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A CONVERSATION WITH PROFESSOR W. G. CADY

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A transcription of a portion of a conversation between Professor Walter G. Cady and the author is presented. The conversation took place on September 28, 1972 at which time Professor Cady was almost 98 years old. Among the topics discussed were Professor Cady's education at Brown University and at the University of Berlin, his teaching at Wesleyan University, his early research in piezoelectricity, some of his hobbies, and his teaching philosophy.

On September 28, 1972, this writer had the privilege of talking with Professor Walter Guyton Cady for about half a day. The meeting was in the home in which Professor Cady was born almost ninety-eight years before at 127 Power Street, Providence, Rhode Island. The conversation covered a broad spectrum of topics, from stories of his childhood and his early education to discussions of his interests in modern physics and his teaching philosophy. Despite his age, Professor Cady described his life with clarity of memory, with sparkling humor, and with humility and modesty. The conversation was tape-recorded by this author,[1] and from a transcription, a portion was abridged. Some rearrangement of the order of topics was made, but the text below is essentially a literal transcription of those portions of the discussion.

HOBBIES AND SCIENTIFIC DIVERSIONS

The conversation opened on the topic of some of Professor Cady's hobbies.

CADY For a long time in my experimental work, I was doing researches into ultrasonic regions and wearing headphones on my head for sometimes hours a day. About that time I began to be deaf. And it's been getting worse and worse ever since.

LANG You said that you had perfect pitch, that you could actually imagine a particular note and match them perfectly.

C. Oh, yes. Well, a good many people have that faculty of absolute pitch. Hearing a note and being able to say a C, a D, or what. I don't know if it's native with me or not. It came anyway. I acquired the ability by listening to bird songs. I have a notebook full of songs of birds. I used to carry a pitch pipe around with me, and I observed by means of the pipe, the absolute pitch. And that is all that it was.

L. Were you actually able to write in musical notes the bird calls?

C. Oh, yes. I've got a lot of those. I've got a bird book full of American birds, and, while I was living in Germany, I made a similar record of a lot of the German birds. They have other ways of doing it now in print. There are special representations of bird songs, not in musical notation, but by curves made automatically, you know. So my things are now pretty much obsolete. I had a lot of fun doing it anyway, and, I got to the point where I could tell what the pitch was by simply moving the little pitch pipe I had toward my ear, and, in actually doing it, I acquired the ability to tell what the note was going to be. It had a C on one side and A on the other and you could get D out of the two notes by blowing on the right side. When I first began it, I had to blow on each side in order to tell what was C and what was A, and after a time, I came to the point that I knew before I blew what it was going to be.

L. That's amazing.

C. I think it's a common enough trait, only I had great fun in developing it.

L. Could the songs be reproduced by some sort of instrument afterwards?

C. Oh, no. I had no way of reproducing it afterwards. I composed one or two tunes of music based on bird songs though. Well, I didn't mean to use up time on this.

L. No, no, this is interesting. You played various musical instruments at one time. You played the violin.

C. Yes, and I played a little piano and never followed it up. I played violin, and then, that violin playing helped me play a little on the mandolin. When I entered Brown, they wanted me to join the mandolin

club because they needed some first mandolins and the fingering, of course, on the mandolin is exactly the same as on the violin, so all I had to do was, instead of bowing, to use the pick. I joined the club there. Well, I think that the old fashioned mandolin clubs they used to have at colleges, if not entirely extinct, are pretty close to it these days.

L. What kind of music did you play? Was it classical music or folk songs?

C. They were Spanish songs. There was a Spaniard who came to this country, Senor Romero, and he instructed Harvard's mandolin club, and we got him to come and teach the Brown mandolin club. He showed the mandolin players how to use the picks and taught the guitar.

L. Then this playing of the mandolin as he instructed you is like the classical Spanish guitar?

C. Well, the tuning of the strings is quite different. The method of playing is different. The guitar is more for the accompaniment. The melody is carried by the mandolins. Mr. Romero came here and taught the club modern Spanish music which was the basis of our programs, mostly light dance music and that sort of thing. And, then, I arranged some music a little more classical like Saint Saens and we played that at the club. So, we had quite a varied program. They have given up that sort of thing pretty much all over the country, except for some classical medieval music which seems to be going over. Nobody has as much fun out of it as we did with our old simple Spanish songs.

