ZWISLOCKI: I have lived already a pretty long life, and I started early. My professional career, so to speak, I finished the ETH in the fall of 1944.

ALLEN: You're talking about ETH?

ZWISLOCKI: ETH, yes, ETH in German. And then I immediately got a position as a research assistant and head of the Electro-Acoustic Laboratory in the Department of Otolaryngology at the University of Basle, Switzerland.

ALLEN: That's a big department there, isn't it?

ZWISLOCKI: That was an important department because it had a very important chairman, Professor Erhardt Luescher. You see, the system in Switzerland was that the department heads, usually the professors were at the same time department heads. The rest was physicians, associate director, all that. But the director that was the professor and was director, so he was a little dictator and everything went the way he wanted when I went there to work. Of course this somewhat German system, because Germany had the same thing, was maintained—that means he was the director. That's why many of the publications we had, his name is first, although all he did is ask the question, "Could we do this and this," and the rest I did, but he wrote it, you see (*knowing smile*).

When I first came to Switzerland, my family settled in the French part of Switzerland, but the best engineering school was in Zurich so I went there. But my German was kind of poor—you know, kind of high school German that you use in a foreign country. So the first year was a real struggle for me. They had some courses in French, so I could take that, but it was a problem. But I was very lucky. I had to pass an exam to be accepted, and I passed it but barely. Then I was a student there in electrical engineering.

The first year it had no difference; all the engineers were taking the same things. You know, in those days, the knowledge was much smaller than today—even that span of time made a tremendous difference. So it was possible to do not only electronics but I had a very strong background in mechanics, and I took every course in physics and mathematics that there was. So the first year I struggled, and I also was very tired from the war and studying to pass this entrance exam. So there were two things. First I had linguistic difficulties; second, I didn't feel too much like working.

ALLEN: What year are we talking?

ZWISLOCKI: '40-'41. That was the academic year. And I also discovered girls [chuckles], so I had dates and all that. So there was an exam at the end of the first year. The purpose of this exam was to eliminate the worst students. So it was a pretty big year. And a minimum requirement on a scale of 0 to 6 was 4, and I passed this exam at 4.2, but I passed it. Now after the second year there was an exam again, and that one I passed quite well. Then at the end of the fourth year was a kind of final exam. You had to construct an electronic device on a bread board; not the housing and all that.

ALLEN: This is when you created the oscillator?

ZWISLOCKI: No, I created the oscillator before. I think, the first semester of the last year. But my advisor, who was at that time also Rector of ETH—the chancellor, but they didn't call him that—Professor Franz Tank, he was in high frequency. And so I passed the exam in his subject, which for me was the main subject, very well. And then my master's thesis was sufficiently innovative. It was an interesting modulation system which was impulsive, which was rate modulated, and the purpose of that was to decrease interfering noise. So that's why it was good for transmission over the ocean or long distance.

ALLEN: This was in the one to ten meter range.

ZWISLOCKI: Yes. My advisor suggested that I publish that in a Swiss journal…

ALLEN: This is Tank who suggested that.

ZWISLOCKI: Yes. Called *High Frequency Journal*, something like that in English. So I published it, and somebody at the Phillips company went home with it and read it, and I get a letter from Phillips: if I had a patent; and if not, if my master's thesis was regarded as a publication. Unfortunately it wasn't. I didn't have a patent and it was not regarded as a publication.

ALLEN: So they could steal your patent.

ZWISLOCKI: Well, it was not that good for them. I wrote to them that no I didn't have these things, and why were they interested in it. So they wrote to me that in the United States there was a Navy patent, and it came after I had written my thesis, and so if my thesis counted as a publication, the patent would have been invalid, which Phillips very much hoped for. But it wasn't, so they came short. Anyway, that was very interesting for me to learn about these things.

ALLEN: An eye-opener for you?

ZWISLOCKI: Yes. And I must say that during my studies in electrical engineering, I got a little bored with the subject. I developed interest in living organisms, in living systems, and this was of course inert, so I lost interest, and I was looking for a position in what today we would call biophysics or bioengineering. It so happened that a friend of mine, also a very good student, was consulting for some company in Basle, and during that trip he got a very bad cold, so bad that he had to go to the hospital, and the professor, Professor Luescher, you may know the name because of some publications.

ALLEN: Luescher was the one who got sick?

ZWISLOCKI: No, Luescher was the professor who treated my friend who was a student the same year that I was but he was in mechanical engineering. So anyway, when this professor was treating him, he asked him if he would be interested to become his research assistant. He had a good notion of physics, this professor, and it was very interesting research. So he needed an assistant, though, because as you know, medical people are terribly busy and they don't really have time for research. What happens is the assistant or some nurses section the preparations and they go under the microscope, and the physician looks at these things in the microscope, and that is his contribution. This is a little nasty, but that's what they say [chuckle].

ALLEN: Not much different today, actually.

ZWISLOCKI: No, I think it hasn't changed. They're still very busy, and now I suppose more busy than ever. So anyway, my friend was not interested in the offer. He wanted to become a good classical mechanical engineer, and actually, after he finished he became professor of mechanical engineering in Canada at Laval. He had a French culture, so he was looking for such a position.

ALLEN: What was his name?

ZWISLOCKI: Talic Tordion.

ALLEN: So you got the job.

ZWISLOCKI: So he came back and asked me if I would be interested, and if I was somewhat, to let this professor know, and he would make the offer. He asked me to come to Basle for an interview. So I came, and it went very well. We liked each other personally also. One thing that we became known for is that we were both very critical.

ALLEN: Well, you wrote a lot of papers with him.

ZWISLOCKI: Ten.

ALLEN: I was looking at your bibliography, and it must have been more than ten.

ZWISLOCKI: I don't think so. Almost, but I don't think so. Anyway, interesting things happen, very key kind of events took place there. So I came against the advice of my ETH advisor, who wanted me to make an engineering career, and actually I had an offer from Braun Bovery, they are the Swiss GE, General Electric. They make electronics, they make turbines—everything. So I got an offer from them, but during the interview I told them, to be fair, I already had a position, and I was interested in bio engineering.

ALLEN: And it sounded more researchy, more to your style, your interests.

ZWISLOCKI: It was in hearing. I was not particularly interested in hearing in those days. I would have preferred to work on the brain, but that was an opportunity I had, so I took the position. My task was to develop new diagnostic methods. It was a sensory facet of physics, only then I did not know about this was a facet of physics. So I built a little laboratory, a kind of anechoic chamber.

ALLEN: What year are we at now?

ZWISLOCKI: My tenure at Basil was until 1951, so it was '45 to 1951. In 1951 I came to this country.

ALLEN: So you had a booth?

ZWISLOCKI: No, in those days we didn't have booths.

ALLEN: So you made some kind of a chamber.

ZWISLOCKI: I made a chamber, yes. This year I was interested in transient effects, you know, when you turn off a sound, what happens then from the point of your hearing. The first thing I did was to find out how long a sound, pure tone, had to be for its pitch to be identified and for it to sound like a pure tone. That was first thing published in a Swiss journal, and I was amazed that at low frequencies you needed only one cycle to estimate the frequency with good accuracy.

ALLEN: Had you read Stephens' 38 book?

ZWISLOCKI: Yes. This and Fletcher were the first two books in English that I struggled through with a dictionary and all that. Absolutely. Fletcher's *Speech and Hearing* may have been the first book I ever read in English on hearing. I had a lot to do with Fletcher. You may not know. Not for long durations of time, but he played a key role in my life, as in many other lives, I'm sure. I did this, it was published. Then the next thing I did was to work on the recovery of hearing after a tone was turned off (we called the threshold elevation we found *adaptation*).

ALLEN: Forward masking.

ZWISLOCKI: Now it is called forward masking. It is very interesting. I did all these things then. There was another publication later on in 1980-something, an afterthought, I don't remember when it was. Oh, it was '59, I have here in notes: forward masking, 1959.

ALLEN: Are you saying in 1947 you were working on forward masking?

ZWISLOCKI: Yes, and publishing and the last one, my last publication on adaptation, was in '59. And I discovered some interesting things. I didn't keep up with the recent literature on forward masking. It became annoying for me, because what people did, and I'm very sincere about these things, some people here discovered some of my work on what has become called forward masking, and what they did was to refer to me in the first publication, but then refer to their publications thereafter, so I didn't not appear. But the other thing is that when I did this original job, which was in '47 (actually before, but the publication took time, so the publication was in '47), so they did that, which I didn't…

ALLEN: Who are you talking about, Zwicker?

ZWISLOCKI: No, Zwicker never did things like that. He didn't work on it. Brian Moore was probably the most important one to get hold of it, and he referred to me in his first publication, thus he referred to it later on. I think David Nelson at Minneapolis did a lot of work on it clinically. When I did that, people now were also interested in clinical application of this phenomenon. I did all kinds of things with it, and we decided that it wasn't worth it. To extract any pathological information, you had to do too much work clinically, it was not acceptable.

ALLEN: It wasn't practical.

ZWISLOCKI: Yes, because you had to integrate the response area.

ALLEN: Steinberg and Gardner had a paper on forward masking.

ZWISLOCKI: Correct. We had a very interesting discussion on this, because we did our work —I mean I did; I mean Luescher came in and the first publication is with him. That was '47, and that's why it is important to you. Steinberg and Gardner said in their publication that they did the work two years before ('50) but didn't think it was important enough to publish, so they did not publish it until '52.

ALLEN: That work was the recruitment, yes? *(Allen confused two papers, as described below).*

ZWISLOCKI: We were hunting for the same thing. We hoped to be able to discover loudness recruitment through what is now called forward masking. They didn't do anything new from what we did. They did some experiments better.

ALLEN: There was Munson and Gardner, and then there is Steinberg and Gardner. When I said Steinberg and Gardner I was thinking actually of Munson and Gardner, which came later at 1950. But then there was the Steinberg and Gardner where they discovered recruitment, but they didn't get credit for it because the guy from Columbia University did it first. (I'm thinking of Prince Fowler (MD), who worked with both Fletcher and Wegel in c. 1921).

ZWISLOCKI: Yes, a medical man.

ALLEN: The lab was named after him or his son for the Tonndorf work. It will come to me. Yes, Fowler.

ZWISLOCKI: Very famous otologist. I met him in Europe; he came to a meeting. Fowler, yes.

ALLEN: There were two Fowlers.

ZWISLOCKI: Father and son, so the younger Fowler, the son.

ALLEN: For the record, I'm going to tell this story. Mark Gardner told me that Fletcher *(JBA: I recall it was actually Wegel not Fletcher)* talked to Fowler (the father)—they were friends—and Fletcher *(JBA: i.e., Wegel)* told Fowler about the Steinberg and Gardner work on masking between two ears with a bad ear and a good ear, regarding loudness growth, and then Fowler went out and repeated the same work, and got it published, where he called it *recruitment.* Mark Gardner was *very*, *very* unhappy about this internal leak. Finally Gardner and Steinberg published their results in '37. But Mark said that was really their discovery; Mark was quite irritated both with Fletcher *(i.e., Wegel)* and also with Fowler. I had a long discussion with Mark on the phone about all this. He was very clear about his views.

ZWISLOCKI: Unfortunately this happens all the time. I had a long discussion at Harvard with S. S. Stevens, Smithy Stevens, on the question of references, and discovered that the attitude towards this in the United States was very different than in Europe.

ALLEN: Lax, I assume. They weren't as rigorous about it?

ZWISLOCKI: What Stevens told me is there is a different philosophy. In Europe we thought what you had to do was to site all the relevant literature that preceded your publication. There, people get annoyed when they are not referred to in general—[chuckles] human characteristic—and from the point about history it's fair because the history gets very muddled when you don't do that. But Stevens told me oh no, this is not what it's for. You have a publication and you don't want to get lost in all the details if somebody else already did it, and so you refer to that person, instead of writing about it you refer to that person to fill in the information. So if you found that somebody who published before you had information that was useful as an add-on to your publication, then you cited him. If not, forget it.

ALLEN: So Smitty Stevens was from Europe, so…

ZWISLOCKI: No, he was not.

ALLEN: He was a Mormon. So he was American born?

ZWISLOCKI: Oh, he was very American. He took a big trip with his wife before she became his wife in Europe, because Mormons, as you know, a young Mormon had to be missionaries and they had to go someplace in the world and preach Mormonism. I had a very interesting experience with Mormons on the boat when I was coming to America.

ALLEN: Let's not talk about that; it's too far off. It's interesting, but not really relevant here.

ZWISLOCKI: It is interesting from this point of view; that I was kind of brought to America by two Mormons; the one was Fletcher and the other was Smitty Stevens.

ALLEN: Many of the auditory scientists at Bell Labs were Mormons.

ZWISLOCKI: That may be because Fletcher was. So I was of course in Salt Lake City, and it was very interesting, the choir was absolutely wonderful as well.

