Interview with Professor David Dennison

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Pioneer High School

INTERVIEW: DAVID DENNISON

One is easily awed by greatness. We were so awed speaking with a man who epitomizes greatness in his field, Professor David M. Dennison of the University of Michigan.

He seems to contradict the image one might have of a retired professor; He is tall, lean and healthy looking with a shock of neatly combed grey hair gracing his forehead. He reminds one instantly of Henry Fonda, with his firm jaw and even row of teeth. He has a hearty, resonant voice with just a trace of his 74 years in it. We found him extremely articulate and witty while talking with us.

Born April 26, 1900 in Oberlin,Ohio, the son of a classics professor, young Dennison recieved his bachelors degree in 1921 from Swarthmore College. He did his thesis on methane gas at the University of Michigan, and received his PhD in 1924. He was on a travelling fellowship from 1924 through 1926, when he went to the Continent and worked at some length with Neils Bohr at the Institute for Theoretical Physics in Copenhagen. He became a member of the Michigan faculty in 1927, and retired in 1970.

This list of his many great scientific discoveries and break-throughs are almost too numerous to mention: He discovered the spin of the proton in 1927 in a brilliant explanation of the anomalous behavior of the specific heat of molecular hydrogen. This is such a basic of science today that Physics Today says:"The present generation of young physicists may be suprised to learn that the proton spin had to be discovered at all." In 1934 his theoretical predictions on molecular structure led to the creation of a whole new field of study; microwave spectroscopy. He has done an enormous amount of work dealing with polyatomic molecules and infrared spectra. He worked with the proximity fuse during World War II, and he has also worked extensivly in the theory of particle acceleration.

He now resides with his wife in a beautiful contemporary home on a rolling estate on Ann Arbor's north east side. It was here that we visited and talked with him for some two hours. He was very cordial and a perfect host, quite willing to help us in any way. We sat on a couch across a table from him, where he was relaxing in a living room easy chair. At first he seemed concerned about answering the specific questions we asked, but as the discussion progressed he began expanding on the general topics that we tossed his way, and became a truly delightful interviewee.

He was modest to the point of hardly even admitting to us how really important his discoveries and experiments were. For example, when we pressed, he did admit that, well, yes, his ammonia inversion theory had been the beginnings of microwave spectroscopy after all, and not "a discovery of little practical application" as he had modestly stated earlier.

We undoubtedly could have kept talking to him for hours, as he is an endless storehouse of knowledge and ideas. We felt, when we were through that night, that we had been talking with a giant.

CZ: What was the atmosphere like in Copenhagen working with the great minds of the day as compared to the atmosphere that prevails today at the university?

Dennison: Well, for one thing, physics, and probably many other sciences, but particularly physics, was not what you would call big business in any sense. It was a very beautiful, intellectual pursuit, occasionally paying off with something of practical interest, but I don't think this was the special thing that lured us into it. We simply enjoyed it.

CZ: Were they any more strict or formal than they are today? There is a lot of free rapport between the student and the teachers, say at the graduate level today. How was it then?

Dennison: Well, this will take us back to where I did my graduate work, here at the U. of M. I would say that in many respects far less formal. I tried to get out of all the classes

I could. This seems quite normal doesn't it? It seems to me that in graduate work I probably only took maybe on the order of three or four formal graduate courses and there were some things, for example, advanced electricity and magnetism, that wasn't given in the year I wanted to have it, so I simply read books and prepared to take the examinations. Actually I don't think any particular courses, though, were necessary. I'm not absolutely sure whether they are legally necessary today. We have a certain schedule of courses that students take but I think that in general the students think, "Well, I know this subject well enough. It'll never make sense to take this course."

CZ: I noticed the strict formality of the letters that you quoted in your talk between you and Professor Bohr.