L. You travelled around with a small group?

C. Well, we used to make summer trips and we went way down in Georgia, for example, giving concerts on the way. My club went up into Maine and we played in Bar Harbor and all the places around there. I was the leader of the club in those days.

L. How many people did you have travelling around?

C. There were about 10 to 12, I think. Some of them were mandolins, some guitars, and I introduced a cello into the club. We also had a violinist in the club. The violinist, incidentally, was John Rockefeller. He was the father of Nelson, I think.

L. Nelson is the present governor of New York. (Now Vice President of the United States).

C. And John played violin and played very well. His appearance was very much like his descendants. He tended to be short and stout, quite a pleasant social fellow. He was at the house many times. He married a Providence girl. That was Abby Aldrich. She was the daughter of Nelson Aldrich and that's why Nelson was named Nelson Rockefeller.

L. I also read your paper on the circular tractrix (Ref. 2). I thought that was a lot of fun.

C. Oh yes. That was just sort of an amusement.

L. Have you done a lot of that sort of thing?

C. As I look back through my long life, I've come to the conclusion that I've wasted a great deal of time on side issues, trivialities; that was one of them. If all the time I spent recording bird songs, I had stuck to physics, I might have gotten farther along. One of the great problems in life, you know, is learning how to distribute your time. How much to spend on work and how much on play.

L. But you've accomplished so much.

C. Would you like to take a look at that thing (circular tractrix device)?

L. Certainly.

(Professor Cady went upstairs and returned with th circular tractrix device, which he proceeded to demonstrate).

C. I got the idea when I was in England. I think I mentioned that in the article, didn't I? That I was with a group there at Cambridge, at the University, and we were sitting at a table. It was a round table and I was playing with my fork. Very unmannerly for England! And, thinking of that curve that you got from the tip of the pencil when you drew it out this way or that. That was a familiar curve. I think it goes way back to Isaac Newton. His was a case where you had a straight line and you dragged the end of this pencil to the right or left and you got that tractrix form. Then it occurred to me, here we are at a round table. Now suppose I just traced this in a circle like th instead of out on a straight line. What kind of a curve would the pencil draw? I left that idea lying around in my mind. After a while I spent a lot of time working out the equation. I think the equation interested me more than the apparatus, and it wasn't until I'd writte it up and was about ready to publish it, that I discovered that these were all old ideas, a couple of centuries old.

L. You could probably make a little child's toy out of this thing as well.

C. You can get some pretty pictures out of it.

L. I remember in the paper, the ratio of the arms determines the type of curve that you would get.

C. Well, yes. The equation that I wrote up tells all about that. I don't believe anybody else has ever taker the trouble to make one. I told you the original idea was old in the literature. The literature didn't say anything about a machine for drawing the curves. It only gave the equation. So this is the first machine for drawing such curves, I think, that ever was made. I took this up as a pastime. It was a pastime that had a close connection with physics, of course.

BROWN UNIVERSITY

Professor Cady entered Brown University in Providence in 1891 and received his Bachelor of Philosophy Degree in 1895 and his Master of Arts Degree in 1896.

LANG. What encouraged you to take up science?

CADY. Well, I was always interested in tools and making things and so on. And, I can remember when I was just a small kid hearing somebody in the family saying, that boy is going to be an engineer. Well, I wasn't quite sure what an engineer was, but I had always kept up the interest in that. At Brown I wasn't quite sure whether I wanted to be an engineer or a physicist, but in my senior year Professor (Carl) Barus came and I took a course under him and he showed some interest in me and advised me to go to Germany to get my Doctor's degree. He was a native German and had acquaintances over there, so he gave me quite a batch of letters of introduction to professors in Berlin and I went over there armed with those things and stayed for five semesters.

