SOME BIOPHYSICAL
EXPERIMENTS FROM FIFTY
YEARS AGO

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... was man ist, das blieb man andern schulding. Torquato Tasso, Act 1, Scene 1
Johann Wolfgang Goethe

Editor's Note

Georg von Békésy died June 13, 1972. In early March of that year, the Editors and Editorial Committee members of the Annual Review of Physiology invited him to write the prefatory chapter for this 1974 volume. He accepted promptly and added “I am sure I will have it finished long before your deadline of June 1973.” He mailed us his completed manuscript three weeks later, early in April 1972, two months before his death.

Professor von Békésy received the Nobel Prize for Physiology and Medicine in 1961. In the presentation speech, Professor C. G. Bernhard of the Karolinska Institute said

The field of Physiological acoustics has a noble ancestry. von Békésy's distinction is to have recorded events in a fragile biological miniature system. Authorities in this field evaluate the elaborate technique which he developed as being worthy of a genius. By microdissection, he reaches anatomical structures difficult of access, uses advanced tele-technique for stimulation and recording, and employs high magnification stroboscopic microscopy for making apparent complex membrane movements, the amplitudes of which are measured in thousandths of a millimeter.

Professor von Békésy, your outstanding genius has given us an intimate knowledge of the elementary hearing process. As a whole this is a unique contribution. The main reasons for the award are, however, your fundamental discoveries concerning the dynamics of the inner ear. With reference to Nobel's intentions it is also a great satisfaction to be able to award the prize for outstanding discoveries which are entirely the result of one single scientist's work.

At the time of his death, Georg von Békésy was Professor of Sensory Sciences, University of Hawaii.
**Introduction**

I like to read the history of science, especially of medicine. It describes the fight between man and nature, which consists of a continuing series of successes and failures. Unfortunately, the majority of present day scientific papers are written in a cold, impersonal style. In earlier times many case histories were presented and by reading them it was possible to make a connection between the conclusion and the actual situation. Today case histories are no longer published and reviews sometimes sound almost like autopsy reports.

Some researchers are of the opinion that it is no longer necessary to study the history of science because science has progressed so fast and always linearly, that earlier experiences are of no value. I do not believe this, but believe, like many of my otological colleagues, that scientific progress is more like a spiral, as in the cochlea of the ear, which always progresses in one direction but with many ups and downs and with many repetitions of earlier forms. I was therefore very pleased to receive from the Editors of this series an invitation to write a free-form article that includes whatever autobiographical, anecdotal, or philosophical comments I wish to make. The invitation encouraged inclusion of personal reminiscences and presentation of my views of any aspects of physiological science of special personal significance. In writing such an article, the pattern of ups and downs, advances and setbacks in my research and other endeavors may be revealed. The Editors believe that this and other prefatory chapters, taken together, will provide a valuable historical perspective of physiology in the twentieth century.

I quote these instructions from the Editor’s letter in an effort to eliminate some misunderstandings that can occur when reviewing earlier periods. One possible mistake is that we do not like the time just past. In art it is very well known that the style of twenty to forty years ago is generally disliked today; you can find the best art objects of this period in junk shops and attics. This held also for art Nouveaux until some seventy years had passed, after which the art of that period became collector’s items. Such aversion to art of the recent past probably results from our rejection of the opinions of our parents and older people and the desire to change, to do something new and better. In not succeeding so well we start to dislike the earlier period.

Another mistake is exactly the opposite of the first—sometimes we tend to make the past more attractive than it really was, omitting the unpleasant parts. And a third problem, ever present in an autobiography, is making your life more important than it really was. I tried to avoid this because it is misleading for the younger generation and can foster mistrust and dislike of autobiographies. I hope I have been successful in avoiding these errors.

One reason my biography is a little different is that my parents were not immigrants, as were the parents of many scientists in this country. I come from a well known Hungarian family of means and therefore our circumstances in general became worse and worse with time. Certainly there were not such improvements as with families who came to the United States and started life fresh.
South Germany and Switzerland Before World War I

I received my basic education in Munich and Switzerland. Munich then belonged to the kingdom of Bavaria and had a certain freedom from the North German atmosphere. At that time South Germany and Switzerland were among the leading countries in science. It was Munich that had the first automatic telephone and it was Munich where garbage was collected in three different cans: one for paper, one for spoilable material, and one for the rest.

In Munich there was much interest in paintings and sculpture, and it was there that I, as a schoolboy at the age of about 8, first saw a well known sculptor, who lived in our neighborhood in the Königinen Strasse, work on a live model. The cooperation between the model and the sculptor was something that I will never forget. Both of them worked on the opposite end of the problem, but they helped each other and produced an exceptional work.