So anyway, we had this forward masking, and at first except for these few people at Bell Telephone, nobody was interested in it. It just died. That was not necessarily tied to the equipment, but it is true that we were trying to use forward masking as an indirect means to discover loudness recruitment. So I think about 20 years ago, forward masking froze up. After a long pause, everybody forgot that it existed, but somehow some people, like Brian Moore, discovered my papers. I don't know whether that gave him the idea or they had it on their own and they started to look through literature and they discovered some of the things that I wrote. Anyway, I think that still is one of the preferred subjects of research inside acoustics. But that was my second job there.

And then becomes one of the most interesting chapters, I think, is Bekesy's publication (I don't think I have to explain who Bekesy was) of his automatic audiometer in 1947. I remember that date well. So this appeared in *Acta Otolaryngologica* I think, and Luescher got hold of it. I think he was receiving the *Acta Otolaryngologica Journal* automatically, so he found this article and grabbed it and ran literally, to my laboratory, showed it to me, and asked me can you do better?

ALLEN: This is the tracking method, the tracking audiometer.

ZWISLOCKI: The tracking audiometer, yes. And I was 25 years old, so of course I said yes [laughter]. And indeed, I had an idea to use the differential sensitivity for intensity to detect loudness recruitment. I did some experiments and found something very interesting that then was not completely comprehended, that the differential sensitivity was the same in recruiting ears as in normal ears provided the loudness was the same. Not sound intensity, but loudness. There was immediately a reaction in this country by Ira Hirsh [[http://news.wustl.edu/news/Pages/20100.aspx]] especially, and a visiting professor with him, however I don't remember the first name, from Finland that this was all nonsense, that we didn't do it right, and that there was a disconnect there, that it couldn't do that. And that started a big polemic. And the important thing, as I look at it now, is that there was something that was not dependent on sound intensity, the stimulus, but it was dependent on loudness, the response. Even some people called it constant loudness, constant JND hypothesis. There was no hypothesis.

ALLEN: Let me inject something. It seems to me that in some ways—I mean I agree that what your saying has to be the case, that the loudness JND is a measure of the internal noise. This goes back to…

ZWISLOCKI: Absolutely.

ALLEN: So in some sense, you're discovery is almost a given, if you think about it a little bit, that the internal noise determines the JND, and so the internal noise is going to depend on the loudness, so how could it possibly depend on the physical intensity when the loudness function is changed? So I'm finding what you're saying very, very reasonable.

ZWISLOCKI: You are at odds with quite a few psychophysicists. You are not really born a psychophysicist. *(JBA: Joe is reminding me that I'm not main-stream, and I agree.)*

ALLEN: Delta I over I, is not in general, a constant. Take pure tones, which are the simplest case. There is the *near miss*.

ZWISLOCKI: Delta I over I, in pure tone is not really constant. *(JBA: Joe agrees with what I am saying.)* But you are off the way you talk about the internal noise. Have you ever talked to Viemister about it?

ALLEN: I try. He's so opinionated it's hard to get in a word edgewise. But he is fun!

ZWISLOCKI: He does not believe that internal noise is coupled to loudness. It's for a certain level, and if you change the intensity of the stimulus and the loudness changes, it does not change the noise. In order for the JND to be independent of intensity and also the slope of the loudness function, you have to have a noise that follows the same intensity function that loudness. I have some publications on this also. And that is necessary. In the lower limits in zoology [???], there is nothing surprising, as you say, about it. It starts with a Poisson process, the way the neurons fire, and then you have the central value theorem, which puts many fibers together and makes it a Gaussian noise distribution, which legitimizes to some extent the theory of signal detectability, as applied to psychophysics by Spike Tanner that was publicized later by David Green. This was not understood. The first person who told me that of course noise decides whether you hear an increment or not was Smitti Stevens, actually, a long time ago. And he was the only one in this belief. The psychophysicists did not…

ALLEN: Say that one more time: what was his (Stevens') belief at that time?

ZWISLOCKI: He believed that noise is determined in the ear, just like you believe.

ALLEN: You mean, the internal noise.

ZWISLOCKI: Yes. Just like you believe.

ALLEN: When you use a pure tone, there is no stochastic component to the stimulus.

ZWISLOCKI: This is how the nervous system works.

ALLEN: There's no external component. The only noise that is possible is the internal noise.

ZWISLOCKI: Absolutely.

ALLEN: If you use a narrow band of noise, then it's a different story.

ZWISLOCKI: People don't seem to understand. Some psychophysicists, I gather, that if it were a pure tone, a really pure tone, it's a straight line without any bumps, that you can detect an infinitesimal change, and in order not to be that way you have to have noise.

ALLEN: Internal noise, yeah. Well I think Fletcher understood this.

ZWISLOCKI: Perhaps. I don't recall especially reading in Fletcher about that.

ALLEN: In the '20s, he calculated the intensities JNDs and he counted the total number of possible tones there could be, Delta I over I, and integrated that over frequency and level. He has a published paper on this (Allen, 1996, JASA **99** (4) pp 1825—1839; i.e. J. Franklin Inst., 1923 126(3), 289–326). He counted the number of JNDs and found like there was over a thousand possible JNDs over the entire range of frequency and intensity. He was very aware of this. He went back and understood Fechner…

ZWISLOCKI: Yeah, you cannot integrate JNDs with loudness.

ALLEN: Fechner said, let's count JND, pulse intensity, and…

ZWISLOCKI: That's what Fechner said, but we may disagree on that. Fechner's logarithmic function is not—and that's what you get if you integrate the JND.

ALLEN: **I agree that what Fechner did with this formula was wrong.** He assumed that Delta I over I is a constant and that Delta I is a constant. Both are wrong. However counting JNDs is a good idea. If you assume that Delta L is a constant, then you get Fechner's law. But neither of these seem to be true. (Allen and Neely, *J. Acoust. Soc. Am.,* **102** (6) pp 3628—3646).

ZWISLOCKI: Well, if you have noise, your stimulus is a narrow band noise, let's say, then it is true. Delta I over I is set.

ALLEN: Yes, like it is 1.

ZWISLOCKI: But if you have pure tone, then it isn't.

ALLEN: Yes, then you find the near miss to Weber's Law, as described by Riesz (1928, 1933).

ZWISLOCKI: Right. The reason for that, I think, is that the internal noise does not follow exactly the same function as the mean follows.

ALLEN: Isn't it Poisson, like you said?

ZWISLOCKI: No, it's really Gaussian because you have many neurons together, so each neuron is Poisson, but if you put the whole thing together so the central body theory tells you that you get the Gaussian noise.

ALLEN: So the variance is some function of the loudness function.

ZWISLOCKI: Absolutely.

ALLEN: So we agree on that [Laughter].

ZWISLOCKI: I'm sure we will discover we agree on a lot of things. One thing we agree on, I don't know if you still think that way on the cochlea, it seems that we are the only ones that see things the way you thought when we had our little discussion. I thought in that way, generally people don't believe that the entire tectorial membrane moves and has a non-negligible mass.

ALLEN: Things are changing. More people are starting to agree that the tectorial membrane is critical.

ZWISLOCKI: It isn't because of all the paper in the proceedings of the National Academy of Sciences by Friedman, I think, from MIT and several other people who discovered to their great encouragement that when they took the tectorial membrane out and to suspend it between two supports they could produce a radial wave on it.

ALLEN: Longitudinal wave.

ZWISLOCKI: No, that was a radial wave, a longitudinally propagated radial wave. They stimulated it that way. The discussion ends that they discovered a completely new theory of cochlear function. So I tried to write a letter to the editor of the Academy, which was rejected.

ALLEN: But you're a member of the Academy; how can they reject it?

ZWISLOCKI: They have new policies. It has become like any other journal. I don't publish in it anymore. I am so fed up with the review process in most journals, with Acoustical Society being one of them, that I stopped publishing articles. I'd rather write books now. You know, I may be arrogant, but I at least think some two-bit scientist judging my work and deciding whether it should be published or not—and that's what the system does now. Actually the most evident is when you apply for a project to NIH is goes first through a preliminary review triage, and no one of the first rate scientists wants to be involved in this because there is a huge number of papers, and many are worth little and are rejected. So what you have is a second kind of tier of scientist who reviews these things before they go to the better review process. So what happens, I was counting how many times you have to apply to get it accepted, and from people around I found that, and not only this university; you have to apply three times. First time you get rejected. Second time the reviewers put their two cents in, it's a little improved, and they say, "Well, you still have to improve this and this." And for the third time, you have a chance to be accepted. But the third time it's no longer your project; it's theirs.

ALLEN: There's not enough money, that's the problem.

ZWISLOCKI: That's for sure. But the process is also…

ALLEN: I think we should not talk about this because this is true for everybody. I could have the exact same conversation with dozens of other people.

ZWISLOCKI: Absolutely true.

ALLEN: It's a legitimate complaint, but the problem is there isn't enough money. Let's go back and talk about von Bekesy.

ZWISLOCKI: With von Bekesy I invented this audiometer, which I think is still popular in many clinics, I think they are still using it. And so when Luescher came to me, I immediately saw that the problem was that it was at threshold, and now we know for sure at threshold you don't see the recruitment, really; it's a little higher that you have to go. So it was an interesting phenomena that was related to the equipment that I didn't understand what it was, and it was present only at high frequencies; it was not present at low frequencies. So what was happening is people with normal hearing produce a certain size of excursions going back and forth from hearing and not hearing. It was not JND, it was…

ALLEN: The first JND—the threshold JND.

ZWISLOCKI: I don't believe that. It was that they went from hearing to not hearing. And interestingly, when somebody had loudness recruitment, the excursions, the difference between not hearing and hearing, decreased. It was quite substantial. It was evident. Not so that it was completely outside the standard deviation, but it was…

ALLEN: Are you talking now about Mary Florentine's…

ZWISLOCKI: No, much before that.

ALLEN: They have this idea about loudness in elevated thresholds that the first—OK, go on.

ZWISLOCKI: I don't interact with…Mary is all fine and nice, but I don't pay too much attention.

ALLEN: Because she works with Rhona (Hellman), and Rhona and you are tied at the ankles.

ZWISLOCKI: We were. Now what happened with Rhona? I sent her an email a month ago and never got an answer. Do you have any knowledge of what is going on with her?

ALLEN: I saw her a few months ago, maybe six months ago.

ZWISLOCKI: That was the last time that I wrote to her and did not get an answer.

ALLEN: She had some possible cancer that might have been quite dangerous. I believe she is OK.

ZWISLOCKI: I know that. That's why I wrote to her.

ALLEN: She's lucky to be alive?

ZWISLOCKI: I don't know, is she alive?

ALLEN: I don't have any information. We all care about her. She is a wonderful caring person. Both her and her husband. I call them H-squared. H and H. Get it? It's my friendly little joke. We love them. Let's move on.

ZWISLOCKI: So anyway, so these excursions were changed by loudness recruitment and became smaller at high frequency, but not at low frequencies. At lower frequencies there was no effect.

ALLEN: Do you attribute this to the outer ear cell function?

ZWISLOCKI: I have no idea. I didn't think about it. I know that this was happening, and I was thinking clinically. That was bad clinically because it was not a clear sign of loudness recruitment.

ALLEN: I want to make sure I understand. You're talking about Delta I at threshold.

ZWISLOCKI: I'm not talking about Delta I. I'm talking about excursions and Bekesy's audiometer.

ALLEN: There's a tracking. How fast you change the frequency determines how fast it goes up and down, which is a problem.

ZWISLOCKI: Yes. There are all kinds of problems that are very subjective in deciding whether you hear it or you don't hear it and all that. Anyway. So when Luescher came with this to me, can you do better, I said of course, yes. But that's before I really thought. But I quickly had the idea that recruitment was showing up really at higher intensities and you had to do something there. So the only thing I could think of outside of measuring loudness directly, which we didn't even think about then, I was just noticing a simple difference in intensity.

ALLEN: Have you read Fletcher 33, Fletcher-Munson in his loudness paper?

ZWISLOCKI: Oh god, I don't remember. It was such a long time ago. That was my beginning with this crowd, and then I was reading the summaries and reviews of this in Stevens' "Hearing". So, you know. In those days I was not aware of it. If they did anything of this sort before, I was not aware of it. I just thought you can't measure loudness directly because in monaural cases that's easy, compare one ear to the other. But if it's symmetrical, the hearing loss, you can't do that, so how can you determine the loudness? So the thing I thought was with JND, and so using my little inventions that I made as a student to have an oscillator produce very low wave forms, which could be used as envelopes, so I used a modulator unlike Riesz; who used beats. I used the modulated tone to determine JND, except that my modulation was not sinusoidal, it was trapezoidal.

ALLEN: You used two tones, which gave it [overlapping voices].

ZWISLOCKI: Using beats, he was using beats. So mine was an improvement in the sense that it is easier to detect changes with that.

ALLEN: You were linearly ramping up and linearly ramping down.

ZWISLOCKI: Yes. And I learned from Riesz that the best frequency to do that was about 2.5 per second, up and down.

ALLEN: Did you meet Riesz?

ZWISLOCKI: No. Where was he? I don't even know where he was.

ALLEN: He was at Bell Labs.

ZWISLOCKI: Well, that's why I didn't.