L. Barus was from Berlin?

C. I don't know whether Barus was from Berlin or not, but he had a lot of acquaintances in Germany anyway and he was well known to the German physicists. He made quite a reputation for himself. So I went and took my degree and, incidentally, became engaged and married while I was over there too.

L. Your wife was an American girl, wasn't she?

C. Yes, it was an American family. Her father was an Ohio boy. He graduated from the University of Michigan, went into dentistry, and went abroad. When he first went abroad as a boy as I did, he had the idea of being a physicist himself. But in order to support himself, he got a job as an assistant to a dentist and changed his plans and went into dentistry and married a dentist's daughter. Later young Miller became Professor of Dentistry at the University of Berlin. And, I think he was the only American ever to hold such a position as that.

L. You did some work under Professor Blake at Brown. You took a course in optics?

C. Yes, that was the only course I took under him and it switched me over from the idea of engineering to physics. I elected his course in optics. I think that was during my junior or senior year. Anyway, I was the only one in the class, so, to a large part, instead of my sitting in front of him and his lecturing, he just took me around his laboratory and showed me the things in the apparatus cases, got out something or other and demonstrated it, you know, and I found that very fascinating. He had a very nice personality, and seemed to take an interest in me. So I enjoyed my course under him so much that I decided to stay in physics. Then, when Blake died and Barus came, I continued and took another course under Barus.

L. Was it a common thing for a professor to teach a class for one student?

C. I don't believe so. I don't know whether Blake would have ordinarily decided not to give the course if he had only one student electing or if he just happened to know that I was especially interested in physics and just decided that he would take me on. Anyway, he did.

L. Were the classes always relatively small though?

C. The number of students was small anyway in those days and the number of students taking physics was very small indeed. There were two who took the Master's degree under Barus. There was Fred Clapp whose father was, I think, city engineer in those days. Fred and I went through the old English and Classical School together and then we went to college together and remained friends. Fred also took courses in physics under Barus. I don't think he did especially well in them. Anyway, Barus told me one time that he was going to give the Master's degree to Clapp and me. He said he was giving it to Clapp because he was a good fellow. So that shows that professors weren't always too strict in their rulings.

L. Was there a very formal program set up for the Master's degree? Did the students have to take so many courses?

C. Well, there were certain courses that we were expected to take and also do some sort of little job like a Doctor's thesis on a small scale and he gave me a little piece of experimental work to do. It had to do with a spinning top. Barus had the idea of making a top with a peg that was flattened off at the bottom so that the part that rested on the table was not a point, but a circle, and that made the top gyrate around this way and he wanted to have an experimental study of that made. So he picked me to do that job. As far as I can remember, it never got published anywhere.

L. How long did it usually take for a Master's program?

C. Those students who wanted to take a Master's degree at Brown usually spent two years in graduate work. I don't remember how it happened that I took my degree in one year. But, I stayed on a second year after that, so I really did two years doing graduate work. But, I was so much interested in Barus and working for him.

L. Did you do some teaching at this time as well?

C. Yes, we all did. The graduate students were expected to teach classes. I remember a course in trigonometry that I gave down in old Rhode Hall, I think it was. I was just out of college myself then, and I graduated fairly young. Some of them were Providence boys whom I'd always known. Some of the members in the class called me Professor and others called me Walter.

L. Do you think that you acquired the love for teaching through the teaching experience then?

C. I don't think I became interested in teaching that way, but all the boys that went into science and took Doctor's degrees here expected to teach. They didn't have the big industries in this country that employed scores of Ph.D.'s. A Ph.D.'s position meant a teaching position in a college.

L. Did the students work harder in those days than they seem to today?

C. I don't think I have the data to answer that question.

L. How did they compare with the ones you had at Wesleyan?

C. One thing that we did not have is the student demonstrations they have today. Another thing they did not have is the idea of getting students who were not suited, to take part in planning courses and into college administration and such things. I think that we ourselves would have admitted in those days that we were totally unqualified for such things, and it would be foolish to appoint us. But they don't feel that way now any longer.