Munich had a museum for the history of science, unique at that time, and many museums for fine arts. The people knew how to live and how to let other people live and how to work. Formally it was a kingdom, but in behavior it was perhaps the best democracy I have even seen.

Switzerland was more commercially oriented. The schools were excellent and life was well organized. There were many refugees from Russia and other countries, mostly revolutionaries like Lenin. They were free to do and say what they wanted. But it is extremely difficult to make a revolution in Switzerland. The intended revolutionaries would listen to the speeches but at noon when the church bells started to ring all over the town, most would remember that they had steaming soup on their table at home and leave. Revolution just does not work after a hot meal.

Life was comfortable and some groups in Berne, the capital, were accused of not doing any work at all. The Imperial and Royal Embassy of Austria and Hungary belonged in this category and it was said that the chancellery of this Embassy had written a sign on the door, "Office hours from 12 to 1."

But despite the apparent beautiful and friendly atmosphere there was tremendous tension in 1913 because everybody expected a war. Even Switzerland had a large military buildup to protect her borders, and the students were drafted periodically because the front between France and Germany could easily be extended into Switzerland if there were no proper defense lines.

World War I

The impossibility of avoiding a war in the next years influenced every boy's thinking. At that time it seemed to make no sense to study mathematics or theoretical physics when you could be drafted a year later into the war. It was known even in 1912 that the Siberian Russian Army had moved partially to the border of Germany and Austria-Hungary. Such masses of soldiers and material were involved that there was no way to stop it. My colleagues in Germany, those who were not Swiss citizens, tried to emigrate from Europe. One of my best friends committed suicide as he simply could see no other way out. Perhaps the worst feeling about the future was the fear that after World War I had ended there would be nothing left of Europe.
Given some of the preparations on both sides, France and Germany, it was obvious that the war would last very, very long, in spite of some of the military who felt they could occupy each other's capital in a week.

Only a few students favored the war. I had one friend from Prague who wanted to serve there so he could participate in the liberation of the Czechs from Austria-Hungary. He volunteered for military service in Prague but unfortunately the regiment to which he was assigned revolted. The liberation force was immediately crushed by the Austrian Army and every tenth member of the regiment was shot. My friend was one of them.

After this and similar experiences, I became an outspoken coward. I did not want to live from day to day waiting for a tragedy, so I made a program for my future life. Once you do so, all your behavior is directed against the disturbing factors that you want to avoid and in this way I completely lost my enthusiasm for military achievements.

Another disturbing factor was the question of a career in music. My piano teacher was like an income tax accountant—very precise, checking everything constantly. His technique was superb. He envied me to a certain degree because the span between my thumb and my small finger, as a boy at that time, was already larger than his, and because I had "good bones" in my hand. I could get from a concert piano about the maximum it could give.

But the real issue was that he played Chopin, for instance, with the precision of a Swiss watch. He realized that my way of playing, with a little Hungarian twist, was much more interesting for the Swiss public. There were things I could not explain to him. For instance, some of the very fast runs across the whole keyboard impressed him very much and I could not tell him that these are always somebody's own manufacture and have nothing to do with the notes which are written on paper. It was Liszt who used them extremely successfully, and knowing about four to five variations, could ornament a piano piece exactly so that it fit the group of listeners. My teacher told me in a very clear way that a concert pianist needed a repertoire of about six pieces and no more, and could travel all through Europe by having only that small knowledge. It would take about two or three years, he said, until I would play them so that I would be a top performer for two or three pieces.

My opinion was that I did not know how to play the piano at all but I did know what the public wanted. However, on this basis I would not start a life career. Another point which for me was very disturbing was the fact that music stuck with me. On hearing a good musical melody, I had to hum it for days and weeks. A painting or a sculpture never stuck with me. I could look at it and draw it and after a few minutes it somehow faded out; but music was different, it occupied my brain and that handicapped good logical thinking on my part. So for this reason, I gave up the playing of music even as a sideline.

Education in Switzerland

Education was developed mainly by Pestalozzi, a simple, practical teacher who worked out new methods of transmitting information from an older man to a young student. I had the good fortune to be educated in a private school, the Institute
Minerva in Zurich. The great advantage of that gymnasium was that it used the so-called mobile class system, in which there were four to six classes in each subject but on different levels of progress. It was therefore often necessary to have six or more professors simultaneously teaching the same thing. A student had free choice to select the class on a level that fit him the best. This type of impedance matching, as we would call it today in electronics, was very useful and generally avoided inferiority complexes in the students and frustration in the teacher. If somebody failed in one subject, he could study the same subject for another year and not be disturbed in his progress in other subjects. In this way I finished my course in physics in about two years so I was able to do experiments at home in my own style and pace. Therefore when I did attend the university I had a definitely better education than many others. I was very poor in English and even more so in German. Unfortunately, Hungarian is a language which has few similarities to other languages, except Finnish, so knowing it did not help in learning other languages.

