September 18, 1930 Birth, Bialystok (Poland).

1951	Obtained BS, California Institute of Technology, Pasadena (Calif.).
1955	Obtained PhD in Physics, California Institute of Technology, Pasadena, CA
1955 – 1956	Research Fellow in Physics, California Institute of Technology, Pasadena, CA
1956 – 1957	Fulbright Fellow, École Polytechnique.
1957 – 1960	Assistant Professor to Associate Professor of Physics, University of Michigan.
1960 – present	Staff Member (1960-present) and Director, Physics Division (1984-1987), LBNL
1960 – present	University of California, Berkeley: Associate Professor of Physics (1960-1964); Professor of Physics (1964-1994); Chair, Department of Physics (1968-1972); Emeritus Professor of Physics (1994-present)
1966 – 1967	National Science Foundation (NSF) Senior Fellow, European Organization for Nuclear Research (CERN).
1973 – 1974	Guggenheim Fellow, European Organization for Nuclear Research (CERN).
1983	Member, National Academy of Sciences.
2001	President, American Physical Society.

Interview by Ursula Pavlish, AIP, March 1, 2006

https://www.aip.org/history-programs/niels-bohr-library/oral-histories/38285

Professor Trilling, you have a distinguished career in physics from the time when you received your PhD fifty years ago from the California Institute of Technology. After several postdoctoral appointments you joined the faculty at UC Berkeley in 1960. You pursued your research through the years and you also held important leadership positions as the Department Chair (1968-1972) and you were the Director of the Physics Division of the Lawrence Berkeley National Laboratory in 1984-87. Before I ask you about your collaboration with Professor Gerson Goldhaber, please tell me a little bit about your own career in physics.