L. Did the graduate students work long hours? Would the students ever be found late at night?

C. Yes, graduate students. Well, I think we all took our work pretty seriously and we worked pretty hard at it.

L. Were you paid for the teaching that you did?

C. I don't think so. I figured it was just part of our duty.

L. Students weren't supported by grants at that time?

C. I don't think that they had any extra pay for that. On the contrary, we must have paid some fee for taking the course.

UNIVERSITY OF BERLIN

Professor Cady began his Doctoral studies at the University of Berlin in 1897 and received his Ph.D. degree in 1900.

LANG You once said that your "main regret is not having in your youth the discretion and judgment in choice of problem and the method of handling it". In a sense though, do you think that if one is a little too careful in that respect, one might miss something of great significance?

CADY Well, when you're young, things are likely to be very different from what they are when you get more mature and there's a great danger of spending lots of time on the wrong thing.

L. I guess that's true, but. . . .

C. I took two years of mathematics in order to be prepared for my mathematical examinations for my Doctor's degree. That was on elliptic functions. I would find it difficult to say what an elliptic function is about now, after two years of studying it there. I think it's partly because the professor himself delighted in details of elliptic functions, but without ever getting out of the subject what it was all about and what we were expected to use it for. Anyway, I passed the examination under him. He was an interesting old chap, named Professor (H. A.) Schwartz, with a long beard and a shirt that kept sticking outside of his vest. I went to him and told him that my examination had been set for a certain day and would he be good enough to be my examiner in mathematics. And he said, on that day it wasn't convenient for him to come into town. He lived out in the surburbs. But he said, "if you'll come out to my home, I'll examine you there". So I did on that certain afternoon. I went over to his house and I met Mrs. Schwartz and they served tea and we had a nice social time together. And then he finally got around to asking questions. It wouldn't have been polite for him to have flunked me after all that. I passed.

L. These exams were always oral?

C. It was oral in mathematics. I don't know whether under ordinary circumstances I would have been expected to do some writing or not. I think that they were all oral; physics was certainly all oral.

L. I was interested by the story that you were also examined in philosophy.

C. Oh yes. That's the way it had to be in Berlin in those days. We had to have four subjects, you see, physics, philosophy, chemistry and mathematics. So I had (Wilhelm) Dilthey, I think that was the name of the man in philosophy, and he asked me questions. I thought he smiled rather sarcastically at the naivety of some of my answers, but I think that there was a kind of gentlemen's understanding that every student who took his examinations in philosophy would pass, unless he showed himself to be atrociously ignorant.

L. Were these things done in such a way that you would sit down with the examiner?

C. Oh, yes. We would sit at one side of the table

and the examiner was at the other. If it was a big room, there would be several such tables where professors and students sat, taking their examinations at the same time. Warburg, in physics, just asked me questions which I answered. I felt at the time that he gave me a very easy examination.

L. Had you already completed your research?

C. My thesis was completed. It had been accepted. The thesis had to be accepted before the examination could go on, and I think the examination in physics was largely, if not mostly, on the thesis. I think he did ask a few questions in fundamental physics to make sure I remembered that. As I look back on it, if I had been examined at Columbia, let's say, the physics examination would have been a great deal stiffer than that one was in Berlin.

L. But you were always examined by your own advisor? Warburg was your research advisor?

C. Well, my personal advisor was Kaufmann. He was at Berlin and he made quite a name for himself. He was, I think, just an instructor or an assistant professor when I was there. Anyway, he was better informed on the details of my work and he visited me more than Warburg himself did. As I say, it was lucky for me that Warburg asked me simple questions.

L. Then you had an examination in chemistry?

C. Yes.

L. Who was the examiner there?

C. (Hans) Landolt. Somebody who had taken the examination recently said, "be prepared to answer questions on alcohol because he always asks questions about that".

L. What were the courses like?

C. You didn't have to attend courses. Nobody took attendance. Strangers could wander into the room if they wanted to and sit there. My brother visited me while I was in Berlin, and I took him to the lecture room and to a few of the courses and let him sit by me there, and nobody paid any attention.