Dennison: Oh, yes, well , of course,this was a little bit different. I mean, this was the old world now, you see. I had been distinguishing between my particular training here for a PhD. and then what happened after that. So, in my graduate work, I would say it was at least as informal as now. There were several courses that I took in which I was the only student. I had to sort of prevail on the teacher to give me the time to do it, but I did. So, as you can see, this was really quite informal. Now in Copenhagen, of course, there were people that were first of all very, very clever; they were the best physicists in the world.

CZ: Who were they specifically?

Dennison: Well, of course, Neils Bohr was the head of that laboratory, the Institute for Theoretical Physics; Wolfgang Pauli; Werner Heisenberg. This was at the beginning. Kramers, Klein; a Japanese, Nishina. Then later Dirac came. These were, however, very, very clever people. Now some would just visit for a short time, but most of those that I've spoken of stayed in Copenhagen for considerable periods of time, three or four months at a time, and they would be working on problems and they were really very clever guys so that in a sense we were up against stiff competition. In that sense, I don't know whether you would call it a formal relationship. It was just that you were thrown in with people who were really very clever and if you weren't careful they would find out in five minutes what would take you five days to find out. I don't know if this answered your question.

CZ: In those days Europe was considered the hub of science as America is now considered the mecca of science. Do you think there was anything significant about you being an American going over there and was their attitude toward you condescending?

Dennison: Very, very nice. In general English was the language spoken in the laboratory, although there were many nationalities, of course, Danes, Dutch, English, German. I don't believe during the time that I was there any Italians came. That was a little bit later. There was a Swiss who was there on about the same footing that I was. An Indian from India, several Japanese. So it was quite international in a way. Although a fairly small laboratory, you know. It wasn't very large. You've probably seen pictures of it. It wasn't a very big laboratory.

CZ: What were your basic beginnings in science as a teenager, and who was it that pushed you in that direction?

Dennison: Well, this is a little bit hard to be quite sure. In a way, looking back on it, it seems to me now that I always was a physicist. But, in a way, perhaps one thing that may have influenced me—my father was a professor of classics. We went to Swarthmore and took on the chairmanship of the Classics Department there in 1910. And I was ten years old. At first I was kind of lonesome. I didn't have very many friends, and just by chance I picked up an acquaintanceship with an Episcopal minister. He had a very small and a very poor church. But one of the things that he did was on two afternoons a week, Wednesday and Saturday, he opened his study to any kids who wanted to came. And, well, there were games to play and several sets of encyclopaedias, a static machine and all kinds of scientific toys to play with. And he knew a good deal of mathmatics. I learned a lot from him, and I suppose I must have also learned, this may have given me a push to go into science. I'm not very sure about it because, as

I say, in many respects, it seems as though I always was a scientist. Then when I went to college my physics courses were not particularly good. I never did understand the beginning courses in physics that we teach here, that your father has taught many times, and I didn't have trouble with doing well but I got as low a grade really in physics as I ever got. I think that was a C in college.

CZ: Didn't that discourage you?

Dennison: No, I didn't care so much because I was interested in physics--yes, but the course didn't really turn me on and I don't think it would have had any special influence on me, in any case. I was myself, you see.

CZ: I know a lot of kids in our school are turned away from science because of low grades, even other subjects. Do you think a lot of potential is being stymied?

Dennison: Well, I tell you. If you would take a kid who is taking a science course, and who doesn't do well. But in his afternoons, he plays with things which are essentially science, and does it on his own, then I think what grades he gets in high school won't make any difference. If he is really interested in dynamos and motors and all kinds of things that have to do with science or chemistry, then it's possible that the formal training won't discourage him.

CZ: So, do you think that a high school course is not important for some students?

Dennison: Well, it's really very hard to tell. Those people who are going to go on and are going to do creative work in science, for the most part, I think will do it anyhow. How there are lots of others that will be very capable guys either in research laboratories or in teaching or in universities, and it may well be that those people--it is quite necessary what kind of course they get. I'm really just not quite sure about that.

CZ: Let's get back to you. If you had to start over again with a new career, would you chose physics, astronomy, molecular biology, or something else?