ALLEN: But he was in the development area. He wasn't in research.

ZWISLOCKI: That was before I knew anything about America except Fletcher and Stevens and Davis. There was a substantial general literature in Europe, so I was reading a lot of that stuff.

ALLEN: And of course Zwicker?

ZWISLOCKI: Zwicker came later. Roudder came after all this. Feldtkeller, who was Zwicker's teacher, came first, actually, and then came Zwicker and that was all later. I was at Harvard. When Zwicker appeared on the scene, I was at Harvard, and Zwicker came for about half a year. We worked in the same laboratory in Harvard. Then he came here to Syracuse to work with us for also half a year, or a little more.

ALLEN: Who came up with the Bark? It doesn't seem like…

ZWISLOCKI: I don't know who, but that group.

ALLEN: Why would they call it the Bark?

ZWISLOCKI: Because they were German. [Laughs]

ALLEN: So we'll drop that topic. OK. Anymore about the audiometer?

ZWISLOCKI: So I developed the Riesz method essentially, except that I adopted it clinically, and was very successful in detecting loudness recruitment with this method. It was trapezoidal modulation though. And what happened is when people had hearing loss, and you give this modulated tone at higher intensity, the JND was the same as in normal hearing, at equal loudness. Luescher was interested in it clinically, so we were looking at it from this point of view. But it struck me that the phenomenon that JND depended on loudness, not intensity, and it was independent on the growth of the loudness function was a very interesting phenomenon. So I kept that in my mind when I came here to Syracuse, I became professor here, I was given three graduate students, doctoral students. One was Studebaker, who worked on bone conduction.

ALLEN: Studebaker was your graduate student? Really!

ZWISLOCKI: Yes. As you know, he became famous later in audiology. Then I had Herb Jordan, who worked on the JND with me. This work was done, as a doctoral dissertation and was finished in 1962 and Jordan absolutely opposed publication of it because he knew it would start a storm, and he was a good, nice clinician, didn't want to have anything to do with that. And so I said good-bye to it, I wouldn't do anything further on it, until 1980 when Hellmann [?] and company at Harvard, I don't know who all at Harvard, but this Harvard group did a very good experiment I think, were able to produce a stimulus configuration where two loudness curves which intersected at one point but each had a different slope, and whichever stimulus you use for the JND, the JND was the same. So they discovered for themselves, which we knew already for a long time here, that JND was independent of the slope of the loudness function.

ALLEN: This was Hellmann and Hellmann?

ZWISLOCKI: No, Hellmann and Scharf and Teghtsoonian. Both Teghtsoonians, and Scharf, and Hellmann. I think that's the crowd. Hellman was the principal.

ALLEN: Wasn't Teghtsoonian the first author?

ZWISLOCKI: No, Hellmann was. Actually, Teghtsoounian was very much against this whole thing at the beginning and they had to convince him.

So this came out, and we saw it. Actually, she did read a paper I think at the meeting of the Acoustical Society. I'm not sure we published that in a journal. Anyway, that woke us up, and I didn't do any experiments anymore; I just took the data of Jordan's and convinced Jordan that it should be published, and we published it. So there was a sequence, I don't know which one comes first, either Hellmann or our paper. Anyway, Hellmann was discussing just the slope and saying that it [JND] was independent of slope, and we were saying it was independent of the slope and of intensity, and only dependent on loudness. There was a storm in the psychophysical world, as you may know, but I didn't do anything further on it, except to write a theory how this is possible for the JND, let's say the idealized case where you have a constant JND.

ALLEN: Would it be fair to say this still hasn't been resolved? Or do you think it's been resolved in the literature? I don't mean in your mind, I mean…

ZWISLOCKI: I have papers on this.

ALLEN: I know, but is it accepted.

ZWISLOCKI: It's been published.

ALLEN: I know that.

ZWISLOCKI: I'm sure they are arguing. I don't keep up with this. I have so many things that I like to do, constructively, that I had a policy that I'm sure cost me not to take part in polemics on the things. I publish. Some people like it; some people don't like it. I let it go. So I'm not entirely aware of the whole discussion. I know there was a storm. Mary Florentine was involved in this.

ALLEN: And Soren Booth [?].

ZWISLOCKI: Oh yes. And the question is whether constant loudness produces constant JND here or not. It doesn't produce exactly constant JND because of the near miss. You have to correct for near miss. But it is tied to loudness because of the thing that we were talking about, that the noise that is tied to the pure tone. The pure tone sounds pure, but physiologically it's a Gaussian noise.

Clinical Applications

ALLEN: So this JND business, I think we've covered it. Do you want to give a summary of that, or should we move on?

ZWISLOCKI: The summary is, I have a mathematical theory, which I'm happy to side with, and to me the question is closed. We can talk now about something else. There were a few things in between, and they are important not scientifically so much, but clinically. Remember my task was to invent new diagnostic methods, and so it has to do with the geometry, and one of the problems the geometry had was bone conduction lateralization. When you put a vibrator on the head, the vibration goes to both ears, so you mask the ear that you do not want to test and use the one that you test. I viewed that as noise. So when I came to the field, the masking noise for the unwanted ear was a broadband noise because they had to cover all test frequencies. So what I did was to—Well, I decided that this was very inefficient because it was loud and disturbing, so I introduced a narrowband noise centered on the test tone. It was produced essentially by modulating the test tone by a low pass noise. So the center frequency of the noise moved automatically with the test tone frequency.

ALLEN: Oh, you modulated the test tone to the other ear by the low pass noise so that you could use that as a masker. Got it. So it was much lower in loudness.

ZWISLOCKI: Much less loud, and much more efficient. And I attached to it also a test showing in which ear was bone conduction test tone heard; because of the monaural effect of masking. Masking essentially is monaural. If you have a long-duration masker, the contra lateral masking is on the order of only two or three [Db]. So I introduced a method where when you have the vibrator and you want to know in which ear the tone is heard, so you put masking in one ear, and then you increase the intensity of the masker. If the threshold does not change, that means the masker is heard in one ear and the tone in the other, and that's what you want. When you increase the noise, it finally goes to the other ear across the head and starts masking the ear that you are testing, and then the threshold goes up. So you know…

ALLEN: The difference is like 60 Db or something.

ZWISLOCKI: All depends on hearing loss. So I had an interesting incident a year ago. I don't have a hearing aide now, but I had a hearing aide. For this purpose I was tested myself. I went to a clinic here and they tested me on bone conduction, and they performed my tests. And I didn't get wise to it right away, then, "Gee, it sounds familiar," even though I told them that, and they said, "Of course, that's your method!" So I had it performed on myself.

ALLEN: I heard you tell that story at the AAO meeting. That's a great story.

ZWISLOCKI: Yeah, it was very nice. So I am very happy today because something that I did was useful. I like to do from time to time these things that people can use right away, that's useful. That's why I'm involved in hearing protection also.

ALLEN: Was this with Studebaker or was this work alone?

ZWISLOCKI: No, before I left Basle; I was still then in Basle. The masking thing, that was in Basle.

ALLEN: So 1943, '45?

ZWISLOCKI: Things always get published later. I think the date you can find it under is before 51, that's for sure. And they even put it in an audiometer with the monaural so-called Luescher-Zwislocki JND method, which I had done there. It was used internationally, especially as modified by Jerger. Now, it has been replaced to a large extent by testing the impedance or reactance of the ear.

ALLEN: Tympanometry?

ZWISLOCKI: Tympanometry rather than the reactance, and I don't really like that. The last thing I remember of ETH was my Masters thesis. Then, before I went to Basle and I kind of had a good-bye conference with my advisor, I was invited to do a doctoral dissertation. I was allowed to do it in Basle. See, the system there then was that not every master student was allowed to go for a Ph.D.; you had to be invited. So I was invited to do a doctoral dissertation, but to the chagrin of my advisor, I was going to Basle to a medical school. I received permission to work on my doctoral dissertation in Basle. So I was working pretty much as an engineer in the field of audiology, but it was kind of difficult to use audiology for a doctoral dissertation at the ETH. I was searching what can I use? When I was reading German literature, I came across Helmholtz, of course. Bekesy, Helmholtz, and Helmholtz of course long before Bekesy. And it occurred to me that the cochlear mechanics problem was not resolved really.

ALLEN: That's an understatement!

ZWISLOCKI: So, as I told you, in those days the knowledge was much smaller than it is now. It was possible for a student to know electronics and also mechanics, and hydromechanics. During my studies I designed and construct a water turbine. That was of course in Switzerland where the most important subject was water turbines, that's how they make electricity to sell to Germany and France and other countries. So it occurred to me thats how the cochlea worked was not resolved, and perhaps this had enough physics in it so that it would be suitable for ETH. And I let my advisor know, and he approved, so I started my theoretical work, mathematical work on cochlear hydromechanics. I did not know very well the wave problem in hydrodynamics, so I studied Horace Lamb, the classical hydrodynamic work, and found out how to deal with the surface waves Bekesy saw in the cochlea postmortem. So a problem with the cochlea was of course that the basilar membrane did not behave exactly like a surface tension on the surface of water, but it changed locally.

ALLEN: Did you read Wegel and Lane?

ZWISLOCKI: Oh yeah.

ALLEN: 1924?

ZWISLOCKI: Oh, I don't remember that.

ALLEN: Because they had a model with series mass and shunt mass and a spatially varying compliant basilar membrane in 1924.

ZWISLOCKI: Didn't know that. See, there was a disconnect between this country and Europe. You did not know our literature; I knew that from references. And we didn't know in detail your literature. I knew Wegel and Lane. I knew their masking experiments. That's how I knew about the laterality of masking, because that was monaural essentially.

ALLEN: You knew about simultaneous masking, Wegel and Lane?

ZWISLOCKI: Yeah. But I was not aware of this. I was aware of the French work by a man named Reboul who wrote a differential equation, actually a transmission line differential equation provided for the cochlear. But when he was doing that, the basilar membrane properties were not known because Bekesy didn't do it yet. And so in order to explain the maximum vibration which everybody assumed was there since Helmholtz, they thought that the depth of the canal had something to do with this, so they assumed a constant thickness of the basilar membrane but a variable depth of the canal. And so I came after Bekesy and I studied Bekesy in and out. This was European, you know—Bekesy was in Europe then and was in Hungary.

ALLEN: Where was Bekesy during World War II?

ZWISLOCKI: In Hungary. He was a Hungarian, and he was working with, I think they call it Post Telegraph and Telephone office, and because they knew his work with hearing had something to do with the telephones, they had to adapt the telephones to the properties of the ear, they apparently allowed Bekesy to do this in-depth work on the ear, including the cochlea, but then the cochlea became main subject because it was the most interesting. So this was my main source of knowledge about the cochlea, Bekesy was, and experimentally I'm sure it was—

ALLEN: This was work that he did before he went to Harvard?

ZWISLOCKI: Oh yes. He started to work on the cochlea, I think in 1923, but the first publication that was a bona fide publication, became known in 1928. What he did was first on post-mortem preparations. He opened the cochlea at the apex and saw waves on the surface of the basilar membrane. I mean, not directly, because the tectorial membrane and all that above the basilar membrane was there. But that's where he first saw the waves.

ALLEN: What species?

ZWISLOCKI: Human.

ALLEN: Cadavers.

ZWISLOCKI: Cadavers, yes. He worked in Europe, I think, exclusively on cadavers. Cadavers in Europe were easy to get. There are not all the laws that forbid this and that. If you had a good relationship with a clinic, you could get cadaver parts. I myself got a cadaver. The cochlea I was working with was extracted from a human cadaver, and then I made a magnified model of it just to see its general configuration. Actually it wasn't necessary, but it was kind of pretty to have the whole thing, the shape, and that's when I found that the cochlear shape could be approximated by the shape of a logarithmic spiral. Somebody recently came and said that it was a linear spiral. I didn't really read it. But it makes no difference. It didn't do anything with it.

The curvature I calculated was so small compared to the acceleration that you had within the fluid of the cochlea that it could be neglected. It didn't matter.

ALLEN: How did you get led to these Bessel function solutions?

ZWISLOCKI: By looking through a handbook of differential equations. We had in our engineering study differential equations, partial differential equations. I mean I took all of the things that I could. Conformal mapping with a very famous mathematician in Zurich named Fisher. He was the counterpart of Jerry Wisner (JBA: later president of MIT).

ALLEN: So this was complex variables?

ZWISLOCKI: Oh yes.

ALLEN: Laplace transforms.

ZWISLOCKI: Oh yes, all this. I was invited, because of what I was doing, forgot a lot of these things. I mean I could come back and do it quickly. But yes, we did all that.

ALLEN: Did you ever go to the city of Konigsberg Germany? According to Arnold Sommerfeld (Vol III, Electrodynamics, Page 1, Part I) this was the scientific fountainhead of mathematical physics. started by Franz Neumann [1789-1894] who was the professor of Kirchhoff.

ZWISLOCKI: Von Neumann?

ALLEN: This Neumann was long before John von Neumann. Von Neumann was at Princeton.