L. Then there were no examinations during the individual courses?

C. No. I don't remember any examinations at all taken during the class sessions, no. But the professor used to give out homework and you could do it or not as you pleased. I took (Max) Planck's course. And he gave out homework and I did some of it and handed it in. I didn't follow the whole course through.

L. So, you were really very much on your own when you took the courses.

C. Almost all your preparation for the examinations could be done personally at home, if you chose to do it that way. If the professor had published a book on the same subject that he lectured on, for instance, you could buy his book and study it. I would have listened to Planck a great deal more, I suppose, if I had realized what an international celebrity he was going to be one of those days.

L. He wasn't known at the time?

C. Oh yes. Many of the students who were taking courses under him told me that they considered him a bigger man than Warburg, who was the head of the Physics Department. He was recognized as a prominent physicist. I think that I put in my book that I had a word of personal praise from him (Planck) one day.

L. I understand there really was a less individual kind of system in the United States than in Germany.

C. I think that there were probably more personal relations between the students and faculty there than in the U.S. We had physics colloquiums there, or whatever you called them, once a week or so. The faculty and students got together and they'd have, usually, one or two papers or talks by members of the faculty on what was being done, or sometimes reports on what was being done elsewhere. Local advances in physics. I think, for example, that it was at one of those general physics colloquiums at Berlin, that I first heard of the X-rays that had just been discovered. One of the professors, who had been in touch with Roentgen gave a little sketchy report of his new discovery.

L. So things in physics, in modern physics, were just beginning to happen at that time.

C. Oh yes. It was an interesting time. It was just in those years that Planck was developing his theory, of course. But, he didn't say anything about it in his classes to us. I didn't know what was going to happen and it was about the time that I finished and came home, that his first epoch-making paper on quanta came out. I think they thought he was crazy at first. But he won his points. If you look back on it now, it seems as if somebody in those days might have anticipated that there would be such things as quanta. Planck's theory had to do with light, light waves that were thought at that time to be of all possible frequencies distributed impartially. What he showed was that it wasn't so, that they come in certain bunches, you know. Well, now all of that was due to vibrations of or in atoms and you would express an atom as if it were a vibrating system with a certain particular frequency of its own. So there was a finite number of hidden atoms in a box and each one has to vibrate with its own particular tune like a violin that had been tuned to a certain pitch. Why shouldn't it be that there are only certain discreet frequencies in the box? No one ever thought of it that way before.

L. Did most of the American students at that time go to Europe to study?

C. A good many did anyway. Barus evidently thought that it would be wiser for me to go to Germany to get my degree than to Columbia, for example, or Cornell. I think I could have gotten a Doctor's degree at Brown, but Barus thought that I ought to be where there would be more doing in physics and where I could get more experience.

L. You must have acquired a good knowledge of German very quickly there.

C. I started taking notes in English, and then, gradually introduced German words and the percentage of German increased as the months went by, and finally, it was pretty nearly 100% German after a while.

L. I guess that Berlin was a very exciting cultural city at that time.

C. Oh yes. It was a great center of opera. There were very many good Berlin opera companies and a symphony orchestra. I was interested in music, so I used to go very often to those things. Saw plays. They gave translations of Shakespeare in German. I remember seeing a performance of Hamlet once. The Germans stood up for the German language. They said German was much more beautiful than English. The "To be or not to be", how much more beautiful it is to say, "Sein oder nicht sein".

L. You travelled around quite a bit when you were in Europe?

C. Oh yes. I took vacations there. Some we took in the country regions and made one long trip down as far as Venice and back.

L. You did a lot of cycling too?

C. Yes, some of them were bicycle trips; some were tramping.

L. Did you do mountain climbing too?

C. Off in the northeastern part of Germany in the mountains. My roommate and I, on a particular trip, thought we'd like to climb to the top of the highest mountain in winter and see what it was like. We never got there; we got partway up, spent the night at an inn, and then had to climb down again disgloriously. We took the train back to Berlin and as we looked back across the landscape, the mountain against the beautiful blue sky was visible. First time we'd really seen it. That's picturesque country.