The University in Berne is financed by the citizens of the Kanton of Berne, a very small group of people. The capital of Switzerland, Berne, had only 50,000 inhabitants at the time. Therefore it was taken as an honor to be able to study at the University. When I first shook hands with the Rector Magnificus he made it clear that to be a chemist and to learn at the University cost the University, as far as I remember, about 5000 Swiss francs per year, although the education of a lawyer there cost the University only around 600 Swiss francs per year. This made it obvious to students that the few chemistry students were using a large amount of the citizens' money for their study and, therefore, the citizens of Berne expected the chemists to produce something of importance.

As a student I had the impression that it was most important to study mathematics. I found out that at the University, just as in most universities, there are very good courses in elementary and in highly advanced mathematics but nothing in between. This made it extremely difficult to learn the subject. I ended up learning four dimension tensor analysis, which at that time seemed to be important in the treatment of the relativity theory. But finally I had to admit to myself that this was a very poor choice. Mathematics, and so geometry, makes a few assumptions and builds an empire based on these assumptions; it is a sort of closed circuit performance. It does not teach anything about what and how to observe.

From mathematics I went for a short time into astronomy, in which I wanted to stay but unfortunately the nights in the cool observatories produced a tremendous physical strain on me. Later on I went into chemistry. Unfortunately, of all the chemistry professors at the University, only one was really original. He was a colloid chemist who was disliked by everybody and who hated to teach. He gave only one lecture every week on colloid chemistry. But looking back at the things that I learned at the University of Berne, I have to say that he did teach me more than any other professor. The reason for this was that he was teaching his own experiences. For his lectures he had one book which he used as a framework, but everything else was his own, and these personal experiences stuck much better than the logically built up performances in the textbooks. Probably one of his most valuable pieces of advice was given to me when I asked him for help on a problem I could
not solve: he listened for a while and then told me simply that the library was on
the second floor and walked away. I was shocked that first day, but having learned
in the military that if you want to complain about somebody you should do so after
a night's sleep, I slept and the next morning realized that he had given me very
valuable advice. Since I did not know the answers and he also did not know the
answers, the solution of the problem could be in the library.

At the University of Berne, the student had complete freedom to select lectures.
There was not even a minimum number of lectures he had to attend. I had, therefore,
a chance to compare the different professors. Besides the colloid chemist, an anato­
mist was the most interesting. He was able to draw with both hands simultaneously.
His drawings of the nervous system and circulatory system were almost as beautiful
as the world famous drawings of Leonardo da Vinci.

The more I stayed at the University, the clearer it became that the subject a person
chooses practically decides his future. I did not have a chance to become the
assistant of a Nobel Prize winner who could show me the road on which he walked
so it would have been easy to continue. My most important decision became how
to select something of good quality. I asked practically everybody questions on that
subject and I found that the art dealers are probably the best advisers in this field.

I asked an art dealer how can you learn which is an original art object and which
is a fake. He smiled and told me there is only one solution—to constantly compare.
You should never buy the things you like but buy one type, and then buy many
different pieces of the same type. Then you will be able to decide on the first look
which is genuine and which is fake without really being able to give a reason for
the decision. I had real success with this method when comparing bronze statues.
I bought several bronze statues from the Byzantine period. The bronzes in Anatolia
have a very well defined patina, but in an antique shop it was never possible to tell
if that patina is the same that fits into the Byzantine period. Having borrowed the
piece and putting it together with all the other Byzantine pieces, there was no
difficulty at all in deciding if that patina was a Byzantine type patina or not. If it
had the same patina it was sure to be a Byzantine; if it did not, it still could be a
genuine Byzantine piece but it may have been cleaned or somthing else could have
happened. I learned to leave out such pieces even when they were extremely attrac­
tive.

To me this method of comparing seems to almost guarantee success over a longer
period of time. But I paid a very high price in working hours because comparing
involves studying the ideas of several people, not just one; a great number of people
whose work I studied and learned to know were simply dropped later, along with
all of the work I had done in their interest, simply because the comparison revealed
their work to be of lesser importance. But I think the process is worthwhile since,
as any archeological excavation shows, it is the quality which determines if some­
thing remains or is lost.