ZWISLOCKI: But Von Neumann came from Europe, didn't he?

ALLEN: Yes, but that's a very different Neumann, by maybe 100 years.

ZWISLOCKI: No, I don't know.

ALLEN: David Hilbert?

ZWISLOCKI: Hilbert, yes.

ALLEN: You wouldn't have ever met him, would you?

ZWISLOCKI: No. I was in the clinical audio logical environment. I went to the Department of Physics at the University of Basle to tell them what my problem was, and they gave me some literature. They had a very mediocre professor of physics who was a full professor, and a much better associate professor who was very busy and didn't have too much time for students.

ALLEN: In your'45 model, or your Ph.D. thesis I think probably we're talking about here…

ZWISLOCKI: It was published in '48. The first publication was 1946, and there is an interesting story attached to that, because this publication was before Bekesy actually saw the waves in the cochlea, not just at the apex. When you read Bekesy, his book that is a collection of all his papers, you notice that it is chronological, but the paper concerning the waves in the cochlea is not in this order. The paper that shows the waves for the first time was followed by older ones; they didn't keep the chronological order. I don't want to be nasty about it, but it struck me.

ALLEN: Well it's his book—that's his prerogative.

ZWISLOCKI: Oh yes his prerogative, but it's interesting to me, the fact that the waves were theoretically discovered by me in 1946, and Bekesy's paper on that subject was 1947. So the waves in the cochlea were really discovered theoretically first, except that Bekesy saw some waves at the apex in early work, and then he saw waves in his models. He did a tremendous number of experiments on cochlear models, which he developed very thoroughly. I mean he gave the model a viscous loss conserved by the dimension and all that. These are serious models.

ALLEN: You are talking about his fluid mechanical chamber…

ZWISLOCKI: That's right.

ALLEN: Tonndorf made some of those.

ZWISLOCKI: Well, he made big ones. But that was interesting from the point of view of the cochlear eddies there, and he could see various other things. But his work really didn't contribute much. It was highly exaggerated; Scientifically, Tonndorf contributed very little, but he was a very intelligent person. He understood things. He said to me I informed the medical people about the research results.

ALLEN: I think one of his contributions was that he made people more aware of what was going on. He played a very important role.

ZWISLOCKI: Exactly. In America he brought me together with the otological population. That's how I entered this international society.

ALLEN: That was an important role.

ZWISLOCKI: Very important. He was a good friend of mine. But you know, we talk objectively. I separate these things.

ALLEN: Did you know he is a good friend of mine?

ZWISLOCKI: No.

ALLEN: Well, I worked at Columbia for several years. He was a very, very dear man.

ZWISLOCKI: He was a great guy. At almost every meeting of the Acoustical Society with some others we had dinner together and all that, and discussed of course.

ALLEN: He thinks very highly of you. I don't know if you know that.

ZWISLOCKI: I didn't know that. I think very highly of him as a person, and I admire that being an otologist, and not having a background in physics or engineering really, he understood the cochlea and similar things, also the middle ear, as well as he did.

ALLEN: He was a veterinarian, I think.

ZWISLOCKI: He was?

ALLEN: Originally he was on a U-boat, and his degree was in veterinary medicine.

ZWISLOCKI: Oh, that's possible. A physician on a U-boat, he was an otolaryngologist physician on a U-boat. I'm sure he told you the story, how he came with the U-boat to New York and was watching the skyline of New York through the periscope. He was a great guy, and I missed him a lot when he died. He was a bridge between me and otolaryngology in this country, and I think he performed this function for quite a few people, which was a great benefit because the two worlds did not have any communication with each other.

ALLEN: They didn't understand each other, and he understood both sides.

ZWISLOCKI: That's right. So this is a little aside with this Bekesy book. I wrote my theory, and then I discovered from what Bekesy wrote, especially later, Bekesy did not understand how the cochlear worked.

ALLEN: I agree with that.

ZWISLOCKI: I am surprised. That makes two of us, I think. First of all, if the maximum vibration of the basilar membrane took place at the resonance of the basilar membrane, you couldn't possibly have waves past the resonance point. So in my dissertation in '48, I derived that the maximum arose through interaction between the compliance of the basilar membrane and friction, resistance. In those days when I was kind of working as an engineer, the RC oscillator was very popular, so we paid a lot of attention to resistance. So that probably helped in coming to the conclusion that the maximum in the cochlea was not at all due to resonance, but it was due to interaction between the compliance and the resistance. I calculated that, and my results agreed with Bekesy's post-mortem, and all the constants I've had were post-mortem, so it fit it very well. I made comments on that, which Bekesy didn't like at all. Of course the dissertation was accepted and I got the doctoral degree.

The requirement was that your doctoral dissertation had to be published by a commercial publisher, so I went to *Acta Otolaryngologica* because that was easy access to otologists with whom I already had contact through Luescher, and so this was published. And of course I don't want to be misunderstood. I had an enormous—and still have—admiration for Bekesy as an experimentalist and a wise person. I would say when I was working on the cochlea, he was my god, you know—he was it. But the truth is the way you see it, and when I saw there were some things wrong with his interpretation I said that.

ALLEN: And of course he didn't like it.

ZWISLOCKI: No, I'm sure he didn't. But he was a wise man, so it was very complicated, our relationship. So I sent my dissertation to him. It was in German, and his preferred language was German. So he read it, and must have passed it to some other people at Harvard and MIT who were involved in organizing the first Speech Communication Conference.

ALLEN: Which was where?

ZWISLOCKI: It was held at MIT.

ALLEN: Is it published in the "Transactions on Information Theory"?

ZWISLOCKI: I don't know about that. The papers were published in the Journal of the Acoustical Society, the general Acoustical Society. My papers were published there, and others also.

ALLEN: Tell me again, it's the first…?

ZWISLOCKI: The first Speech Communication Conference.

ALLEN: Who ran it?

ZWISLOCKI: It was several people. The official chairman of this was Professor Locke (I don't remember the first name) of Harvard. Maybe he was at MIT; I'm not sure. Anyway, Prof. Locke was the chairman, and when I came to this country, the first invited dinner I had was at his house.

So let's go back to the event. It is somewhat interesting, I think. So some of the people who were organizing, probably some of the younger people, were Licklider, Beranek, somehow Jerry Wiesner was involved in it also. The conference was bigger than just for speech—Norbert Wiener took part in it. I think Shannon did, I'm not sure. Stevens, Galambos and Hallowel Davis, Rosenblith and Bekesy of course. Bekesy was very interesting. Nothing is simple [chuckles].

Bekesy must have shown my dissertation to several people because I got a telegraph from Locke, I think, whether I can come, all expenses will be paid. I had to ask Luescher's permission. However, he was in Paris. Of course I wanted to come, right, even though I couldn't hardly speak English. I was taking some lessons. I telegraphed Luescher and he telegraphed me back that I could go.

ALLEN: What year was this?

ZWISLOCKI: That was '49 or 50, I think.

ALLEN: And Fletcher wasn't there.

ZWISLOCKI: Oh yes, he was there. I'm sorry.

ALLEN: Any other Bell Labs?

ZWISLOCKI: Well, I didn't know who was at Bell Labs and who was not then. I bet Gardner was there and Munson was there, but I didn't know them. The reason I met Fletcher is the following. So I get this telegraph, and I telegraphed Luescher in Paris—I knew the address somehow—and Luescher says of course, go. There was no problem with cost, but we ran out of time so I had to fly, and Luescher I think added the money to allow me to fly.

ALLEN: And it would take a week or five days.

ZWISLOCKI: That's right. Because they didn't have me in mind at all until I sent my dissertation accidentally at the right time and just before the meeting. So then Stevens wrote me a letter explaining more before we met. But anyway, so I flew. The flight is worth describing, but we don't have time, because I flew near the North Pole. Oh, the polar lights—it was just out of this world. Anyway, I landed in New York, where I had an uncle, then took a train to Boston, and at the station I was received by Beranek and Licklider, and they were driving an old car, old, old station wagon, which had in front of the passenger seat a hole in the floor.

ALLEN: So you had to keep your foot out of there!

ZWISLOCKI: So I could speak English enough to communicate, so I asked them what for do you have this hole there, and they said, "Oh, this is for the ashes." [Laughter] Then they said, "That's the good car that we are driving because it has brakes!" [Laughter]

ALLEN: So they had a good sense of humor.

ZWISLOCKI: Yes. We were young then, you know.

ALLEN: Beranek is older than you, isn't he?

ZWISLOCKI: Oh yes. They were all somewhat older than I. In '49, I was 27.

ALLEN: Beranek must have been five or six or more years. Isn't he like 92 now?

ZWISLOCKI: Yes, I think so. Anyway, so they brought me to the graduate dormitory—that was where the European guests were located. And then they said, "As soon as you get ready, you have to go to Prof. Locke's for dinner." Well, you know, I'm half sleepy, just came from Europe, jet lagged of course. So we go to a very elegant European style dinner, with service and everything. We have dinner. I tried to say a few words. I understand some things they are saying; not too much. And after dinner, as is often customary, men go to the smoking parlor and ladies go to a different room. So we went to a library with a lot of books, the library, and I don't know whether everybody smoked or not, but I found myself surrounded by these people: Beranek, Rosenblith, Licklider, Jerry Wiesner. Jerry Wiesner is important here because he played a main role—he was the man who knew math.

ALLEN: He became the president of MIT.

ZWISLOCKI: Oh yes, and advisor to Kennedy, and then he made Rosenblith provost when he became president of MIT. So a really nice social event. But I discovered that they are eventually coming to my dissertation and to my talk, which was on the dissertation. I had 40 minutes; I was invited to give a 40-minute talk. And they sat talking about the theory of the cochlea, mainly it was Jerry Wiesner, so they were asking some key questions. And then I don't know which one it was, if it was Jerry or somebody else, said, "Well, that's all right." And then Licklider, who was supposed to read Ranke's paper, said, "So, what am I going to do?" What happened was Ranke, who was an otologist, and was in Hitler's command, in the military he was one of the top positions, he had a theory of the cochlea also. It was written before mine, actually. And he assumed that the waves in the cochlea were short compared to the depth. My assumption was they were long approximately, which was also a crude approximation near the maximum which I then corrected in this book. So we had a big discussion with Ranke in Europe already. He accused me of plagiarism because I used an exponential function for the basilar membrane compliance, and he used in his equation an exponential function also. So he decided that this was plagiarism. You know, exponential function is used all the time, and my theory had nothing to do with his. His was boundary value approach and mine was the differential equation, telegraph kind of equation approach. So he sent in a complaint to the ETH, and the ETH sent it to me, and I responded, and they decided that of course there was no plagiarism.

Ranke was invited to this conference. Ranke was a good friend of Bekesy. Bekesy had close ties to Germany. Actually, I think Siemens helped him with instrumentation for his experiments. So at first I was not invited at all; they didn't know about me. Probably at the suggestion of Bekesy they invited Ranke. At this conference at Locke's home when they learned my theory and they compared with Ranke's and decided that Ranke's theory was wrong. But before that, when Bekesy learned that they invited me to this conference, he was invited. He decided he was not going to take part in it.

ALLEN: That's terrible. Because he was being true to Ranke, I guess?

ZWISLOCKI: He was true to Ranke, and probably wasn't so happy about some of my criticisms either. When they invited me, by the way, from what they read already they decided to un-invite Ranke, and they did it in the following way. They told him that he was invited, but they cannot pay his expenses. In those days, Europe was just recovering from the war, was very poor, even prominent otologists. So Ranke didn't come; sent his paper. And Licklider was supposed to read his paper in which Ranke severely criticized my theory, and his paper was just before my paper—in that session. His paper was first, was supposed to be Bekesy's paper, my paper was second. So I read my paper, and I began by saying, because it was after Ranke, "I'm very sorry that I have to talk to you about a wrong theory." The room exploded. They really liked this.

ALLEN: You were being sarcastic about your theory. Am I understanding this? I just want to make sure I understand the nuance here. Ranke's paper was before yours…

ZWISLOCKI: Ranke was saying the time of paper, most of it, saying that my theory was wrong. So I said "I'm sorry that I had to talk to them about a wrong theory." So laugh, laugh, laugh, and that gave me a better understanding of what was going on in the audience. I must not have been used to talking in a microphone. I talked a bit loud. And so Fletcher, who was in the first row, now I remember well, went out—I already knew who and where he was. I was terrified that Fletcher was so unenthusiastic about my talk that he wanted to leave. But he went around and then soon came to the last row. Apparently it was too loud for him. But he was there! And then when Fletcher came in, I also noticed that von Bekesy was there. So Bekesy also came to hear my paper!

After my presentation, I had some very nice questions. They were quite helpful to me, they assigned an interpreter so whatever I could not communicate directly in English the interpreter would help. The most pertinent question I got was from Bob Galambos. He was interested in tuning curves, and things like that. Once we had the session it was lunch time.