Experimental particle physics

Well you, of course, covered it relatively well. I started in physics as a graduate student in Caltech, the California Institute of Technology. The work that I was involved with in doing my thesis was already elementary particle physics, the topic that I would stay with the whole time of my career. In those days, accelerators powerful enough to make new particles didn't operate yet, and so the sources of these particles were the high energy cosmic rays. I was involved in using a kind of detector which is called the cloud chamber, which is not appropriate for an accelerator, but with cosmic rays it is operational, you can make it work. My thesis involved studying new kinds of elementary particles, studying their decays in the cloud chamber. I wrote a thesis on that. Then I went from Caltech, I spent a year in Paris, at the Ecole Polytechnique, beginning to work on particles produced in accelerators. I spent a lot of the year looking at the tracks made by particles in photographic emulsions, their interactions and so on, looking through a microscope. Nothing terribly extraordinary came out of this work. It was just, sort of pedestrian, but it gave me the opportunity to work with a different group in a different way. Then, following that year in Paris, I went to the University of Michigan, where I spent three years, 1957 to 1960. And there I joined the group of Professor Donald Glaser, Nobel laureate. He became a Nobel laureate soon after he came to Berkeley, not yet at Michigan. But he was already very well known, because he had just fairly recently invented the bubble chamber. So I worked on several bubble chamber experiments with him and with his colleagues, post docs and so on. The bubble chamber we used was filled with a so-called heavy liquid, with liquid xenon, very very expensive, but it provided the capability of observing gamma rays rather readily, and identifying electrons rather readily, which was not easy in other kinds of bubble chambers like hydrogen bubble chambers. So we did some specialized work and some useful and interesting measurements in those. Now I should say that I joined his group in 1957, and we worked together. And then, he left Michigan in 1959 to come to Berkeley. And I joined him. He helped organize an appointment for me and I joined him as a faculty member here at Berkeley in 1960. Shortly after arriving in Berkeley, Don heard that he was awarded the Nobel Prize. Soon thereafter he shifted his own interests towards biology where he's been involved essentially ever since. So he basically left the Radiation Laboratory where we were working which has now been renamed the Lawrence Berkeley National Lab. And so it fell on me to be in charge of the group that he left. That is, the group that consisted of myself, two staff scientists, and some students. So, in about 1963, with encouragement from the then director of the laboratory, Edwin McMillan, Gerson Goldhaber and I joined forces to form a new group. Originally, what had been the case, was that Gerson and his wife at that time, Sulamith Goldhaber, who was a distinguished physicist in her own right, worked together. They were a team, with Gerson a member of the Segrè group which later became the Segrè-Chamberlain group. I think Gerson was a little bit separate from the others in the group, so he wanted to move. Sula was in a different group, the Lofgren group. I think that arrangement was simply a matter of not putting husband and wife together. Then, with the encouragement of Ed McMillan, the Goldhabers came together in one group with myself, and we formed a group that was called, at least for a while, the Trilling-Goldhaber group. That started, I believe, in 1963. We were involved in a large number of bubble chamber experiments. These were all hydrogen bubble chamber experiments. I don't think I remember them all. A lot of them were focused on the interaction properties of positive kaons, and also we studied the interactions of pions, pi plus and pi minus. We did a number of high energy experiments. That occupied the period around 1963 to 1972. Some of those experiments were done here at Berkeley, some of the experiments were done elsewhere where we could get capabilities that we couldn't get here at Berkeley, like at Brookhaven for example, where we could get higher energies. We may have done an experiment at SLAC. Then, in 1972, the interest in and potential of bubble chamber experiments was beginning to dim considerably, at least for doing strong interaction physics. So we had long discussions and consideration within the group. At that point we decided to accept the invitation to collaborate with Burton Richter and Martin Perl at SLAC. They were designing and constructing a very advanced detector for use at the SPEAR storage ring. We had a lot of discussions within our group, and it was not just Gerson and I, we had some other senior physicists, one of them was John Kadyk, another was Gerry Abrams, who are still here at the lab. And there were probably some post docs. I don't remember; we've had so many post docs over the years. Anyway, the decision was to join Burt and Martin and their colleagues. They had a very outstanding group of young people working with them on the construction and the exploitation of what became known as the Mark I detector. So this was a period of very great excitement. I have to say that the initial period of running was one which I was not at because I was on sabbatical leave at CERN in 1973-74. I came back in late summer of 1974 just in time to participate and be aware of the great discoveries that came out in 1974, the discovery of the charm quark. So, Gerson and I participated in that program. The Mark I was at SPEAR. Then we built, with the Richter and Perl groups, the so called Mark II detector, which was a detector at PEP, the storage ring that was built at SLAC, the Positron Electron Project. There's a slight bit of history associated with that title. I think the original title was Positron Electron Proton because the original idea, which was much more ambitious, was to build an accelerator that could actually collide electrons against protons which it never did, but which was eventually built in Germany. But then it was renamed Positron Electron Project. This was a higher energy ring than SPEAR. In those days the hope was that the energy was sufficient to discover the top quark. The notion of quarks was very well established. People were looking already for the top quark, or soon thereafter. In those days people had very optimistic ideas about how heavy it might be, that it might be light enough to be seen. But that wasn't true, of course. There were other accomplishments of that program, but that wasn't one of the accomplishments. In any case, we were at the Mark II at PEP until the early 1980s. In the early 1980s the Mark II was moved to the newly being built SLC, Stanford Linear Collider. It was the first detector in use there. Unfortunately the SLC took a little more time than anticipated to produce useful data. And so it did not have a very long run before LEP, the big project at CERN, turned on with a much greater luminosity and much greater potentialities than SLC. Although eventually the SLC got another detector called the SLD, much more powerful than ours, it itself was operationally improved substantially, so that it became a really competitive program. Anyway, at the end of our efforts at the SLC, it was clear that we had to find a new direction in which to go. And there was a point at which Gerson and I split up. Gerson had the wisdom to join the Supernova Cosmology Project which was in its infancy at that time, in the late 1980s. And I got involved, with the encouragement of various high level people here. I got involved in the Superconducting Super Collider (SSC), the preparations for that. And I was the spokesperson for the detector, for the Super Collider, which was approved by the program committee. Unfortunately, the Super Collider never got quite the resources that it needed. And in 1993 it was cancelled by the Congress. I retired in 1994. I still spent quite a lot of my time trying to organize a US participation in the LHC, which has happened. The CERN leadership and management was eager to get the US involved, to get help from the US, both financially and technically, but that involved a lot of discussion, different kinds of collaborative arrangements were discussed, about how much the US was going to put in. There was a special subpanel, chaired by Professor Drell, of SLAC, in 1994 I believe, to in fact advise. They did advise the Department of Energy to support a US participation at the Large Hadron Collider which itself was approved in December 1994. A bunch of groups joined. Major groups here joined. I was involved in the preparations of that. I have not, I have to say, in the recent past been actively participating in the physics preparations or in the exploitation of the Large Hadron Collider. Certainly, if you ask me what was the most exciting thing in your lifetime, certainly the most exciting thing was the discovery of the charm quark, following from the discovery of the so called Psi resonance, now called the J/psi resonance. Completely unexpected. Extraordinarily surprising. So that's a general outline.