As a student I was very unjust toward my professors. At the end of every semester
I made a sort of inventory of what I learned and what of the learned material could
be useful in later years. Such an inventory showed an unbelievable lack of efficiency.
There were lectures at which I spent three or four hours per week without taking

6 VON BÉKÉSY
anything useful home. One reason for this was that they were very hard to memo-
rize. The more I studied what is memorized and what is forgotten, I came to the
conclusion that the Arab way of teaching by telling anecdotes, used around 1200
to 1400 A.D., was a very good method. It cannot be used in chemistry or mathematics
but certainly it can be used much more than is done today. Today we mistrust
anecdotes because somehow they do not represent a statistical mean value, but in
general they do represent certain principles. I still remember very well the fairy tales
my mother told me, scientific diaries such as of Faraday's, and some of the books
on the beginning of electrophysiology. The anecdotes seemed to be successful be-
cause they rounded up in small, meaningful units what the memory can use and
keep.

Listening to illuminated films, let us say on the method of doing certain surgery,
never taught me anything useful because I could not remember the details. If I
watched the actual surgery I went from landmark to landmark with the surgeon and
could remember the landmarks. In most movies on surgery the landmarks are never
even mentioned so they were, in spite of the huge amount of work invested in them,
of very little practical use.

After the revolution in 1918, my family lost practically everything. I could have
stayed in Switzerland and continued my studies, since the Swiss were very nice and
they even offered me a job to sustain myself, but I had the feeling that I should
somehow help to reconstruct Hungary. That is the optimism of a young man of 20
years. I misjudged tremendously the speed with which a country is able to rebuild
and I misjudged also the new situation in Europe, which was more and more
approaching financial chaos. There were no experts on economy in Hungary, only
excellent people in science.

I received my PhD from the University of Hungary; my thesis dealt with a
method to determine the diffusion coefficient of fluids in a very short time (some-
times less than three minutes). From the diffusion coefficient the molecular weight
could be determined. It was a method I should not have given up but should have
developed much further.

My general feeling at the University was that I wasted my time, especially the
years which were the most valuable for a young man, namely when memory is still
good and judgment becomes more and more objective. This feeling was further
increased when it was impossible in Hungary for a PhD in physics to get a job. I
visited factories and laboratories one after another and I was always told the same
thing—what do you want us to do with a physicist. This was probably the most
difficult time of my life and it was my mother who kept me going.

After a certain time I decided to look around systematically and find out which
was the best equipped laboratory in Budapest. I found that it was the laboratory
of the government controlling the research in long distance telephones, telegraphy,
and radio stations. Hungary was in the middle of Europe and therefore communica-
tion was a very important feature. The government was forced by peace treaties to
spend a certain amount of money to keep the transmission lines in good shape. To
do so, they constructed a laboratory and gave the laboratory a certain amount of
money to buy the necessary equipment. It was this financial support which started
my research. There was a fixed income to the laboratory with no questions asked as to how it was spent. I still think that this is the basis of every big discovery.

The laboratory gave me a salary that was the lowest of all of the 80 people in the laboratory group; it was less than the salary of a carpenter in those days. But I tremendously enjoyed the possibility of learning new things. Every day was a new experience. On one day the telephone line between Prague and Belgrade would be down, on another day the radio station would have some problems. So I had the chance to study large fields of very different background. Sometimes chemistry became important because the cables, with lead mantle, corroded. Or there were stray currents in the ground and they produced trouble. This was the field where I learned to pick up and make conclusions from stray ground current about a sort of large scale encephalography. In some cases I was very successful; for instance, at that time it was necessary to check the condition of international transmission lines. To do so from Budapest a loop was made to London and from London back to Budapest, and the input of the voltage transmitted to London and back was measured in the loop arriving at Budapest. From this, conclusions were made about the state of the transmission lines. In general this measurement took about 15 or 20 minutes. Since there was a great number of telephone lines of this type, there was much excitement every morning in the control room while the lines were being checked. I developed a new method by which I could check the telephone line in a loop in one second. My new method consisted of not using sinusoidal tones, in general use at that time, but by using the transients. By looking up the transients it was immediately possible to see not only the amplitude distortions in the telephone line but also their phase distortions, and the phase distortions were much more sensitive and gave a much better control of the stability of the telephone line. With such small tricks I was able to escape much of the routine work, and I later applied them in the field of hearing.

**How I Became Interested in Hearing**

Of all the developments after the war, communication became one of the most progressive. Hungary was constantly forced to build cables of international quality. They tried to standardize them in endless international meetings, but they were just as unsuccessful as the peace treaties.

In trying to fulfill the often very strict requirements, the government always had to ask for several bids. I was very much surprised to learn in reviewing some of these bids that those submitted by different companies for cables to connect the same two cities differed by less than 1%. At first everyone thought there was a secret agreement among the companies since most of them were controlled by financial groups outside Hungary. However, after having reviewed all their calculations, beginning with the price of copper, paper, and lead, from which the cable is made, I was sure that the mathematics of cable construction was so well developed that it was amazingly precise.