Bekesy comes to me, and in German tells me he would like to have lunch with me. I was flabbergasted! But of course I accepted. So we went to the Harvard Square to have lunch there in one of the fine restaurants. He invited me to that. And during this lunch, one of the things he told me is I shouldn't fool myself about America. He explained that it is very nice when you come here as a guest, but when you work here, the situation is very different. At first I didn't understand it, but later people explained to me that he didn't really want me to come to America.

ALLEN: He didn't want the competition?

ZWISLOCKI: I don't know. But anyway, he didn't want me to come to America. The morning after the conference I got a telephone call from Harvey Fletcher, at 8:00 in the morning. You know, we had a lot of parties going on during the meetings at Harvard, and they lasted until 3 o'clock in the morning, a lot of drinking. When I drank I spoke English much better [chuckles]. So I get a telephone call at 8 o'clock in the morning from Fletcher.

ALLEN: He didn't drink, he was a Morman.

ZWISLOCKI: I was half awake, he said "this is Harvey Fletcher," so I woke up very quickly! He said, "You know, I have a position for you, if you are interested. But I know that this evening, Stevens will make you another offer. So if you prefer Stevens's offer, I will not be angry with you." So in the evening there was a party in the psychoacoustics laboratory. A lot of people. And at one moment Stevens asked me to come with him to his office and he offered me a position as a research fellow. Coming from Europe, if somebody offers you a position at Harvard, you don't…[laughs]

ALLEN: You just salute. Yeah.

ZWISLOCKI: But I had enough presence to say, "Well, I'm very honored, and thank you very much. But I have commitments in Switzerland, and it is possible that I could come only for three months." So he said, "Why don't you go home and see how things are, and if you decide to come here for a longer time, the position is open." I was also in the process of divorcing my Swiss wife, so it was very complicated for me. So I came back to Switzerland, and first of all settled my affairs, especially with Luescher—Luescher was extremely disappointed, but was a gentleman. So I took a leave of absence for a year from Basil. So I came for a year. Things in Switzerland that year were being liquidated, and so I decided to stay at Harvard for three years, which was the normal tenure for a research fellow in at Harvard. After three years Stevens asked me if I wanted to stay another three years, which I did. After the other three years, the faculty of the Department of Experimental Psychology in which the laboratory was housed made a protest: "You cannot keep Zwislocki any longer! This is bad for him. This is not a tenured position." So I left.

I had several offers. One was at Columbia, Cyril Harris. Then I had one in the Navy. I had Baylor Medical School. I even went there, Baylor, but I decided that Texas was too far from Europe. That was a difficult problem with Baylor, because they were giving me $25,000 a year for research, no questions asked. In those days, that was big money. It was a famous school, but I just didn't see how I could live there. So they offered me a position here (Syracuse University), a faculty position, in the hope that I would organize a research laboratory in what was called the Gordon D. Hoople Hearing and Speech Center. He was a very famous otologist; he was one of the top people. He was a statesman in otolaryngology here in this county. He had an interesting past. He was a missionary in China, etc. So he was instrumental in bringing me here. And then there was DeCarlo, a famous audiologist, who was directing the center. And Syracuse had certain advantages. The disadvantage was it was *not* a famous school. The advantages were that it was close to Boston and close to New York, and it was offering me a position in which I could do what I wanted to. So I accepted.

That the Center was administratively located in the School of Education, which didn't fit too well. But I brought with me from Harvard an ONR project. They just took some money from the Psychoacoustic Laboratory there, I learned later, and gave it to me. It was $50,000 a year, some such thing. So I started a Laboratory. I also applied to NIH for a grant, and I was successful. So our laboratory grew. Zwicker came for a while to work with me, Bert Scharf came to work with me, and some other people. Before long, the tail started to wag the dog, and so the chairman of the Department of Electrical Engineering approached me if I really wanted to stay there or perhaps be an engineer and join back the engineering crowd. So they offered me an associate professor position, which I took. I mean I really didn't sit there. I organized my environment so I had a lot of young human subjects that could do what I wanted, but there was nobody to talk to outside my little crowd.

So I came to engineering, and I had a little problem with my degree being an associate professor because I was doing my auditory research still, I continued to do that, and I didn't think that the faculty of the College of Engineering would permit me. You and I know that engineering is just great for doing auditory research, but they didn't know it. I mean I assume they didn't know it.

ALLEN: They simply didn't understand multidisciplinary research back then.

ZWISLOCKI: So I went to the dean, who was the greatest dean I encountered in my university career, and I told him the problem, and so he was very quick in deciding, and he said, "Well, you don't have to go. I'll just change your appointment. I will make a new appointment and I'll appoint you as full professor of electrical engineering." So that's what he did, and things worked out very well. I first organized a laboratory of sensory communication, taking the title from the symposium that was organized at MIT, and that became the Institute for Sensory Research at the departmental level, so I answered directly to the dean. That worked great, and we were very, very multidisciplinary.

ALLEN: When did Ron Verrillo come?

ZWISLOCKI: He was from almost the beginning. Ron was a social psychologist, to begin with, in Rochester. He got his Ph.D. at Rochester University, and he already heard about our Bioacoustic laboratory, he comes to me and he tells me that he is very disappointment with what was going on in his profession, especially at Syracuse University; could he possibly join my group. So I told him, "But Ron, you are a psychologist. You don't know physics. You don't know math. It's a great risk you'll be taking." He said, "I'll take it." I said, "OK, we'll try it. But if it doesn't work out, don't blame me.

ALLEN: Well, it seemed to have worked out.

ZWISLOCKI: It worked out because Ron was a very likeable people kind of guy, and he knew how to get coworkers that were knowledgeable in things he was not. So at first I introduced him to some psychophysics and worked with him, but I never put my name…maybe once…on the publications. Then he learned tactile psychophysics and anatomy and was getting good coworkers and first-rate students. So his laboratory grew and became quite well known in tactile.

ALLEN: There wasn't that much tactile research going on at that time.

ZWISLOCKI: No, there wasn't.

ALLEN: So that was important.

ZWISLOCKI: There was some, Geldard in Virginia was certainly one of the most famous experimental psychologists. He was in touch.

ALLEN: Nice pun Joe!

<u>Cochlear Mechanics</u>

ZWISLOCKI: I believe with Hal Davis that we have two maxima in the cochlea. It's not quite accurate, but it's easy for people to understand. The Bekesy maximum and the live cochlear maximum. So what we were talking thus far is the dead cochlea, post-mortem. And I view the theory of the cochlear mechanics postmortem as a contribution of mine, but maybe, there were some people who did it before, I will never be sure. I was the first to show theoretically the cochlear waves and the nature, of cochlear mechanics. Then, specifically, I showed that the maximum vibration was not at the resonance point of the cochlear partition but was ahead of it.

Where the resonance of the partition happens is much beyond the maximum, and that's why you have the maximum in the presence of waves; otherwise, if the maximum were at the location of the resonance, then beyond the maximum or very soon after that you would not have any wave motion; you would have a decay of amplitude. So this is the second thing.

Then the third thing, which people have been uncomfortable with for a long time, has been to show that the input impedance of the cochlea is not inertial but resistive. Frankly, for anybody who models a structure like that in terms of transmission lines and knows a little about these things would know immediately that the input impedance, if you don't have wave reflection, must be resistive. So this was confirmed now quite a few times, and the MIT team did some experimental work, and they reluctantly agreed that it was more or less a resistive. There was an article not long ago, I don't remember where and the details, which showed very nicely that the input to cochlea was resistive. By cochlea I mean the actual true cochlea—that means where there is a canal divided longitudinally by the partition. In fact, there is a column of fluid ahead of this part which is just a plug of fluid which adds a mass component to the whole cochlear impedance, and of course there is impedance of the windows, especially the oval window whose impedance is substantially greater than of the round window.

ALLEN: In the oval and round window and the annual ligament.

ZWISLOCKI: Yes.

ALLEN: Was Lynch the first one to experimentally observe this real impedance? Or did Von Bekesy? Or who was the first person?

ZWISLOCKI: No, Von Bekesy—They were all close. But you see, it is very difficult to have a pure cochlea, so their measurements are contaminated. But Lynch didn't remove the membranes of the windows, and so he has an impedance that has resistive and a reactive components.

ALLEN: But that reactance is at very low frequencies, below 100 hertz, and he identifies it as such. I mean he was clear that this low frequency compliance in the input impedance was due—There's no question about where it came from.

ZWISLOCKI: There is no question about it. But their measurements with the resistance are a little bit moving around, kind of, as I remember. But anyway, there were several times this was confirmed, and I had a correspondence with Bill Peake with whom I was closer, and there was a question of the factor of 2, whether the 2 should be in the differential equation or not. So we went back and forth, and we agreed completely finally that it should be there. Peake, I think, was for a while a leader of this group. So this is the basic cochlea that is true post-mortem, and these things are also true in live cochlea, as far as I am concerned, but there is much more to it, as you are well aware.

END OF FIRST PERIOD

So anyway, that was a first period, and it ended pretty much when I came to the United States. Here, Bekesy had a big tiff with Weaver and Davis. Davis and his associates performed some experiments with cochlear microphonics; didn't find an amplitude maximum, and decided the cochlea worked like a low pass filter. Now, I looked carefully at the experiments that they did and Bekesy did, and found they had different input conditions. Bekesy kept the displacement of the stapes constant, and Davis and his associates kept the amplitude of the basilar membrane in the base constant.

ALLEN: Using the CM?

ZWISLOCKI: Using the CM, yes.

ALLEN: They didn't really have a way of measuring that.

ZWISLOCKI: No, it was all CM. So Bekesy was very unhappy about it. Then Wever saw that he could not explain the mechanics of the paradoxical waves of Bekesy—why the waves were running away from the source. If you stimulated the cochlea at the apex or some place in between, the waves still run away from the oval window.

ALLEN: The waves always go towards the helicotrema regardless of the source location, right?

ZWISLOCKI: Yes, they always go towards helicotrema. Weaver didn't understand that the waves could do that, and maintained that, therefore, the waves that Bekesy saw were an illusion, and the whole thing just escalated, I guess.

ALLEN: Did Weaver have any physics in his background?

ZWISLOCKI: No, he was a psychologist and an anatomist. He was a very good anatomist. This will come up later, because I got a lot of important information from Weaver's work.

So Bekesy was attacked by Davis and by Weaver, and I don't remember whether it was Weaver's or Davis's paper came out—Do you know Memorial Hall? It was in the basement of Sounders Theater, and one part of it—

ALLEN: This is where Von Bekesy's office was.

ZWISLOCKI: Yes.

ALLEN: That's your office, too.

ZWISLOCKI: Yes. There was a corridor going around in an elliptical way, and so when Bekesy found out about this work, he ran around this thing, and finally I think stopped at Stevens's office and complained, and told me about it, and also I read the articles. And so I felt sorry for Bekesy, and decided to unscramble this apparent paradox that they couldn't understand. So first I showed that the input conditions were different in Bekesy and Davis, and that's why Davis didn't see the maximum and Bekesy did. When you have a constant amplitude of the stapes, and a resistive input impedance, then pressure at the base increases with sound frequency. And so as you go up in frequency, you come to the maximum. There is a maximum.

ALLEN: The pressure over the velocity (the impedance) at the stapes, is a real constant. Right?

ZWISLOCKI: Yes

ALLEN: So for the displacement, you get the pressure over Z times J omega is equal to the displacement. So the displacement is dropping as you go up in frequency, if the impedance is real and independent of frequency.

ZWISLOCKI: No, you increase the frequency and keep the displacement constant…

ALLEN: The velocity is constant.

ZWISLOCKI: The velocity is not constant, but increases with frequency.

ALLEN: The volume velocities are going to be equal, I guess.

ZWISLOCKI: The amplitude has to go up, and that goes to the maximum, and then because of the friction everything goes down. The thing is that the input to the cochlea depends on the longitudinal velocity, but the displacement of the basilar membrane depends on the transversal pressure. That means the compliance of the partition, the impedance of the partition is a compliance.

ALLEN: Well the invariant is because of incompressible fluid that the complete displacement of the basilar membrane, the integral of the displacement of the basilar member is equal to the stapes displacement because of incompressibility of the cochlea. Gauss's Law.

ZWISLOCKI: The integral, but not at some location; there is a wave. So when you have a constant displacement of the stapes, then if you have inside a resistive input impedance, then the pressure amplitude goes up and with it the displacement amplitude of the partition. So that's why when Davis was doing his experiments and kept the displacement of the partition constant at the cochlear base, because of cochlear resistive impedance, as I said, then he couldn't get the maximum. It was getting a low pass filter. So that's one thing.

Weaver's thing had to do with paradoxical waves, so the question was how is this possible to have the vibration source near the cochlear apex and the resulting cochlear wave run toward it. Well, he didn't think there was a wave because he couldn't understand that the waves can go towards the source. So he was stimulating at the apex, and the waves were running toward the apex, he couldn't believe that was possible for a wave.

ALLEN: This goes back to that question that we were talking about as we were walking out (not recorded on audio) about bone conduction and where the stimulus really comes from. It could be that you're stimulating the bone, but the input really comes through the middle ear. That's a possibility.