WESLEYAN UNIVERSITY AND TEACHING PHILOSOPHY

The years from 1902 to 1951 were spent at Wesleyan University.

CADY. By the time I went to Wesleyan, I had taken my degree in Berlin, came back from there, and spent two years on the (U.S.) Coast (and Geodetic) Survey in magnetic work. And then, they tried to keep me down there and continue in government work, but I decided I'd rather have a college position. I wrote letters to, I think, six different colleges to see if they had any openings in physics, and so it happened that there was one at Wesleyan. They didn't give a Doctor's degree at Wesleyan then, but they did give a Master's degree. So, I started in there teaching and had, I think, from the start, one of graduate students. It worked liked this. We had to employ one or two assistants who were recent graduates, sometimes Wesleyan graduates, sometimes from other colleges, and we employed these assistants to help us with the demonstrations and to supervise laboratory work and so forth. And, at the same time, gave them an opportunity to work for a Master's degree. It's only within the last few years that Wesleyan's given the Doctor's degree, but they are giving it now. They didn't at all when I was there.

LANG. Did you have a graduate student working for you at all times during your career there?

C. Oh yes. We always had one and sometimes two or three assistants in the department. The number increased as the college increased in size. When I went there, there were only two teachers of physics, two professors.

L. Besides yourself?

C. No, including. There was old Professor Crawford. He was an old Wesleyan graduate and had been teaching physics for many years. He was one of the first to use X-rays in this country, incidentally. And so he was the professor and I was an instructor in physics. They made me an assistant professor after a few years, and then, finally, full professor. So there were two professors in the department, and then, as I said, the college grew and we needed more teachers, so we began taking on more instructors or assistant professors.

L. Did you enjoy very much teaching at a relatively small school?

C. It's pretty hard to say whether I enjoyed more the teaching or the research. I think one of the sad things is the great number of people who enjoy research in the different departments of science, but who do not enjoy teaching. It's kind of a sacred duty to become a good teacher, if you're going to do anything in science.

L. There was something that you wrote: "If I had my life to live over, I would want to spend it again in a small college where recompense is low, but where one can maintain one's freedom and dignity".

C. Yes. I see in the physics journals articles written by perhaps eight or ten different co-authors, you know. You don't know which is the leading spirit of them all. You don't know which one may have just set up a galvanometer or so and did enough to deserve to be included in the list of authors. I suppose the peculiarity on my part was that I always had a kind of revulsion at the idea of working in a chain gang, which it seems like, working in a crowd like that. And, yet, considering the nature of the work, it isn't a thing that can be done by one man. Modern research in physics requires cooperation. It requires different techniques and knowledge of special branches of physics, cooperating together so it would be quite impossible to carry on any program in modern physics single-handed, except in rare cases perhaps.

PIEZOELECTRICITY

Professor Cady described his early years in piezoelectricity research.

LANG I'm quite interested in how you first got into the piezoelectric work. You really didn't look into it at all when you were at Berlin?

CADY No, my thesis at the University of Berlin had to do with cathode rays. The phenomenon of piezoelectricity had been discovered before that, but no one had paid any attention to it and there didn't seem to be any applications of it. When I came back to this country, I had my job at Wesleyan and I hadn't been there very long before there was a convocation called in Washington (1917). I was present and there, we were told of what (Paul) Langevin had been doing in France in making underwater signals by means of quartz-steel sandwiches. When they invited me down to Washington, I didn't have much of a reputation in physics, but it so happened that I had spent a couple of years on the magnetic survey in Washington under Bauer. Bauer, who was fairly well known among physicists in those days, visited me in Middletown at about that time and I told him of some thoughts that I had been giving to possible magnetic methods for detecting submarines under water. Then came the call for physicists from all over the country. Bauer remembered his conversation with me and he told the people who were organizing that meeting, that Cady had been giving some thoughts to this in Middletown, and they'd better invite him to come to that meeting. It was there that I became interested in crystals and that has riveted my attention more or less ever since.