Dennison: Yes, I think I would chose one of these. Actually in these early days, physics and astronomy , I was equally interested in. We had quite a large refracting telescope. I did get into a research program, and things of that sort. Would I do it again? Given the same temperament that I had, sure. How could I help it?

CZ: What aspect of your own work has given you the most satisfaction?

Dennison: Oh, two things, I suppose: one of them is, I do very much enjoy teaching and I very much enjoy watching how students who have been in my classes have gone on and done very good work. This is one of the satisfactions, all right. The other one is just simply as you are doing some piece of research and all of a sudden something just falls into place and you understand what's happening. And this is just lots of fun. I enjoy it very much.

CZ: So then your rewards were basically esoteric. How do you feel the way today's young scientist is motivated differs from the scientist of 50 years ago in terms of monetary versus intangible rewards?

Dennison: Gee. I don't really know. I always wanted to make enough money to live nicely and get the things I wanted to get. I don't think it ever really occurred to me very much, the problem of salary or of my position. I had the feeling that these things would come more or less automatically, and I certainly would never have considered, for example, going from one university to another because the salary was higher in one place than in the other. I just wouldn't

CZ: Do you think that's true today with young scientists, the graduate students and associate professors?

Dennison: I would think that this would be true, although I'm not really quite sure. Mainly, for example, someone who enjoys doing research in a certain field and he feels that at university "A" first of all he'll have the facilities to do this kind of research, he will have other people who are there, companions he can talk to, and, of course, he will receive a reasonable salary. Now, if he is comparing this with university "B" at which he has to do, let's say, teaching and some research, but perhaps administrative work that he doesn't like, but he might get 50 percent more salary. I think most young people would choose the one they enjoy doing, and that the 50 percent more salary wouldn't make that difference.

CZ: Speaking of rewards, my dad and several of his colleagues speak somewhat embitteredly over the fact that a lot of less worthy scientists, in their opinion, have won the Nobel Prize for physics, and you have not, as of yet. Do you feel that you might perhaps receive it in the future?

Dennison: Actually, for a lot of people, getting that award has made life very tough for them; they don't feel the freedom to go into whatever kind of research they want to go into. They feel that they have a reputation that they've got to keep up, and it is really true that, in my opinion, a number of moderately good people have been almost ruined by it because they haven't been able to keep up their "reputation", so to speak. I don't know, I've always thought that prizes of this sort are sort of a mixed blessing. There are some people that are really so up at the top, such eminent, distinguished people that it really doesn't make any difference to them. For those people, it won't hurt them. But for people who are not quite so good, I feel, that it can be a great deterrent to their future work.

CZ: With all the people pouring into the science field today, do you think the fact that its becoming increasingly difficult to find jobs in science should influence a young person's choice of career?

Dennison: I think that the physics profession as a whole is to blame for having oversold science, the fact that is of very considerable, practical economic importance made it so that ten to fifteen years ago there was a great urge for people to go into it. It was to be first of all a good job, a good living, but then they began with the heavy courses in grade school, high school and college that were all aimed in this direction, and a lot of people went into it. Well, as so often happens, they just got too many people in it, and there has come a period now when it has been very hard to get jobs. My own impression is that this is one of those fluctuations, and it will settle down, and there will probably be a fewer number, a smaller proportion of the population as compared to four or five years ago going into the field. I think that physics is one of the things that the country can't very well get along without, and soon after this time more jobs will come up. This is my optimistic feeling, should you go into it. One of my colleagues who was here at the U. of M. and is now at Rockefeller University, George Uhlenbeck, is in theoretical physics. And, when students would come to him and want to do a PHD. thesis in theoretical physics he would discourage them: "No, you can't do it, it's a bad field, there aren't enough jobs. The work's too hard. You shouldn't do it." And he would give every argument he could, and if in the end he couldn't discourage them, and they still wanted to work, then he would take them on for a thesis student, if they were any good. So, in a certain sense, this is, I think, my answer: Don't go into it because its glamorous or because its a respectable profession or anything else. If, on the other hand, you find that these are the things ~~that~~ you're interested in, that keep running through your head and that you enjoy it, then eventually this is the thing to go in for.