ZWISLOCKI: In fact one component does. I have a paper on that, and that's when I had a problem with Bekesy, because he thought the bone conduction, part of this was coming through the inertia vibration of the middle ear ossicles, and I found that if you have an earphone on the ear, the bone conduction components come mainly from the motion between the earphone and the head. When you have an earphone on the head, it is very like the vibration if you put a vibrator on the head.

ALLEN: What are you describing the path of sound is when you acoustically drive the skull? What is the path of sound from the bone driver into the cochlea?

ZWISLOCKI: You have a low frequency—the whole head vibrates.

ALLEN: Let's say at one kilohertz or two kilohertz.

ZWISLOCKI: Oh, I don't remember that. Because around this frequency it already has not a uniform motion of the skull, but you have a bending mode, but I can't at this moment remember the exact pattern.

ALLEN: If sound can go in through the vibration of the walls of the canal, it could go in through the oscilles…

ZWISLOCKI: Oh, it goes in everywhere. It is very complicated. I don't think anybody really unscrambled the whole thing. Tonndorf was working a lot on this.

ALLEN: Tonndorf told me that he didn't know. He studied it, but he didn't know. He never knew the final answer. I was at his bedside just before his death. I visited him. His wife was very caring. We all cared about him. It was very sad.

ZWISLOCKI: I agree. So with the mathematics I showed what was happening with the paradoxical waves, that when you stimulated the cochlea through the fluid at the apex, let's say, on one side of the partition, there was no exit for the fluid at the apex on the other side, so it had to go as a compressional wave (JBA: Compressional wave) to the base of the cochlea, and then run back as a the transversal wave (JBA: slow wave.). You can have both waves in the cochlea. (JBA: Both waves must exist, it's a matter of relative magnitude. This is determined by the BM wave speed over the sound speed in the fluid. This depends on the ratio of wavelengths.)

ALLEN: So Bogart and Peterson dealt with that issue. They had the common mode and the differential mode.

ZWISLOCKI: Yes, they had both modes, yes. The thing is that they didn't try to explain the paradoxical wave; they just dealt with the two kind of waves, which is absolutely right, and then it was shown by Dan Geisler, I think, that this compressional wave produced a phase change along the cochlea only at frequencies above 8,000 hertz.

ALLEN: I don't know that paper. How would he have shown that? I know Susan Voss worked with Bill Peake and maybe John Rosowski, and she measured cochlear microphonics, and she drove both oval and round window in phase, and she saw no cochlear microphonics. So that kind of ruled out a compressional wave.

ZWISLOCKI: No, the compressional wave wouldn't do anything to the partition. It affects both sides, and the partition is, therefore, not moved. The impedance of the partition—the partition acts as if it didn't exist when you have the compressional wave running this way. It's running on both sides.

ALLEN: I see the cochlea as a differential signal analyzer. The compressional wave is subtracted, and is thus removed. Indeed, it's a very nice design.

ZWISLOCKI: Yes.

ALLEN: Differential—it looks at the difference. It's like an op-amp, it looks at the difference between the oval and round windows.

ZWISLOCKI: Regarding the compressional wave, when you generate a compressional wave at the apex, then the partition impedance is low compared to the fluid impedance, and the compressional wave runs towards the base as if the partition did not exist.

ALLEN: Aren't Mario Rugero and Jonathan Siegel, and maybe some other people, talking about how the wave coming back from auto acoustic emission is a compressional wave; the wave going in is…

ZWISLOCKI: Well, that is common to the more recent work. We showed it even on the electrical model of the cochlea, that when you stimulate the partition, then the wave is propagated in both directions—let's say some place in the middle of the partition, you stimulate the partition. One wave goes to the apex and another to the base.

ALLEN: That's what Duck OnKim showed with his propagating distortion products. They generated a distortion product, and then they looked at the excitation pattern using neural population studies, and they saw the excitation pattern of the distortion product propagated in both directions.

ZWISLOCKI: This is all newer. I'm talking about—

ALLEN: '70s, I'm talking '70s. Peter Dallos said that there was no non-linear propagation.

ZWISLOCKI: It was before that. There was no publication yet explaining this thing that we're talking about. Because that's at the time of the Weaver and Bekesy dispute, which was in the '50s. So we're ahead of that. So anyway, I explained the difference between Bekesy-Davis on one side and Bekesy-Weaver on the other side through these calculations, and I showed them to Bekesy. Bekesy was very happy. He was relieved that his waves are okay. And I did a paper on that at a meeting of the Acoustical Society, and I remember that well. Licklider was there, and Licklider stood up and said, "Well, this may be the most important paper of this meeting." That was why I remembered that [chuckles]. This was in the '50s. I was at Harvard from '51 to '57. Then a famous otologist in New York City, a Jewish guy, he had his own practice, Lempert—maybe not Lempert. Not long before Lempert did the stapes mobilization, right? Well anyway, this otologist asked Bekesy and also Weaver to consult for him. Bekesy got interested somehow in his clinical thing. Before Lempert invented the mobilization operation, another otologist, Rosen, made a whole in one of the vestibular semicircular canals to replace the blocked oval window. You still had a hearing loss of 30 decibels; that was much better than what people had before that.

ALLEN: What was the point of this operation? Why did they do this?

ZWISLOCKI: Because the transversal wave in the cochlea requires two windows, one on each side of the partition—He simply was looking for a way. Stapes didn't work, so he was looking for another hole in the cochlea, because you need two openings to have a transversal wave.

ALLEN: So this is to solve otosclerosis?

ZWISLOCKI: Yes, absolutely. And so later came the mobilization procedure of Lempert's and the little opening in the oval window, and all these are much better. Anyway, so Lempert engaged both Weaver and Bekesy to consult for him, and that's how they got together. They got together, made peace. Weaver sort of surrendered. And they published a paper together, and everything is hunky-dory. But I explained how you could reconcile Bekesy's wave results with Weaver's results in stimulating the apex, and I thought they should mention that I did that, but they didn't because that was very important to Bekesy, this thing. And so I remember, I went to Bekesy and told him—I was shaking—"What are you trying to do to me!" You know, our careers depend to some extent on mutual recognition, right.

ALLEN: Entirely.

ZWISLOCKI: So it was important for me to be credited with this resolution of the conflict between Bekesy and Davis and Weaver, three of the most important people in cochlear physiology. So then Bekesy in the next publication kind of mentioned a little bit that I did something. That's one thing that the world doesn't know about Bekesy: he was fine, he was a gentleman and all that, but he did not like to give credit to his coworkers.

ALLEN: You could say that of Stevens too, couldn't you (JBA: yes and Zwicker.)?

ZWISLOCKI: Well, in a way. I didn't interact with Stevens that way, so it didn't strike me. Stevens did a great thing in psychophysics. I consider Stevens the second greatest psychophysicist after Fechner. That doesn't say anything about his character. Just what he did was to almost convince people that he could measure, for instance, loudness directly by attaching numbers to it. His explanation was long and all that, and we could talk for hours about that. Rhona Hellman and I did a lot on that. You later did work on loudness also. But Bekesy was really bad. He didn't give credit to anyone. He didn't give credit to his coworkers. At a meeting in Hungary, when we had this meeting in Hungary, a Hungarian middle-aged man came to me, and they knew probably that I was thinking very highly of Bekesy, and he said, "Well, you must know one thing. When Bekesy was in Hungary doing his work on the cochlea, he had seven coworkers. He didn't mention one of them." The man was shaking.

ALLEN: He must have been one of them, I take it.

ZWISLOCKI: Yes, of course. So Bekesy was doing the same thing here. Being European, he should have known better. He was not subjected to the American attitude towards this thing. He got worse than American; I got the information about the American attitude from Stevens, so I cannot generalize.

ALLEN: Well, in this country today you always put your students as the first author and you be the last author.

ZWISLOCKI: The nice people do that; not all. That's not in medicine. I don't know, but I don't think that they do. But if there are people in another laboratory who did something that you are using, you should also give credit to these people in other laboratories in other countries—whatever. That's the European attitude that I learned, and Stevens was telling me that you just mention people when you need to explain something. So anyway, Bekesy was really bad as far as this was concerned, so he did the same thing to me. You won't find in Bekesy's publication my name but maybe twice. I think, as far as I know, maybe there was some precedence of which I don't know, I think I supplied the theory for Bekesy's experiments on post-mortem cochleas. If I am wrong, correct me.

ALLEN: You asked me why I'm here. I'm here to try to help to get the record straight. Your opinion is important, and that's what we're sampling here. You could be wrong, and Bekesy could be wrong, Stevens could be wrong. But at least we're going to get the record down as to what your opinion is. So we get your opinion, and then somebody else can figure out what's true.

ZWISLOCKI: That's my opinion on that matter.

ALLEN: So we're just interested in your opinion. This is your honest opinion.

ZWISLOCKI: That is my honest opinion. This is the end of his era, I would say. Then Bekesy did all kinds of experiments at Harvard, you knew that already, which I went to Eddie (Edwin) Newman, who was the chairman of the department.

ZWISLOCKI: Edwin was a psychologist, and was chairman of the department for a while. George Miller came in and took over the chairmanship. I complained to Newman about this attitude of Bekesy, that he didn't mention hardly anyone. Other people did some theorizing on the cochlea. You won't find much reference in Bekesy's things.

The Live Cochlea:

ZWISLOCKI: Now the next period is the live cochlear. To me, as far as the mechanics of the cochlea is concerned, the first paper that I know of was Australian by Brian Johnston and Boyle. It was a very good experiment. They demonstrated that the vibration maximum on the basilar membrane was much sharper than Bekesy saw, like animals.

ALLEN: How about Kohlloeffel?

ZWISLOCKI: Following that, as best I know, is Rhode, 1971. Rhode's very elegant experiment, he measured both amplitude and phase, and the maximum wasn't as sharp as you see it today, but it was much sharper. And he found, as well as other things, one was that post-mortem the basilar membrane became softer. Kohlloeffel did that actually before him, but Rhode did it in a much more accurate and systematic way. That was very important. It was something that many modelists did not take into consideration, that post-mortem basilar membrane compliance is greater than during life. That means the velocity in the propagation in the live cochlea is twice as great as post-mortem. I calculated, of course, when I was doing all the theorizing for Bekesy experiments, even calculated the velocity of wave propagation, and came out with 45 meters per second. I didn't consider then that there was a difference between life and death; we didn't know that.

ALLEN: You're saying on a given position on a basilar membrane, the velocity is greater in the dead cochlea.

ZWISLOCKI: No, greater in live cochlea. Of course you have to be careful near the maximum because there are other factors that play a role which are non-linear essentially. So I am talking about the velocity basal to the maximum.

ALLEN: Because the velocity slows down right when you get to the CF.

ZWISLOCKI: Yes, very much so. I'm talking at this moment about the part of the cochlea where you have long waves—that means they are an approximation of long waves where the velocity is independent of frequency.

ALLEN: Maybe a half an octave basal to CF?

ZWISLOCKI: Yes, something like that. You can look at the curve and decide where you make the cut off. So what happens in the cochlea, why does the maximum become sharper? Of course it gives us a clue with this active process, that's what Davis called it, and apparently that is the sort of thing Gold in England predicted mathematically. It is a kind of interesting aside about that, because when Gold wrote his paper, Gold and Pumphrey, I got the manuscript to review, and what they were saying in that paper had absolutely no experimental base whatsoever then. And this was a Royal Academy paper, and I suggested not to publish it. Sometimes one makes an error [chuckles]. But they published it anyway. So they kind of always referred to Gold as the man who gave the idea. He didn't give it. Why? Because of the resonance. The resonance I think was wrong.

ALLEN: He claimed that there was very high damping in the cochlear if you didn't have this added process.

ZWISLOCKI: Right, absolutely. But he also had the maximum resonance and all that.

ALLEN: I think he didn't do his homework in calculating the viscose boundary layer, because if you calculate it properly—Dennis Freeman did work on this—you'll find out that the damping is inherently very low, because the viscose boundary layer is extremely small, it's in the micron range. So the fact that it's a small object with fluid in it does not automatically mean there is large damping. So he didn't do his homework and he didn't calculate the boundary layer thickness.

ZWISLOCKI: Yes, but you can do better than that, because when you don't have the active process, you have post-mortem cochlea where you kill the outer hair cells, you don't get a sharp maximum. So I think this is indisputable evidence. I know you were toying with the idea, that perhaps you don't need the active process to explain the thing. And I agree with you, we modeled it. You put low damping, you get the same affect practically that you get with the active process.

ALLEN: Many people have played with damping in cochlear models, and it doesn't really solve the problem. It just makes the peak very, very sharp and very narrow, but it doesn't make it look like normal cochlea. So I don't think this is about active versus passive. I'm not that comfortable injecting my opinion into this. I want to hear about your opinion. But it's hard for me not to express my views!

ZWISLOCKI: I knew this would happen. But I don't disagree with you. The model you have on the computer, you can play with it if you ever had nothing better to do. You can get a sharp maximum without the active process very well.

ALLEN: If you're willing to put a tectorial membrane in there.