L. So when you went to the meeting you were really thinking of the magnetic approach.

C. That's right. (Ernest) Rutherford was at the meeting among others. And Mesny and Jouaust from France. And somebody from Italy; I don't remember who. Anyway, they told us about what Langevin had been doing in France, and we were all asked to interest ourselves in some phase of that work and try to help out in the improvement of underwater devices for detecting submarines, because the need was very critical at that time and lots of ships were being sunk. So I came back to Middletown and decided that's what I wanted to work on. Somehow, Bumsted who was professor of physics at Yale at that time, learned that I was getting interested in this, and he asked if I would join with a group at Schenectady, I think it was. And, I went down there and found that Wills and Morecroft were also starting in the same field. So we pooled our resources; we worked together. I worked on detectors and Wills worked on the transmitters, and together we made a trip down to Key West to the Navy Yard, where the first tests were made of that. I had a lot of fun out of that.

L. You had had no background in crystallography or in crystal physics at all prior to that?

C. Oh, I had to learn the whole thing from the ground up. Well, that happens to lots of people who become interested in some new thing. They have to acquire a background the best way they can.

L. You were already in your 40's at that time, I guess.

C. Oh yes. I was born in 1874 and it was (before) 1924. Yes, I was pretty nearly 50 when I did this crystal work. I was no infant prodigy.

L. Did you know any of the people who worked on piezoelectricity in the early days, like the Curies?

C. No. They didn't do any work on vibrating materials. Their work was all static, you know. I don't know any continuing uses that were made of deformed crystals until the war came and Langevin had conceived the idea of using them for sending out signals underwater. I meet Langevin there for the first time (in France on Professor Cady's 1923 trip to Europe). He invited me to his home. And, when I called on him and told him about my work he said, "Oh, I never thought of making the crystals vibrate". If Langevin had thought of that instead of submarine detection, he would have been the initiator of the crystal-controlled oscillator.

L. So it's just a matter of timing.

C. It's just a matter of luck.

L. I guess luck is a very determining factor; you choose to do one kind of experiment rather than another.

C. Yes, the queen of luck is a periodic phenomenon in itself. What I'm pretty sure of is, that if I hadn't

done that work that I did around the early 30's, it wouldn't have been long before somebody else would (have). It was bound to come sooner or later anyway, and I just happened to get there first . . . There are other things that are receiving a lot of attention nowadays, as you probably know. Surface effects. Waves that travel in a thin film on the surface of the crystal and don't penetrate. The physics of surfaces is a special branch by itself, that has come to prominence lately. Molecules are under a different balance of forces when they're in contact with the body of the crystal on one side and air or a vacuum on the other. And, that's how it happens that these little wavelets can travel and they've turned out to be very important. Important because of the importance of high frequency. You see, they're getting far up into the millions and billions of vibrations now, because so many different messages can be packed into a small space that way.

L. You were the chairman of the Crystal Committee of the Physics Division of the National Research Council.

C. I don't think that they ever had many meetings, but I had that title then. At that time, they couldn't forsee what subjects would have a lot of meetings and which ones would depend more upon individual efforts. There were meetings in Washington in those days, and I don't remember who it was that called them. I think that someone at the National Bureau of Standards did. I remember one meeting where they were talking about the name to give the different cuts of crystals. They called one the Curie cut and one was—oh, it had some other name of that sort. Well, I thought I would have a name for a cut that would apply not only to quartz, but to any crystal. And, I think I was the first one who suggested that they speak of an X cut or a Y cut or a Z cut, meaning a cut across that particular axis and this has been standard ever since.

L. How did the terminology for some of the other cuts, the AT cut arise? Was there any logic to that?

C. Well, I think some of those cuts were purely arbitrary, just for convenience. As I remember the AT didn't have any meaning. The AT may have stood for transverse or some such thing as that. I'm not sure now.