<u>CZ:</u> To focus more on your scientific discoveries: in the 1920s you were the one who discovered the clue to the specific heat of hydrogen. What made finding it so difficult?

<u>Dennison:</u> This was a time in which the Quantum Theory, which is now the standard theory but which was then a completely new theory, was being developed. It just hadn't existed and in a relatively few years it was being developed. The specific heat of hydrogen gas da

of a hydrogen gas had been measured very accurately by a good many people and the interesting range in this case was from about 100 degrees absolute up to a little bit over room temperature, and in that region the curve went up in a more or less smooth fashion. Now when one applied the new quantum theory to this problem, it predicted a curve that was quite different; it had a bump in it, and it didn't resemble the observed one at all. And it was quite a puzzle for several years. Some very good people worked on it. It was when I was in Cambridge, England, and I was giving three lectures to a graduate class there, the first two I had enough material for and as I got onto the third one, I could see that I was going to run short, and I didn't know what to say, and so I took another crack at this. The idea was really a perfectly simple one. There were hints that the energy states of the hydrogen molecule could be divided into two classes and one thought, in general, was that these states, well, you could turn one into the other as you heat it up or whatever. But there were indications that it might go quite slowly, and so the idea that I had was really a very simple one; that during the period in which they were doing their experiment that it just didn't "go". It was for that reason that instead of having a single gas, they had in effect what was two different gases which did not convert one into the other, and that gave the correct answer. Well, it had no practical interest at that time. In a way, it has a little practical application now, although not so very much for example, Liquid hydrogen is stored for a good many purposes; for example, it is one of the rocket fuels. Now if you would just take hydrogen and liquify it, you try to store it, it will rather quickly evaporate. It simply won't stay and the reason is that you get this conversion of one type into the other, heat is evolved, and it is quite a lot of heat and it simply evaporates the stuff. Now knowing that there are these two it is possible, using catalysts, to convert it all into one of these two classes, one of these two species. Then it is quite permanent. You have to put it in an insulated container like a dewar flask or a railroad tank car or something like that, but it will then last very well. So that, in a certain sense, it is a practical application. If one did not know about these classes, one would find that you just wouldn't be able to keep liquid hydrogen for any length of time. But at any rate, after that time it was done, it was purely science. It was interesting in that the quantum theory, which had been applied to so many problems and worked here was one of the few problems where when it was applied, it would not work. Now the answer was "apply it properly", then it did work.

CZ: What led you to think that it was two separate entities?

Dennison: Well, there were indications of the thing, and I don't know why one gets ideas. This was just one of those that the normal point of view was that there would only be the one class, one type of hydrogen, and not the two. The ortho and the para hydrogen. So, I suppose everybody had a mental block, and for some moments my block disappeared, and then it came out all right,like so many problems of this sort. Do you think you're interested in science?

Zorn: Well, I haven't really thought about it. I've been doing all right in the class.

Dennison: I might recall a little story. --Actually I did finish up my PhD. thesis and got my degree just fifty years ago, 1924. And this was before I got married and we went to Europe. I have a couple of cousins, once removed, very nice girls, just a little older than I. They were the daughters of a successful manager of a steel mill in Joliet, Illinois. When I saw them after I had just got my PhD. they wanted to know what I worked on, what did I do. I did my very best to briefly explain as well as I could to someone who wasn't a scientist, what the problem was and what I had done. They didn't believe me. And especially when they asked me what is this good for? What are you going to do with it? Nothing. --This was part of the physics that I was interested in. I was pleased to do it as physics, and what good was it? Well, as far as I knew, it was of no good. It had to do with the molecule of methane, the marsh gas, and it wasn't going to make methane any better or any easier to get or anything else. This was incomprehensible. They simply could not understand it or believe it. Finally there was only one way they could reconcile it to themselves. That was that I had some how or another stumbled on something of very great commercial value, was in the process of getting it patented, and during this period I would have to not tell anybody about it. They thought I was just covering up! This was a time in which physics was not of any great commercial or economic value. I think that illustrates very well the difference. Perhaps today one might believe that people might be interested in things just because of their intellectual interests or stimulus.