ZWISLOCKI: Oh yes. That's the key. You know, this may be very arrogant, but I think that my conclusion that the tectorial membrane has a greater compliance, so to speak, globally, than the aggregate of outer hair cell stereocilia was as important as any other thing that was discovered in post-Bekesy era. Some things are very underestimated, it seems to me. For instance, the discovery by Flock that the stereocilia of the outer hair cells are very stiff. The key element in the modeling that you did and I did, they have to be stiffer than the tectorial membrane per centimeter length; you know, you have to do all the transformations, if I may grossly state it that way. So that was a necessary ingredient. The second ingredient was my measurement of the compliance of the tectorial membrane.

ALLEN: This was with Lisa Cefaratti

ZWISLOCKI: Yes, she was my assistant.

ALLEN: That's the work where you measured the tectorial membrane.

ZWISLOCKI: Yes, it was with Lisa. And this is being criticized that I did not do it on sufficiently small amplitudes.

ALLEN: You do what you can do, right?

ZWISLOCKI: No. I showed that it was linear; the stiffness did not change with amplitude down to zero. The curve extrapolated a little bit, went to zero, the stiffness curve. And it was a linear up to several microns.

ALLEN: So it was very high compliance (low stiffness), and very linear at high levels, is that what your suggesting?

ZWISLOCKI: The compliance was about ten times greater, if you take a comparable length of the cochlea and take the aggregate of the outer hair cells, stereocilia, which was measured by Flock and Stralioff. If you take the stiffness of a single stereocilium, and you take the aggregate of these things, per centimeter length of the cochlea, and you take a comparable length of the tectorial membrane, the tectorial membrane is, by an order of magnitude, softer than this aggregate. You would have to show me good experimental evidence to prove that it wasn't.

ALLEN: What's the implication of that? (Allen agrees, but what to draw out Z's opinion).

ZWISLOCKI: Your model and my model could not be possible. The tectorial membrane…

ALLEN: It wouldn't play a role if it were rigid.

ZWISLOCKI: Right. It was a completely different situation. What I say popularly, "it's not the tectorial membrane that moves the stereocilia; it's the stereocilia that move the tectorial membrane". Of course I just got this all in my book on Auditory Sound Transmission. You cannot explain, as far as I understand it, the events in the cochlea unless you assume that the tectorial membrane can oscillate radially; second, that it is softer than the aggregate stereocilia of the outer hair cells. You know, I read articles on this subject. People are finding absolutely incredible explanations for some of the things that happen in the cochlea. If you look at the 6th chapter r in the book, you'll see how well the theoretical results here agree with experimental results. I mean just superimpose. Even the narrowing of the maximum, when you measure say the neural output of the cochlea cochlear microphonics, are dangerous because of their space constant. But you see that the maximum in sheer motion is considerably narrower than the maximum on the basilar membrane, and it is practically symmetrical.

One thing that happens is that the maximum becomes narrower, and the reason is, look at the basal turn of the cochlea, well before you reach the maximum. And let's say I'm right that the stereocilia aggregate has its highest displacement at the tectorial membrane in the radial motion of the tectorial membrane. What happens, there is a very little sheer motion between the tectorial membrane and the reticular lamina because the stereocilia entrain the tectorial membrane. Mathematically this is all in my model; it's implied. So then you come to the series resonance of the tectorial membrane, which is produced by the mass of the tectorial membrane interacting with its viscoelastic attachment to the spiral limbust. That was the zero that was dear to you because in 1977 (*J. Acoust. Soc. Am.,* **62** pp 930-939) I think, you found that no way can you explain the cochlear events with a second order system, as people generally thought. That had to be something else; had to be mathematically a zero. You didn't know what produces the zero, but you knew there was a need for it. Popularly speaking, the way I see it, I supplied the evidence for the zero by showing that the tectorial membrane is softer than the stereocilia.

So we go to the parallel resonance produced by the stereocilia and the tectorial membrane interacting, and then you can visualize perhaps an electrical analog of this whole thing. You come to a parallel resonance produced by the tectorial membrane interacting with the stereocilia, and it's parallel because in mechanics when the stiffness precedes the mass, you get a parallel resonant circuit (*J. Acoust. Soc. Am.,* **68** (6) pp 1660-1670). So when you have the parallel resonance, the amplitude of the motion of the tectorial membrane becomes very large—larger than the motion of the basilar membrane. Between the two things, from the zero and then the maximum vibration, you have—actually, at the zero you have a phase change by 180 degrees. Agreed?

ALLEN: Agreed. And Duck OnKim saw in his neural population studies (Kim et al, JASA)

ZWISLOCKI: Yes. Well, it is a giant knot.

ALLEN: Well, I think that's different, because that's non-linear.

ZWISLOCKI: You're right, that is higher up.

ALLEN: But Duck OnKim saw this 180 degree phase shift right where the tail meets the tip.

ZWISLOCKI: Oh yes, I looked at his data.

ALLEN: And he stands by his data today. He still believes the data. But unfortunately nobody ever repeated that. When I bring this up in front of Mario, Mario goes into orbit. And Mario has lots of his own phase data, but it really doesn't look like Kim's data.

ZWISLOCKI: Yes, Mario is one of the most productive men I think in the cochlea. I used some of his data. I used also the Pfeiffer-Kim phase data in my book, which was very important for the calculation of the wave velocity.

So then you get to the basal part of the radial motion, the motion of the reticular lamina and the base of the stereocilia is greater than of the tectorial membrane. That means the stereocilia are bent in such a way that excitation occurs during basilar membrane displacement toward scala vestibuli. Past the resonance, when the tectorial membrane goes through the parallel resonance, you have a phase reversal by 180 degrees. You can do very nice experiments to show that it is in the outer hair cells. You kill the active process, so called, then the amplitude of the tectorial membrane becomes low, becomes smaller than that of the reticular lamina, and you have phase reversal back.

ALLEN: How do you explain Rhode's and Ruggero's and other's measurements where they show that the basilar membrane is neural-like? They would claim that the basilar membrane tuning is neural like, whereas my understanding of the model you just described is the basilar membrane would be broader tuning, but the sheer signal driving the stereocilia would be narrow tuned.

ZWISLOCKI: Agreed. I understand.

ALLEN: Well, there seems to be some theoretical problem with this idea, that if the tectorial membrane is sharpening up, the basilar membrane responds, as I understand you to be describing. The measured basilar membrane response should not be equal in tuning to the neural tuning.

ZWISLOCKI: They are different.

ALLEN: The basilar membrane response is one thing; the sheer signal is another.

ZWISLOCKI: It is.

ALLEN: Basilar membrane is not the same as the neural?

ZWISLOCKI: Correct. The difference is toward the base. That's how the curve, the tuning, becomes almost symmetrical. There is a suppression of the amplitude basal-ward.

ALLEN: So neural tuning curves have low frequency tails, and basilar membrane responses do not have similar tails.

ZWISLOCKI: It is the reverse—well, I don't know which phase you're talking about.

ALLEN: [Draws on blackboard] So the basilar membrane response, there's a S1, S2, and S3, and the basilar membrane response has a S1 and an S2 that are very different than the neural response. So the S1 and the S2 in the basilar membrane is really more of a continuum, whereas neurally S1 is almost zero, and S2 is much closer to S3 (J. B. Allen. (1988) "Cochlear signal processing"; In A. F. Jahn and J. Santos-Sacchi, Editors; *Physiology of the ear,* pages 243-270. Raven Press, 1988).

ZWISLOCKI: Absolutely.

ALLEN: But these people, they keep talking about what happens at the tip, and they think they're done. And you and I are talking about what happens in the tail.

ZWISLOCKI: Exactly!

Cochlear Mechanics Continued (next tape)

ZWISLOCKI: These are experimental curves, by the way.

ALLEN: You're looking at page 332, Figure 6.25 (Joe's book)

ZWISLOCKI: I hate to attach myself to it because things are going a little fast, you cannot systematically do these things. These are the typical basilar membrane transfer function passive resistance, and you have this figure that you drew on the blackboard.

ALLEN: **(**Joe is looking at page 327 and 328 of his book.) These are model calculations here? This is Figure 6.26, Theoretical Magnitudes of the basilar membrane. These are standing waves due to the helicotrema

ZWISLOCKI: Yes, I don't bother with such things. This is the phase change concerning the famous outer hair cells.

ALLEN: Which were measured…

ZWISLOCKI: And you see the 180º phase change here near the minimum.(JBA: I believe Joe is referring to where S1 meets S2, where typically high frequency turning curves have a local dip. On the tuning curve this is viewed as amiximum, but one knows this is an upside-down tuning curve, so this is therefore referred to as a minimum. To an outsider such jargon must be very confusing.)

So this is what shows the difference, why the neural is this way and the basilar membrane is this way, and the reason for that is that because of the stiffness of the stereocilia before the resonance of the tectorial membrane, the stereocilia move the tectorial membrane about the same amount as the reticular lamina, and so you have very little sheer motion, and that makes it the transfer function steep…The interesting thing is that when you…oh, what's the name, at Harvard. He works in the Peabody Laboratory, but he's at Harvard.

ALLEN: Charlie Liberman.

ZWISLOCKI: Yes, Liberman. Liberman showed that when you eliminate the active process and you made the cochlea sick, you have a region of enhancement of the neural output.

ALLEN: The hypersensitive tail—this was Liberman and Dodd.

ZWISLOCKI: And there is the explanation of it. What happens when the cochlea is sick, you do not have the increased motion of the tectorial membrane; I mean the motion of the tectorial membrane is always smaller than of the motion of the reticular lamina. Now, if it is much smaller, then you'll get a bigger amplitude gain.

ALLEN: What about Prestin and the soma of the outer hair cell? What's the role of that?

ZWISLOCKI: I don't know.

ALLEN: What about Peter Dollas's story about Prestin and the stiffness?

ZWISLOCKI: I don't know about that. I tend to side with Peter rather than Hudspeth. But in my model, it's not relevant. It comes to the same thing whether you have the source in the stereocilia or in the somas of the outer hair cells. I cannot say anything about that. Except intuitively, I agree with Peter. It seemed to me to be such a waste if this if it did not have an effect, and the stereocilia, we know the structure of the stereocilia - It consists of many parallel fibers; it is a little bit difficult to look for the source there.

ALLEN: That's the story.

ZWISLOCKI: That's half the story, yes. In some animals it is true, it is shown. So it is very inviting to think that in humans and in mammals it would be the same thing. I don't think so, but I don't have any proof. It is outside of the things that I did work on. So anyway, the tectorial membrane is key difference between the traditional modeling of the cochlea and what I think and I think you think is the modern view of the cochlea.

ALLEN: It's just taking people so long to get there! That's my problem.

ZWISLOCKI: You agree.

ALLEN: Absolutely.

ZWISLOCKI: But you know, you read history, it happens again and again. One of the greatest from our point of view of vision and hearing and electrical circuits, March, 19^{th} century physicist Mach. He wasn't recognized at all by his contemporaries. He had to die to be recognized. I think there are many things—I read an article not long ago, in the Proceeding of the National Academy actually, where the guy said he was lucky enough to be recognized for the particular thing that he was talking about during his lifetime. I am too old, I have too many other things to do to fight for it.

ALLEN: I think it's mentally very healthy to recognize that this has happened, and there are so many people who have died or have committed suicide (Boltzmann) because their work wasn't recognized, and that's just not worth it. I mean it happens all the time. People had a good idea, they did their best to sell it, didn't get sold, and then a hundred years later finally…

ZWISLOCKI: I don't know if I'm peculiar, but if I count all my discoveries—pardon the all; the few—and the application. You know, I did some work that then was applied clinically, so I can measure the time span between the discovery and the clinical application, and for me it has been around 15 years.

ALLEN: Nice. And David Kemp experienced the same thing. It took 15 to 20 years to…

ZWISLOCKI: I think David Kemp made a big discovery, bigger than his understanding.

ALLEN: I think he did appreciate it.

ZWISLOCKI: I don't mean appreciate it. What he was doing was not sophisticated enough to make such an important discovery. But he did notice it. That's one part of—

ALLEN: He was looking for it, Joe.

ZWISLOCKI: Was he consciously looking for it? Because of Gold.

ALLEN: Probably.

ZWISLOCKI: I didn't know that he was. I had the impression that he accidentally…that's what I was told.

ALLEN: I'm not going to go on record as to what exactly happened. I don't want to make stuff up, tell you more than I know. But I certainly heard him more than imply that he was looking for it, and based on Gold's work. So I'm pretty sure he—well, he certainly represented it as if he was looking for it. And I think he was. He got more than he bargained for, which was good.

ZWISLOCKI: I think a very important thing is in science to have peripheral vision. You look for one thing, and you discover something else which may be much more important than what you are looking for.

ALLEN: The most important thing is that when you see something you didn't expect, that you move on it—to recognize that it is important. Regardless of who you're talking about, a great scientists recognizes the really weird thing when they see it.

ZWISLOCKI: I call it peripheral vision.

ALLEN: This is a great term. That's what separates a great scientist from an average scientist.