L. How would you orient the crystals?

C. The orientation of the quartz is definite enough according to its natural crystal axes. It was way back, many, many years ago, that the, main axis of the crystal was called the Z axis and the others the X and Y axes. But, I think that some crystallographers called them ABC instead. Finally, everybody settled down on X, Y, and Z. And, then the oblique cuts came in and they used different letters of the alphabet to designate those. What they called the Curie cut was the cut that the Curies made in their tests and that was a cut perpendicular to X, the so-called X axis. And the X axis was already well defined by crystallographers.

L. What did you use for cutting the quartz? What kind of cutting tools?

C. Diamond saw. A thin disk of metal with diamond dust impregnated all around. I think some of those saws dipped down into diamond dust and picked it up and carried it around and cut through the quartz. A good deal of it got rubbed off on the quartz, and then it picked up a little more when it got down there (into the diamond dust again). Rochelle salt is soluble in water and one way of cutting Rochelle salt is, instead of by a metal saw, to use a wet thread kept in continuous motion. The idea for that came from a student of mine at Wesleyan. He saw me working on the cutting of that salt with a metal saw which heated the crystal to an undesirable extent and he said, "why don't you use wet threads?". I think I gave him credit in my book.

L. I understand Dr. Hans Jaffe worked with you in the 1930's?

C. Yes, Hans used to be my assistant. I was very lucky to have him. He was a refugee from Germany and had specialized in the very field that I wanted a man to work in. And, through some agency in this country, I learned about him and engaged him, took him on and he came as an assistant. I think we made him assistant professor before we got through; I'm not sure now. Anyway, he's a very able boy and has a responsible position at Gould (formerly Clevite).

L. Was it your employing him that got him into the country? I know a lot of refugees had a difficult time.

C. There was great persecution you know. A great number of Jews were coming to this country and there were agencies here that were finding positions for them. Incidentally, Hans was here with his wife to call on me not so many months ago.

L. You keep in close contact with him? Do you keep up with the work that he's doing?

C. Well, I try to keep acquainted with the field. Incidentally, I have a patent in that general field that isn't yet issued and I've been working on that for a long time (Ref. 3).

L. This is the most recent thing that you've been doing?

C. This patent has to do with detecting vibrations. There've been a lot of patents issued in that field using different principles of physics for detecting and measuring and recording such vibrations on airplanes, for example. You want to know what different parts of airplanes are vibrating and what direction they're vibrating in. So, I have little things that can be put

up against the material and be set into vibration and they can make an electrical record of it. And, my idea was using crystals for that purpose.

L. Did you actually do some work on it recently, some experimental work, or is this based on something you've done a few years ago?

C. Almost all that I did was theoretical. I made a few experimental tests, made some here at Brown, incidentally, up at the Thayer Street laboratory there. That's the laboratory on the West Side.

L. How long ago was it?

C. Five or six years. The application was made several years ago and then they said that this other patent had covered somewhat of the same field and some of my claims have had to be rewritten. It's all in the hands of my patent lawyer who is in Boston. So we've got correspondence back and forth between Washington and Boston and Providence and I've got a sheaf a foot high of papers and things connected with that, that transpired during these last years. The end hasn't come quite yet, though I hope it will, very soon. Then there is an article I want to write on the same subject for one of the electrical journals and that's still in the works. So what I'm hoping is that I'll last long enough to get those things done. (Also) I wish I had a little more time to develop some ideas that I've had on the philosophy of physics, but since I had this stroke a year ago, I haven't been able to work more than an hour or so a day, anyway. So I've had to put all such ambitious ideas aside.

EPILOGUE

In June of 1973, this writer sent a copy of the transcription of the tape recordings to Professor Cady for his comments. On July 6 he returned the transcription with a letter he had typed himself and the comment, "As you will see, I have made numerous small corrections, mostly in the English. I didn't try to do an absolutely complete job in this, since I assume that the stuff will not be published". Professor Cady's modesty was well expressed in the preface of a manuscript entitled *Saving Ancestors* that he wrote for his grandchildren in 1963: "My only excuse for saying so much about myself is that I happen to be the ancestor of whom I know most".

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2. W. G. Cady, "The circular tractrix", *American Mathematical Monthly*, 72, 1065 (1965).
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