ZC: What would you consider the most important scientific breakthrough in the last ten years?

Dennison: Gee, I don't know. There are so many people working on various things. I find myself most interested in some of the astrophysics and in the application of general relativity, which is certainly an impractical sort of thing.

ZC: What do you see as the next breakthrough in science in the next twenty-five years? Do any great discoveries loom on the horizon?

Dennison: I am reminded of a time when I was in Copenhagen. A reporter came to interview Niels Bohr and he asked exactly this same question. In what region was there going to be the next important discovery. And Bohr's answer was. "Well, if we knew from what direction it was coming, it would have been already made." And I'm afraid that's what it is now. If we could have any ideas where the next really big one comes, it would have been made.

ZC: Do you think it'll be something in the nuclear field?

Dennison: It may well be. There are a lot of very interesting things that are going on there. There are still some puzzlers that have been known for more than thirty or forty years--which very few people talk about.

ZC: Such as?

Dennison: I think that they are somehow very deep problems which one does not yet have any finger hold on. Now one of these is the following: in the world that we live in the different materials, metals and gases and liquids and all those things--you can understand all right in terms of atoms with charged nuclei and electrons, using the quantum theory. Now it turns out that one of the absolutely essential points in this explanation of the world we have is that all electrons are absolutely identical. In the same way, all elementary particles, like protons, are also absolutely identical. Now the difference between being identical and being alike is as big as the whole universe. If they were merely as much alike as you would like, you could make them as much the same as you wish, but if there was still a tiny little bit, so that you could distinguish between electron "A" and electron "B", the whole world would be entirely different. There would be no such thing as our metals, copper, gold, and so on. They wouldn't like it at all. Their existance absolutely depends upon the fact that these things are identical. Not alike. Now up until now, no one has the faintest idea why this is so. I suppose it is because it is one of those general problems, that, just at the moment, there seems to be no way of getting to understand. I am optomistic that one will be able to. It seems to me that many of the other thing that are puzzles will probably also fall out with the answer to this. Somehow or another it is a consequence of the space/time that we have that you could not have an electron that had a charge that was, let's say, a hundred millionth part larger or a hundred millionth part smaller. It has to be exactly the same thing, And of course, there are other, the proton is approximately 2000 times heavier than the electron and no one as yet knows why this is so, but I feel sure it is not chance--it is somehow part of the structure of the world that makes it so. Now these are very deep problems. The second one of these, mass difference, there's perhaps maybe a little progress being made with elementary particles ; one begins to classify them in families, and you think of them as perhaps excited states of one particular entity. And so maybe one is making a little progress. But, one just hasn't gotten very far. So I wouldn't say that this would be the next one in any way. On the other hand, here is one of the things that is not known and that probably has a very deep meaning.

CZ: Will the answer to the question be definable in terms that we now know? Will a whole new terminology be necessary?

Dennison: I have no idea--not the slightest.

CZ: What is your opinion on the theory that there is a possibility of finding super-heavy, stable nuclei with 300-400 nucleons? And if that is possible, would we have a whole new group of elements?

Dennison: It seems unlikely all right. If you could get the stable nuclei, then I see no reason why you couldn't have enough electrons to make them into stable atoms. The trouble is the nucleus. It's so far outside the range of what has been produced in the laboratories of nature that my bet is that it is unlikely.

CZ: What are the difficulties in experimenting with that possibility?

Dennison: Well, the things just won't stick together!

CZ: What kinds of apparatus do they use to try to do that?

Dennison: This would be the sort of thing that you make by bombarding elements, probably generally heavy elements, with protons or nutrons or other things. And you hope that occasionally some of them will stick, and that it will then be stable. Well, they stick all right, but they don't stay together very long. When you sit down and figure out what the forces are, it just seems very unlikely that you could find a combination that will stay together.