A more difficult point resulted from the fact that a communication line consists of three parts: the telephone apparatus, the cables, and the central switchboard. The
cost of the switchboard did not play a very important role in the calculations of cost, but the price of the telephone sets, because of the large number of them in a city, was comparable in importance to the price of the cables. Therefore, the question was, if we wanted to improve the quality of a telephone transmission, where should we invest the money—in telephone sets or in cable? This was purely a question of economics, but I had the feeling that only the ear could supply the answer. It was a bioeconomical question.

It was possible to calculate the cost of improving the cable compared to that of the telephone set. Unknown, however, was which improvement the ear would most appreciate. My first experiment was to show that the ordinary telephone membrane vibrates in a much more distorted way than does the eardrum; to have a perfect transmission system, the telephone membrane should vibrate in such a way that the quality and the damping is comparable to that of the eardrum. These observations were again made with transients and they gave a clear answer as to where to invest further improvements—international cables or the local telephone system. After settling this question, I received all the financial support I needed to investigate the mechanical properties of the ear and to match the earphone to the membrane of the ear in such a way that sound transmission would be optimal.

My first conflict was with the institute of anatomy where I wanted ears so that I could measure the mechanical properties of the middle ear. In general it was said that the physicist, even if he does not have a job as a physicist, should not get involved in anatomy. Especially as I was employed by an engineering laboratory, they did not want me present at autopsies. Unfortunately I had no alternative and so used the simple fact that the anatomical institute had two doors: one front door with a beautiful stairway used by professors and a back door where I was able to walk in and out with a few temporal bones. Naturally, there were difficulties because removing parts of human bodies (I had been taking them outside the anatomical institute) was improper conduct. The institute made it clear to me several times that if the police became involved I would have difficulty proving I used the bones only for scientific purposes. But in time everything quieted down and I was able to extend my research further to live anesthetized animals. I think this was real biophysics.

It is an extremely peculiar situation that a completely exposed nerve trunk (the chorda tympani) runs across the middle ear. That seemed to be one of the best places to measure the velocity of electrical transmission in the nerve fibers. The professor of physiology at the University of Budapest, Dr. Beznak, heard of my interest in the chorda tympani and immediately brought an anesthetized cat to my lab so we could do the measurements. Measurements at that time were quite difficult because we recorded with a Siemens loop oscillograph. Any time there was an overload on the loop it simply burned out this very expensive equipment; but we found that by putting a glow lamp parallel to the loop with a transformer, we could do peak clipping. I do not know who discovered peak clipping but we used it extensively before it was described in the literature. It made the oscilloscope and the Edelman galvanometer foolproof. Today, I think the biggest discovery is the oscilloscope because it has the advantage that it never burns out. You can use any voltage
without damage to electronic beams. It has one disadvantage in that anyone, even if he knows nothing about electronics, can suddenly become an expert. It was the beginning of the age of bioelectronics.

Experiments with the first cat were a shock to the professor of physiology and to me. The professor said the chorda tympani serves mainly to stimulate the salivary glands with electric discharges. Therefore he put a tube in the salivary gland duct, and under a microscope it was possible to see that every time a small condenser discharge was transmitted through the chorda tympani the fluid in the capillary tube moved along one or two millimeters. It was a beautiful, clear experiment, very impressive to me because it showed that the secretion in a gland is just as precise as the reading in a voltmeter.

Unfortunately, my idea was that the chorda tympani picks up the stimulation of the taste nerves, and the transmission of the electric line is in the opposite direction. We could not agree and finally we gave up the experiment. Seemingly it turned out that the chorda tympani does both things and in different cats the distribution is very different. The chorda tympani is thus not the best object of research.

I was very lucky that I had this failure because it focused, for all my life, my interest on the importance of the material used for experiments. There are animals on which certain experiments cannot be done. That became clear in these conditions and therefore the selection of the right animal, just as it happened later with the squid or the limulus eye, is just as important as is the development of new methods in research.

The Elasticity of the Membranes in the Cochlea

I had the impression that the rapid development of electroacoustical and telephone engineering methods would make it possible to retest the different fields of hearing and biophysics, and I decided, therefore, to direct my attention to the theory of hearing. At that time, around 1930, there were about five different theories of hearing, just as we have today. Also at that time one of the main questions concerned the form of the vibration pattern of the basilar membrane for a pure tone. Since this is a purely physical question, I felt that this could be solved with the modern method.