Pavlish:

Thank you very much. I guess this is what you would also consider to be your most important work?

Trilling:

The most important work was the work that followed. The groundwork was laid when I was in Geneva working on the Intersecting Storage Ring (ISR) at CERN. That was very interesting too. But I was involved, considerably, in some of the subsequent work, to study the properties of these so called charm particles, their decay modes, excited states, and so on. So I would say, this was the most interesting.

Pavlish:

How did you first become interested in physics? What drew you to the field?

Trilling:

Well, I went to Caltech as an undergraduate and a graduate student. As an undergraduate I actually majored in engineering. I majored in electrical engineering. I enjoyed very much the physics and math courses that I took as a freshman and as a sophomore. And so by the time I got to be a junior and a senior, in addition to doing the required engineering courses, I also took all of the required physics courses, basically for a Physics major. So, as soon as I graduated I applied to graduate school at various places that included Caltech, where it was my preference to go. I didn't have to waste any time. I was immediately admitted to physics graduate school. Now, I had started some research activity when I was a senior, already before graduation at Caltech, doing cloud chamber work. So I was quite interested in that. All the physics courses I had taken had been very interesting and enjoyable. I had done well in them. Both in Physics and in math, but particularly in Physics. So that's what I thought I wanted to do.

Pavlish:

I have a question here which you already partially answered. In what time period did you work with Professor Goldhaber? You said it was about 1963…

Trilling:

It started in 1963 to the late 1980s. And I'm not 100 percent sure if it was 1987 or 1988. It is whenever he started working on the Supernova Cosmology Project. It was sometime in the late 1980s. I would guess 1987 but maybe I'm off by a year.

Pavlish:

May I ask you what it was like working with Professor Goldhaber?

Trilling:

I would say, overall it was extremely enjoyable. I think that if the answer had not been that it was extremely enjoyable, then we would not have stayed together for so many years.

Pavlish:

What characteristics do you think distinguish a scientist, or a great scientist, and Professor Goldhaber in particular?

Trilling:

Curiosity of course is one of the obvious ingredients. Curiosity and an insight into what is likely to be interesting and important. I mean, you can make lots of measurements and you can do lots of things, and some of them will not be of any particular interest, and some of them will turn out to be very important. I think that one way of phrasing it, that people sometimes use, is having a nose for discovery. That is, being able to feel out what directions might be interesting, what directions might lead to something new and exciting, without necessarily knowing ahead of time what it is that's going to come out. But just a certain sense of what directions are likely to prove really interesting. A perfect example of that, now there are many examples, a perfect example is the most recent one, which is that Gerson Goldhaber joined this Supernova Cosmology Project. In fact, no one expected the excitement of results that it actually produced in 1998 when the great discoveries out of that program came. This is just an example, there are many examples. He had a real feel for which directions to go which were going to be productive, interesting, and important. He adjusted the directions of his program. It was a group decision. But he was certainly a very strong proponent of our joining the Mark I program at SLAC, at SPEAR. Again, nobody knew what would come out of this, if anything interesting would come out of it. But of course, some very important results came out of it. Not every scientist has that capability, but Gerson does. And that's one of the things that makes him really a very outstanding scientist.

Pavlish:

Do you have any stories or anecdotes about your work with Professor Goldhaber that you’d like to share?

Trilling:

Gosh, I don’t know. If I thought hard enough I could probably think of some, but I can’t think of any right now. If I think of it, I’ll send you an email.

Pavlish:

That’s all my questions for the day. Thank you very much for your time