CZ: Is it then possible to make gold, the way alchemists believed?

Dennison: Oh, sure, sure.

CZ: Why isn't it done? Is it too expensive?

Dennison: Oh, yes. You can only make individual particles at a time, you see. I'm not a specialist in nuclear theory, so I don't really know. It is a standard sort of thing, that you can change one element into the other by either absorbtion of neutrons and protons or disintegration of one or the other. The numbers that are involved to make up a sizable hunk of metal are just too, too great. If you make a billion a second, it will take you, about a billion years, I'm sure, to make enough to see. That's the trouble.

CZ: That's pretty expensive.

Dennison: Yeah, you'd be tired of it by that time!

ZORN: My dad mentioned that you had worked with something called the 'proximity fuse' during World War II.

Dennison: Yes, that's right.

CZ: What is that, and what did it do?

Dennison: Well the problem essentially arose during the period in which London was being bombarded by the German aircraft. There seemed to be no way of countering it. It was really very bad, fires, people killed, bombs dropped, all the rest. Things looked very, very bad. Now there were several ways which one tried to conquer this. One way was with fighter aircraft, of course not nearly quite as good as we have now, but still faster than the bombers. Generally speaking the bombers would still get though and drop their bombs, and with the facilities that the English had, most nights they didn't knock down any of the German planes. The other way was with anti aircraft, and this was really very difficult. The luck was very poor. You see, what you had to do was; the airplane is going along, and by the time you can get a shell up there. the airplane will have moved about a half a mile or something like that. This means that you had to have a considerable lead ahead, and you have to calculate very carefully. Also, the height is one up near as high as the shell will go, so it's traveling relatively slowly when it gets up there. Now to make it detonate, fragment, up there and hopefully knock the airplane down, you have to guess how long it's going to take the shell to get up there, where the airplane is going to be, and so on. The shells were set with what was called an artillary clock, an actual little watch, that was in the nose of this shell. It might be set for, say, 27.3 seconds, and the clock was started when the shell left the gun, so 27.3 seconds later it would detonate. You can see this is a very difficult operation. For one thing the airplane can dodge, and all sorts of things can go wrong. The idea was that one might be able to do something to be of help for London, by putting in a small radio transmitter, which, as it got up near the airplane, the electomagnetic waves that went out were reflected from the airplane and would have an effect back on the transmitter so that when this thing got to within a hundred feet of the airplane it would trigger the shell and it would explode. This meant that you didn't have to set any clock, you see, and when it got near enough to the airplane where it would do some good. This was a thing that Dick Crane and I worked on here, and there was also a group in Washington that was working on it. Our initial work here was to try and show if you could make such a thing, and would it be worth making, and how much better would it be? This was one of the things that we did with various kinds of experiments here with the theory and so on.

CZ: Isn't that more of a radio technician's job?

Dennison: That had to be. That's right. This, however, was at a time when physicists were by far the best radio people. I think any innovation at that time would have been done mainly by physics people. It had never been built, we didn't know that you could make tubes. This was before the transistor. That had not come yet. You had to have a battery that was activated during this tremendous acceleration when the shell went up. It had to have radio tubes in it. The tubes were miniature tubes, but they had to be not broken by the tremendous acceleration.

CZ: Wouldn't they be costly? And what would happen if it never got close enough to the airplane? Would it explode on land after coming back down?

Dennison: Yes, Yes. That's right and so ultimately, for that reason, it wasn't really used over land. It was used instead by the navy against the aircraft that were attacking navy ships. This was where it was mainly used, and it had its successes. Now I'm sure there are many better ones. I don't know about them, but I'm sure there must be much better ones. It was a kind of a defensive sort of thing that we were doing here, just trying to ward off attacks of which there was no other way of doing it in those days.

CZ: What do you think of the anti-ballistic missile system?