Helmholz had looked at the basilar membrane 150 years earlier, as had Corti. But they prepared the basilar membrane by chipping off the bones. Since the cochlea is embedded in the hardest bone of the human body, the basilar membrane was generally displaced during preparation and this prevented making precise measurements. In Corti's preparations, the whole cochlea dried out almost completely by the end of the dissection and this resulted in distortion of its structures. With Helmholz it was probably the same way. To avoid this drying, it is best to do the entire anatomical dissection under water or physiological solution. Therefore I used a square bath and let the fluid flow in one side and out the other. The fluid stream was kept constant in the whole cross section and by using a drill instead of scissors for dissecting it was possible to slowly peel off thin layers of the bone. Any time the drill was used, a formation of bone dust clouded the water, but the streaming water washed it away in a few seconds and the whole field of view was again clear and
ready for new dissections. This method of underwater dissection was very convenient. If there was a membrane to be lifted, it was picked up with forceps and, by using an underwater (plankton) microscope with a magnification of 180 or even a little greater, the membrane could be pulled off carefully. By opening the forceps the membrane piece which was pulled off flowed away with the water. It was a pleasure to do dissection that way. It had the advantage too that there was no danger in dissecting an infected ear. With the drill it was very easy to expose one full turn on the tip of the cochlea allowing good observations of the basilar membrane.

The next question was whether we could make an opening in the cochlea without disturbing the vibrations of the basilar membrane. I spent too much time in developing an underwater glue which could fasten a window over the opening that I made in the cochlea. Later it turned out that the best way to fix a window on the cochlea opening is to use a highly viscous fluid, perhaps physiological solution with gelatin. Being highly viscous it is still moveable for DC displacement but not so for frequencies above 30 cycles per second, when it becomes almost completely rigid. Under stroboscopic illumination this could easily be checked.

In almost no time it was possible to show that there are traveling waves on the basilar membrane going from the stapes to the softer parts of the membrane. It is quite interesting that traveling waves were not readily accepted mainly because of the mathematics of the whole problem. Even today, most of the mathematical treatments of this problem have so many omissions and simplifications that they do not describe the movement of the basilar membrane properly. Even for me the traveling wave looked a little strange at the beginning, but as time went on it was obvious that whenever a system changes its mechanical properties continuously there is always a traveling wave. It is the only wave form by which energy is transmitted in systems with a lateral extension. Therefore, the traveling wave is the natural transmission form for the cochlea. After a while the theory and the whole measuring process were simplified so that by the simple means of testing the deformation of the basilar membrane under the DC pressure of a needle tip, it was possible to determine that the basilar membrane in the human cochlea should have vibrations corresponding to a traveling wave and not to resonance or other type of vibrations.

After having done measurements on the temporal bone of human cochleas, the question was whether these measurements were reliable. To answer this question it was necessary to make the vibration amplitude of the basilar membrane so large that we could see it with a magnification of 200 under a stereoscopic microscope with stroboscopic illumination. Once this was accomplished, the additional question was raised as to whether these vibrations would be the same as those in the basilar membrane if the amplitude was about 100,000 times smaller. This is the question of nonlinearity and it is quite clear that if we listen to a very weak tone at 1000 cycles, we can increase it almost 100,000 times and still the tone is unchanged. In all my measurements I never went to a higher amplitude of vibration than the vibrations of a pure tone. This can easily be checked if we keep the middle ear intact because the moment the amplitude goes higher we will have a tickling in our ear and a change in the vibration pattern in the middle ear. In general I used a sound
producer attached to a $T$ tube. On one side was the preparation and on the other side was my own ear. Since in my own ear I heard a pure tone for that amplitude, there was no good physical reason to assume that anything would be different in the preparation.

Another question was, is the elasticity of a living basilar membrane different from that of a basilar membrane without a blood supply? As far as I could see there was no real difference. I developed a very peculiar and very sensitive method of stroboscopic illumination which again did not measure the amplitude but rather the phase of the vibration. It could be shown that a vibration pattern measured in a live lightly anesthetized guinea pig did not change when the animal was killed by an overdose of pentobarbital or by inhalation of nitrogen. It was stated many times in the literature that the tissues change their physical properties in ten to twenty minutes after death, but I never could prove that. There is some change, for instance in the eardrum, produced by the stopping of the blood supply, but since the eardrum consists of three thin layers, when blood flow stops the humidity in the ear channel is immediately decreased and the eardrum starts to dry out.

If a patient is taken to an operating room, he is usually rolled into the room receiving a continuous intravenous perfusion of physiological solution. In animal surgery, this is seemingly seldom done and therefore in all animal experiments dehydration is a problem. The smaller the animal, the faster it dehydrates because the surface relative to the weight is increased. It is incredible how many severely dehydrated cats I have seen, with almost brittle tongues, on which records from the cortex and the inner ear were made.