ZWISLOCKI: Stevens told me about that, what separates a great scientist from the good scientist is the recognition of the significance of the discovery, and also, the magnitude of the problem. Little scientists do kind of unimportant things, whether accidentally or on purpose, and so it is very important to choose your problem—is it a key problem, an important problem, or is it just…

ALLEN: Don't solve problems that aren't worth solving.

ZWISLOCKI: Well, some people better do, because they do have to be solved eventually in a small way. Nelson Kiang said one thing to me that I remember, was that 90% of science is contributed by 10% of scientists. I believe that this is true.

ALLEN: Probably an understatement.

ZWISLOCKI: Anyway, coming back to the cochlea. I wish people could buy the notion that the tectorial membrane is very flexible, soft and act mainly as an inertia, because this would solve so many problems for them. And it is not quite understandable for me, this obstinance.

ALLEN: They seem so dead-set against this idea. Yet you and I independently came to the same concept, and perhaps you before me?

ZWISLOCKI: In latching on to a concept that is really a 19th century concept. Bekesy has to be blamed a little bit for it, not that he did it on purpose, but it happened, and that is this. When you look at some articles, one especially in his book, you see that he put probes into the tectorial membrane, just as I did, but he didn't measure anything; he just looked at the shape of deflection. And you see that he put a micro pipe head there, and it's a very nice shape like that, which shows you that there is not very much bending motion but is sheer motion, kind of. But then he did an experiment where he stained the tectorial membrane very heavily with Janus Green. The tectorial membrane became stiff like a board, as he said.

ALLEN: So what's your interpretation of that?

ZWISLOCKI: Who knows much about these things? Well, the simple answer is that apparently that some properties of Janus Green upset some structures in the tectorial membrane that makes the tectorial membrane stiff. It's also how I discovered it.

ALLEN: What I would take out of that statement is that when you put Janus Green in there, it goes through a configuration, which is what everybody is assuming it already is. So the implication is that if you can make it a lot stiffer by adding Janus Green, then it's not that stiff in the first place.

ZWISLOCKI: Well, that's what I did. Bekesy doesn't comment on that. I just know he was using Janus Green, and from all the things I see, he was heavily using it.

ALLEN: If it was already very stiff, and you make it stiffer, you wouldn't notice the difference.

ZWISLOCKI: Yes, but he didn't know at that time whether the Janus Green made it stiffer or didn't; he just concluded that it was stiff. But when he didn't stain it or very lightly stained it, I don't know what he did, but he drove micropipettes through the tectorial membrane, you know, just like I did with my electrodes. So it couldn't be completely stiff when it was not stained. By the way, I have preparations where I didn't stain it; I used micro spheres to follow the displacement of the tectorial membrane, and I got the same results as with the light staining with

Janus Green and also Alcian blue. Alcian blue has other effects. Tonndorf investigated quite a bit of this and some others.

ALLEN: He worked on the tectorial membrane?

ZWISLOCKI: Anatomically. They were staining it. There is a paper of his with a physician. I don't know, he didn't do much else, where they were using all kinds of stains on the cochlea, and found all kinds of—they didn't measure mechanics. They just did it from various colorings and all that. You know, MIT here did quite a bit with the staining. They found, for instance, it was not the perilymph that affects the tectorial membrane a lot. The calcium content was very important. That's why in my preparations I used very light staining in most, but I kept everything as it was naturally.

ALLEN: What did Karen Steele contribute?

ZWISLOCKI: Karen Steele, she was studying the structure. She didn't make any mechanical measurements that I know of that would be helpful here.

ALLEN: Marinna Vatter did some very amazing things. She did some really nice work. She worked on the bat and she did 1000 TM cross sections. She just did some really lovely work, and probably didn't get, so far anyway, much credit. But I think her work is phenomenal. When she showed me her data, the light bulb went in my head, and I then really and truly appreciated the significance of the TM. It was impossible to ignore this after I saw her micrographs. I was amazed by her data and I told her so.

ZWISLOCKI: The problem with the bat is that it is special.

ALLEN: That's one of the reasons why it's interesting, right?

ZWISLOCKI: Yes, for the bat, but not for others because the width of the basilar membrane, it varies the specialization around the location of the bat, like to do sonar.

ALLEN: The acoustical fovia, as we call it.

ZWISLOCKI: But this is only good for bats so far. So if you are at the stage where you don't understand really how the typical cochleaa works, I tend to work on the typical cochlea to first understand that and then go from then on and study the perturbations. Apparently many people have this attitude. The most frustrating thing here is the conservative scientists, and egocentricity, too. I think we are all a little bit that way; it comes with the trade [chuckles]. If you didn't do it, your coworker didn't do it, it can't be important. That's one thing. The second thing is conservatism. I say scientists are very liberal politically and very conservative scientifically. Now, that has it's a good side.

ALLEN: Maybe there's a conservation law here.

ZWISLOCKI: Yeah. If you were too liberal, it would be a mess. So you have to be careful. You have to be shown something that it is so. But you have to be reasonable about seeing the demonstration. I mean not be a chauvinist. The people who don't believe today the tectorial membrane is not stiff are chauvinists. They are just blind. And some even did the experiments— This brings Peter Dallus to mind. We had a long discussion. Peter is very Hungarian in this way. He always would defend Bekesy, no matter what. That's the impression I have, and that's fine. His wife made a beautiful medal, which I am lucky enough to have.

ALLEN: This one at the top? [Retrieves from a shelf] Who made this medal, Joanie?

ZWISLOCKI: Yes, Peter's wife. She is a sculptor, she is an artist.

ALLEN: You know they were at my house the night before, Peter and Joanie. I invited him to give a talk at the University of Illinois. I didn't know Joanie made this. This is great.

ZWISLOCKI: Oh yes, she is a first-rate artist. Peter also finally got this also. I'm sure his pleasure is double because he got it for his achievement in the cochlea and his wife made it also.

Back to science, Peter had his hemi-cochlear model with Claus Richter. There are several papers on this. And I had a lot of discussions with Peter because he is latching on to the stiff tectorial membrane—as far as I know; maybe he has now changed, but best I know so far he didn't. Bekesy says, and that's it! And what Peter did, which did not occur to me until two days ago, is actually to demonstrate that I am right, especially about this piece here.

ALLEN: This here being Figure 6.31 on page 234.

ZWISLOCKI: In his hemi-cochlea, he showed that not at the maximum, someplace basal, all the parts of the basilar membrane move together, where his preparation is not sensitive enough to see even 10% relative displacement. So when he says basilar membrane, reticular lamina, tectorial membrane, all move together. Well, they couldn't do that unless the stereocilia were very stiff. I mean stiff compared to the tectorial membrane. I find it too tiring to go to meetings, so I kind of apologize that I don't take a more active part in things. I'm getting old.

ALLEN: You're 87 years old, Joe. You did your thing. You're still mentally very active.

ZWISLOCKI: I'm still working, but at different things. I don't like to repeat myself too much. So I wrote the book—take it or leave it. There are some errors in it; I know about that. At some time that's going to be corrected, and all things are going to change probably. Now I have another book coming out end of next month or beginning of November. It is *Absolute Psychophysics*.

ALLEN: That's what I meant when I said you have two books; the second one hasn't come out yet.

ZWISLOCKI: And I have a third book in my head, if I live long enough, and the third book is going to reverse the psychophysics based on stimulus-centered psychophysics.

ALLEN: Tell us a little bit about that.

ZWISLOCKI: No. Sorry. Bekesy told me not to talk about anything that one didn't do already.

ALLEN: One reason I would agree with that is when you actually do it, you find all sorts of things that didn't turn out exactly like you thought and you need to work it out. So if you project where you're going with an idea, you'll probably…

ZWISLOCKI: Yeah, but he was saying that for a different reason. You are right here, but he had another reason in mind. When he came to America, he was very free in showing people his experiments, that's what he was telling me. They stole his techniques, but didn't do it well, and messed up the experiments, and then they came out against him. And so he became traumatized to the point where you could not get into his lab. See, I was a lot in his office, where he would show me statues, various sculptures, all that, we had good talks, but I never saw Bekesy at a physical table doing an experiment. And apparently at Harvard, in this whole department, the only person who was once shown was Stevens. Of course Stevens was director of the whole project which gave Bekesy money, so he may have been forced. But this man had real trauma. Interestingly, he was doing the same thing to other people.

ALLEN: Well, it's kind of sad. It seems like he was a bit of a paranoid man.

ZWISLOCKI: I think he probably was.

ALLEN: And my experience is that people can't steal your ideas, because you spend your lifetime trying to convince them of your most important findings, and you can't do it. So the idea that somebody is going to steal it is kind of outrageous, because even when you tell them in your very, very most sincere way, they won't believe it even then. So the best ideas, it's impossible to steal them, because people reject them, even when they're given the best possible information.

ZWISLOCKI: You're right, up to a point. But they are perhaps ideas not that important. Techniques, I've had them stolen many times. We had a misunderstanding. I didn't quite know the situation, so I was accusing you of having stolen mine. I later looked at the things, I know that in '77 you had an important article which demanded the zero thing, and which you were already looking at to document that zero point. But people do steal. You know, it's not always conscious. I had an administrative director here who was an electrical engineer. I got him from the department because I didn't want to do administration, really, of the Institute, so they gave me the person who was running the everyday administration. He was a pretty good theoretician as far as electrical networks are concerned. One day I made some mathematical derivation on the blackboard, which he kind of agreed with halfway. A month later approximately he comes to my office and said, "You know, Joe, I think I had a good idea," and literally repeats what I told him, and this one was absolutely honest. He was not conscious of the fact that I told him this.

ALLEN: I have observed that also. You show somebody something, and their mind is in a fog, then they go home and they work it out, and they think they came up with it. You were trying to show it to them and they didn't get it the first time around, and then they worked it out because you inspired the thought, and then they think it's their idea. How do you know that you didn't have the same thing happen? This is the problem.

ZWISLOCKI: I'm sure I did. Because it's so frequent, but how is it possible that I would—you know.

ALLEN: This is human, and we shouldn't be critical of it, unless somebody is just plain outright wicked, but I think that's rare.

ZWISLOCKI: Not all so-called scientists are scientists. I mean there is a lot of science being perpetrated by people who have no business in this, and these are the most dangerous people. But among scientists, I think that these agreements and this kind of plagiarisms that are unconscious, subconscious. But I think that there are more honest people in science almost by definition, because you are looking for truth.

ALLEN: It's so easy to get caught. By scientific method, you have to leave a trail for how you came to where you came to. And you have to publish your results. So to cheat the process is just asking for trouble. David Baltimore, I think this is an example we should all look at. The guy is an outstanding researcher, scientist, rock-solid, and he gets accused of cheating, and in the end it's proved wrong. I think before you go and accuse somebody…

ZWISLOCKI: Meanwhile, it almost ruins our life. It can.

ALLEN: I think you have to really have some rock-solid evidence before you accuse somebody of anything.

ZWISLOCKI: If you do something and then you discover that somebody has a publication that actually repeats what you did, the simple thing is to send a reference to the person, maybe an interesting letter, something like that. But I had a nasty occurrence lately with the *Proceedings of the National Academy of Sciences* which really surprised me. [Discussion continued off tape]

Add on concerning the last item in the interview.

Around 2010, a Faculty member of MIT and some of his students published in the *Proceedings* a paper on cochlear mechanics, which described an experiment showing transversal waves on an excised tectorial membrane floating freely in fluid. The waves implied that the tectorial membrane was flexible. This confirmed my findings that led to a revolution in our basic concepts of cochlear mechanics. As a member of the Academy, I wrote a laudatory comment for the *Proceedings*. I stated, nevertheless, that such waves could not occur in the cochlea in situe because the tectorial membrane was attached through the stereocilia of the outer hair cells and the outer hair cells themselves to the reticular lamina. My comment was rejected by the PNAS editor on the basis of the opinion of a reviewer that, although I made valuable contributions to our knowledge of cochlear mechanics in the past, this was a long time ago, implying that I could no longer be considered as an expert on the subject. The current rules of the Academy introduced by the editor made it impossible for me to defend my comment. Nevertheless, a book of mine published in 2002 by Lawrence Erlbaum Associates under the title of *Auditory Sound Transmission; An Autobiographical Perspective,* and mentioned several times in the interview, deals extensively with cochlear mechanics and includes the most recent information on the tectorial membrane in vivo, in situe. The book was reviewed a year after its publication by John J. Rosowski, a prominent member of the Eaton-Peabody Laboratory, Harvard Medical School, and an authority on auditory sound transmission. The review ends with the following sentence: *it (the book) is clearly appropriate and recommended for all more senior students with a primary* *interest in auditory mechanics, including those senior students who have studied this field for more than 10 years*. Outside of me, there are currently only two members of the Academy whoever did any work on the cochlear mechanics, and this work either concerned isolated aspects of the subject or the subject only indirectly. Perhaps I should mention that the stream of articles on cochlear mechanics dwindled in JASA, the most appropriate journal for the subject, to a trickle within about two years of the publication of the book.