Dennison: Again it looks like a tremendously costly system that very likely will not be too effective. I have no specific knowledge but that's just my opinion.

CZ: Can you explain your ammonia inversion theory? And also, what caused you to believe that matter would not behave in the predicted manner?

Dennison: Well, roughly speaking it's something like this. If you had, let's say, a trough , and, let's say, another one right next to it, and you had a marble and you started rolling it in one of them, it will roll back and forth in that one. Unless its got enough energy, it will never get over that "hump" in the middle and roll into the second one. Well, now that's because, in the usual theory that we think about, particles are particles and behave thusly. This marble is a material object and its as big as the marble and that's it. But, quantum theory, which was the sort of thing which was being developed in the twenties, shows really that particles also have wavelike properties, and waves never can be really confined to a small region. They always kind of leak out a bit, and that is the reason that in this marble case the waves which go with the marble leak through, and they can, in fact, for this reason get through. I'm sure you are able to get this in high school to some extent, don't you? The wave-like properties? It was a very difficult idea to get across when it was first discovered, but not so much now.

CZ: What has been the long range impact of the inversion theory?

Dennison: I don't really know, again I'd be hard put to find any practical consequence of it. When I started working with the first work on methane, ammonium, water vapor, and so one, mine were among the first papers on molecular structure and interpretation of infrared spectra. These then led you to understand the forces in a molecule, and all these things. At that time there were extremely few papers, my first one was one of the very first, I think, that appeared on this subject. Physists kept on with it and them chemists took it up and now there are several journals that are devoted to this one subject. It is one of the ways which chemists are able to make new substances and understand old ones. It is simply that they can now understand the structure of a molecule and while they are using methods which are more refined than the ones that I used, still, these were the initial steps which led into it. At that time there just weren't any. Mine was about the first article that was ever published on molecular structure.

Zorn: My dad had implied that this theory was the founding of microwave spectroscopy.

Dennison: That's right. This is probably the case if you ask a chemist. Now who is able to make something, "How are you able to do that?" Well, its because he does understand the structure. He knows how to alter it, what sorts of things to do. He will cite you dozens of papers where this is, and they cite other theses, and so on back. There may be thousands of researchers who go into these. The ammonia and water vapor were some of the early ones.

CZ: Did your discovery of the proton spin sort of go hand in hand with finding the specific heat of hydrogen gas?

Dennison: The proton spin really was one of the consequences of understanding the specific heat of hydrogen. In order to have these two varieties, ortho and para hydrogen, the proton had to have a spin, and it had to have the magnitude that it has, one half unit.

CZ: Well then you discovered that the proton does indeed have a spin?

Dennison: In that sense I discovered it, that's right.

CZ: Have you ever met Einstein?

Dennison: Yes, but not until he was a relatively older man, though. Let's see, it was a while back, oh, maybe twenty years ago, something like that, when he was working at the Institute for Advanced Study at Princeton. I didn't know him nearly as well as I knew some of the other people out there.

CZ: We all see the scientists,as they are written up in books, as hard-nosed, serious people. Do you have any interesting anecdotes about these men?

Dennison: Well, they're all very human people. I'll just repeat one story that shows Professor Bohr's sense of humor: This was a little bit after I was in Copenhagen, in the early thirties, when he got interested in going to the cinema. It was mainly the period when there were these wild adventures, train robberies, Perils of Pauline, you know, where the heroine gets saved at the last minute. Bohr would enjoy going to these, and he would often be at an episode and he would say "You know, it's very silly to do this, but still I would like to find out what happened after the last week's episode" So he and some of the people at the institute would go off. Well, his comment after one of these sessions was the following: "It's really very remarkable. Now of course there is the villain and he tries to get the girl in trouble in one way or another. Well, that's not so remarkable, that happens everyday. Then finally when things look really very bad, suddenly the hero appears, and he punches the villain in the head or knocks him off or something. Well it's really very remarkable that he shows up at just this moment. But the most remarkable thing still is that there should be a cameraman there to take the picture!"