In time, I came to the conclusion that the dehydrated cats and the application of Fourier analysis to hearing problems became more and more a handicap for research in hearing. Therefore, my interest went more into the psychological questions. I am very thankful to one otologist who, in cases in which the labyrinth had to be taken out because of disturbance in the vestibular organ, gave me a few minutes before the operation to test the mobility and the difference between DC displacement and vibration transmission in the middle ear on patients who were under anesthesia but whose blood supply, and therefore humidity, were normal.

At this point I had a well developed and productive laboratory. Unfortunately all my work was interrupted by World War II.

**World War II and After**

At the start of World War II our impression in Hungary was that a scientific laboratory would never be bombed by American airplanes. But it was not so: on the second day we found out that the American airplanes really did not hit a specific target at all. Instead, they used the tactic of carpet bombing, in which the leading airplane makes a circle and the following airplanes throw all their bombs inside that circle. The bombing was extremely inefficient; it killed people who had very little to do with the war and, in many cases, were definitely opposed to it.

A few days later a building near my lab and most of my equipment and writings were completely destroyed. It is interesting to note that the largest destruction was not done by the bomb itself but by the air suction produced by the explosion wave.
It pulled out the windows and everything in the cabinets and built it up into a mess which could not be separated.

The Russian attack came on the ground. They fought man against man. Toward the end, it seemed to be one German against eight Russians. Everything was depressing because there was no visible reason or logic in the whole behavior. The Russian Army worked like a machine; every morning at 7 o'clock they started with the Haubitzen to shoot and destroy one section after another very systematically. At 5 o'clock they stopped, had their supper, and the next morning they moved ahead again and went sometimes a few miles, sometimes only a few hundred feet. At the end the whole section under attack was destroyed, including the section where I lived, near the Danube. The highest wall left was about one meter high. I had many friends living in that section so I visited them before I decided to leave. I shouted their names under the blue sky but nobody came out so I went from one opening to another as I knew they had to come out for water. In some of these openings you could see the tragic history of Budapest. On the upper level was the modern type of buildings built in the nineteenth century, something of a modern empire style. One layer deeper you could see a clear empire style and going deeper in the layers there was the Baroque style in the staircases and cuttings of the stones on which the buildings and the cellar was based. If you went even deeper, there was a definite Gothic cutting of the stones and later a Romanesque style. It showed that Budapest had its history beginning from Rome up to modern times and it was destroyed several times during the last 2000 years. But every time it was built up again on the same place.

I have been asked several times why Hungarians are relatively successful compared with other people, especially in science. I have the impression that this sticking to one place and to one aim is the main reason why in the long run Hungary still produces important contributions to the culture of this world.

Since it was obvious that I would not be able to continue my scientific work, I decided to leave Hungary. Professor G. Holmgreen from Stockholm invited me to Sweden and from Sweden I went to Harvard where Professor S. S. Stevens took me into his laboratory.

Unfortunately, while at Harvard a great tragedy for my research occurred when the tower of Memorial Hall burned down. My entire working place in the basement of Memorial Hall was flooded and I lost the most cherished writings and old books, that I collected after I left Hungary.

I have found, on numerous occasions in my life, that it is impossible to rewrite the same idea the second time with the same freshness and logic as it was done the first time. After the fire in Memorial Hall some of my writings lost their precision because of the destruction of most of my data. To write a paper takes me, in general, one or two years. It takes almost one year to formulate the question that I would like to answer. It takes a half year to carry out the experiments and a half year to put it in the correct shape. I work on several problems simultaneously and the older I got the more problems I had to work on; this takes away the freshness of the logical buildup. Lately, the ordering of equipment, working on the financial aspects, getting an award from the funding agency, and receiving and setting up equipment in the
laboratory can take two or three years, even in the case of a simple experiment. This has completely changed my way of building up an experiment and my method of working, which was not to collect data but to collect different methods, and not to do every observation many times but to do the same observation with different equipment and different methods. This approach now has become almost impossible to carry out. In earlier times I had no difficulty in measuring, for instance, the elasticity of the basilar membrane with three or four different methods. In general I published one method which I figured out was the most simple and the most reliable one. It always amused me when later somebody picked from all the possible methods exactly that method I had found most unsatisfactory and introduced it as a totally new method.

Life in the United States of America

I really had very little information about the situation in the United States of America when I lived in Europe and a large part of the information was distorted. For instance, American films of good quality were so expensive that they did not reach Hungarian theaters. Those we saw were cowboy films and gave a relatively distorted picture compared with the German films which were excellent and of philosophical value. When I arrived in Sweden I was sure I would get good information about the US and I found the booklet given by the United States Army to their soldiers when they went to France. It contained instructions on how to behave when contacting French people and on the difference between a Frenchman and an American. The interesting thing was that at the end of this booklet was a chapter on how to survive in the United States. My first clear information came from that booklet. Unfortunately this information was also useless to me, being rules for survival of an American, not for a European who just arrived in the United States.