CZ: You made the comment that at one point you were interested in astronomy. I was wondering, what is your opinion of the space program? Do you think it's a worthwhile endeavor in terms of all the money we're putting into it?

Dennison: Oh I think so. We generally do things that we can do, and this is one of those things that has, through science and technology, become possible. There are a number of practical applications that have come about from it. Alot about computers has been the result of making the guidance computers for sattelites and space vehicles. But then again I guess I can't really say always what is worthwhile. Was it worthwhile to build the pyramids of Egypt?

CZ: Well that has recently been thrown into a new light with the speculation that the pyramids had some scientific significance with respect to astronomy. Regardless, I guess to the people of that civilization they were worthwhile, so in that sense, perhaps.

Dennison: Well, what would they have done if they hadn't built them? Something that people have wondered at, admired,and thought about for thousands of years? They could have just as well not built them. Were they worth building?

CZ: It all depends on what sort of worth one is discussing.

Dennison: Well, that's the way with the space program. Here is something that has been done, and we could easily not have done. I don't know that we would have been any better off than having done it.

CZ: What we have done in that direction will probably be better evaluated in one hundred years. Perhaps looking back we will be able to know if it was worth it or not. That can also be said of all science, I guess.

Dennison: I suppose so. Also, what do you do with the money? You can talk about feeding people and so on, but these people who make a space vehicle can be financed, but we wouldn't be able to feed the enormous mass of people in India, for example.

CZ: Are you still fascinated with science?

Dennison: I've enjoyed physics very much. I find I'm interested in anything in science, really. Particularly physics and astronomy. Of course in recent years the discovery of neutron stars and the possible discovery of black holes is very interesting. I find that I am very intrigued by it.

CZ: Speaking of black holes, there was an article in one of our school science magazines discussing the theory that a black hole had gone through the earth, entering in Russia and escaping in the Pacific, that was the size of a pencil eraser.

Dennison: I think it's pretty fanciful. I tend to believe that it was a meteor , similar to the one that hit out in Winslow, Arizona. Have either of you ever been out there?

Conzett: Yes, I've seen it.

Dennison: That must have been quite a whack!

CZ: Isn't it contrary to the definition of a black hole that it would be able to enter the earth's atmosphere like that? Wouldn't it burn up?

Dennison: In principle ,I suppose, it is possible to have small sized ones. People have speculated a little bit about it, but I'm inclined to think that it's very unlikely. They're probably all good sized sorts of things. What they do have is a very high gravitational field around them. If one that was only a few miles in diameter were to come at the Earth it would raise an awful lot of havoc because of its gravitational field, first of all, and any matter that would be sucked into it would add to that, but it would disappear.

CZ: Something that small, weighing that much! Do you think it would be of a new element?

Dennison: Perhaps. It's very hard to know exactly what it is. The stage before this, the neutron star, is in many respects like an element which is all a nucleus. The nucleus of an element becomes so big that it is the whole star. Now the reason you can't make ones in the intermediate range is, that the thing that makes the neutron star is the gravitational field. The gravitational field goes up as the square of the number of particles. So you just have to have a higher and higher number before you could get anything like this critical field that would hold the things together.

In principle, if you made it small enough you could also do it with a smaller size, but I'm personally not a believer in it.

CZ: Experimentally in a laboratory?

Dennison: I doubt it. I really doubt it. Some things seem most unlikely to me, and this is one of them.

CZ: Well, we've probably taken up far too much of your time already.

Dennison: Well it's been very nice to have you.

CZ: One final question; do you know Richard Ryan?

Dennison: Oh no, no. I wouldn't....no, no.

In principle, if you made it small enough you could also do it with a smaller size, but I'm personally not a believer in it.

CZ: Experimentally in a laboratory?

Dennison: I doubt it. I really doubt it. Some things seem most unlikely to me, and this is one of them.

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Dennison: It's been very nice to have you.

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30 April, 1974
Interview by Eric Zorn
and Pete Conzett