I could read English very well at that time, especially the technical language, but I could not speak the language at all and there were many incidents which were quite amusing, at least in looking back. The entry for a Hungarian into the United States was at that time quite difficult. Having arrived at La Guardia Airport after a long, long flight on a two-engine airplane with a long stopover at Labrador, I approached the officer of the Health Department. He looked at my passport and then he asked me if I was healthy and I told him no. He asked me again if I was healthy and again I told him no. Then he just put his hand on my shoulder, pushed me across the line, and I was in the United States. I was very much disturbed that already in the first seconds in the USA, I had probably made a very big mistake. My English-German dictionary soon made clear that I had mistaken the word healthy for wealthy. Since I had only $100 in my pocket and was thinking about the Rockefeller fortune, I was not able to tell him yes to his misunderstood question.

This small incident right at the beginning gave me an inferiority complex for which even today I am not really able to compensate. At Harvard University my colleagues were very friendly and helped me with my English. I learned from them good classical English. I still cannot write a good paper but I can judge if a paper is good or not. The secretary at the laboratory told me several times that I should
read Churchill's book on the English speaking people because it is written in beautiful English. I bought the book and I agree the English is just magnificent, but I did not like it for two reasons. One was the fact that it did not correspond exactly to the situations I had first-hand knowledge of and the other was that Churchill is a very poor painter. My general theory is that if a person is really bright and is good in his own field, he is bright in his amateur field also. It was a peculiar experience that a year later I was told to forget Churchill. I had an inkling that Churchill published at that time his controversial volume on the American revolution.

The American attitude toward science was very different from that to which I was accustomed in Europe. In Europe there was a certain pessimism about the degree of progress scientists can achieve, and there was an important difference in the role which financial support plays in new discoveries. In Europe you were born to be either an artist, a scientist, or a banker. You had to inherit the specific qualities which made you important in that field. In America everybody could learn to draw, everybody could make a million or lose it, and everybody could make excellent new discoveries. It was quite difficult to switch from one style to another style, especially after a certain age. My opinion was that both of these assumptions were extreme. The consequence of my optimism was incredible. Biophysics, which had been more biomechanics, became suddenly molecular physics, an absolutely new field with a tremendous potential for new ideas. Some old telephone experiments were developed into information theory. It was quite clear that in this field the optimism really paid off. But sometimes optimism went too far: a fund raiser told me that if he succeeded in raising $9 million, he would solve the cancer problem—that was about twenty years ago.

In the beginning the most interesting events of my life in the United States were trips to meetings. I met people whom I had known only from books and papers, and I saw the country and the large museums which contained collections I had never seen before. I soon enlarged my field of view and became very interested in the West Coast because it was so different from Boston. Eventually I decided to live in Hawaii and build a laboratory there. The decision was a good one and in the last six years we have made a few measurements which I think are new. Hawaii is basically just as different from the mainland as Europe is from the United States. The United States is very impressive for anyone who comes from Europe because it is so large and varied. In the United States you can have almost anything that you want, it is only a question of money. It was not so in Europe. In Hawaii you cannot have anything you want, but you can have things that you never expected before. Living in paradise, as it turns out, is not a very simple thing. It is so beautiful that there is really too much beauty and too much color. It is well known that the nervous system is more sensitive to variations than to continuous stimuli because of the role of adaptation. The same holds true for the life circumstances.

The University of Hawaii is a new university and therefore tradition does not play any role. This can produce many differences. For instance when I first came to

Hawaii I was very much surprised by the fact that between lectures the students walked on the grass of the campus and not on the beautifully planned roads. On the Harvard campus all the students walked on the roads, never on the grass. It took me a long time to explain this phenomenon. Obviously, the architect who designed the roads of the Hawaii campus did not know the different doors to the lecture halls, so the students had to make shortcuts. When I went back to Harvard I was told that the roads were there 200 years ago. Originally all the different houses on the campus were homes of professors and every professor owned a cow. These roads were selected by the cows when they went to and from the pasture. The cows had very good sense in making shortcuts, much better than the modern architects; and I have a very high respect for Harvard in that if something is good, they keep it, completely independent of who made it first.

Lately it seems that the different places in the United States are becoming more and more similar to each other, which makes life less and less interesting, and I have come to the conclusion that the most fascinating things today can be found mainly in museums. That is the one thing I enjoyed so much on the East Coast. The museums taught me in many ways how to look at the great diversity that human genius has produced during the past milleniums.