Face to Face

This section features conversations with personalities related to science, highlighting the factors and circumstances that guided them in making the career choice to be a scientist.

Excerpt of the Interview with Mathew Sands

Mathew Sands talks to Finn Aaserud

Matthew Sands is an American physicist and educator.

He joined the California Institute of Technology (Caltech) in 1950 where he was a colleague of Richard Feynman on the physics faculty. Sands was the first to show the importance of quantum effects in electron accelerators. In 1963 Sands became deputy director of the Stanford Linear Accelerator Center (SLAC). He persuaded Richard Feynman to accept the 1965 Nobel Prize in Physics even though Feynman was averse to such honors. Matthew Sands later joined the University of California, Santa Cruz (UCSC) as a professor of physics and served as its vice chancellor for science from 1969 to 1972.

After retiring from UCSC in 1985, he worked as a consultant for local K–12 schools in Santa Cruz to develop physics laboratory activities for students. From 1960 to 1966, Sands served on the Commission on College Physics, which carried out a national program to modernize physics instruction in the colleges and universities of the United States. He also helped write the famous 1964 physics textbook *Feynman Lectures on Physics* with Richard P Feynman and Robert B Leighton, based upon the lectures given by Feynman to undergraduate students at Caltech in 1961–63.

The subject of the excerpted interview reproduced below is the background to the *Feynman Lectures on Physics*, and the collaboration between Matthew Sands and Richard Feynman in seeing this project to completion.

(Excerpt of interview with Matthew Sands by Finn Aaserud in Santa Cruz, CA, 4 and 5 May 1987)
http://www.aip.org/history/ohilist/5052.html

Aaserud: Yes, we didn't speak much about the book, the Feynman Lectures. That we didn't treat particularly. I don't know if you have anything to say about the origins and your work on that.

Sands: Well, I think we somehow got interrupted. It was my involvement with the Commission on College Physics which got me sort of to focus on the undergraduate situation at Caltech.

Aaserud: Which was from 1960 to 1966 that you were on that Commission, and chairman from 1964 to 1966.

Sands: So I started thinking, discussing and looking at the undergraduate curriculum at Caltech, and decided that we really should do something. They were still using the book that had been written by Millikan some 25 years before, I think, and we should look at the modernization of it. Of course, one of my big concerns had been that the Caltech focus was on graduate education, and that the undergraduates were so small in number. All the time I was there, I never gave an undergraduate course, was never asked to give an undergraduate course. So I started making some internal noises that, we should do something, and decided that what we should do was completely revise the first two years, introducing much more modern physics. It bothered me a great deal that students—these excited young high school students, 18 years old—would come to Caltech all fired up about learning about atoms and quanta and all that kind of thing, and not hear a word about atomic physics until the third year they were there. To spend two years not hearing anything about what was going on in physics in the last 60 years just seemed to me terrible. And no wonder they lost motivation, or had crises about what they were doing with their lives. So I thought it was very important to integrate the ideas of modern physics into the first two years, and so I started saying we should do this. Well, it was clear that if there was going to be time devoted to this sort of thing, there had to be some money found to support this activity, because Caltech couldn't superimpose it on top of the other thing without having the teaching load suffer. So Bacher went and inquired around and found a sympathetic ear at the Ford Foundation for getting some money to pay for released time for hiring visiting faculty to do some of the teaching load.

Aaserud: So he gave you an ear immediately?

Sands: Well, he couldn't very well say, "No, we shouldn't be improving our curriculum." Although it was interesting, the first reaction I got from the majority of people, both faculty and administrators at Caltech, was "What do you mean? We're famous for doing a good job of teaching and we're doing a good job of teaching." "Yes, I know we're doing a good job, but we could do better." And so they were convinced. I got some big help from Lauritsen, particularly the younger Lauritsen, Thomas, and a few others who felt, yes, it was important to do something, and so Bacher felt that he had to. I think he felt constrained that there was a strong enough

sentiment among the faculty that he should try to find some way of making it happen, and he got money from the Ford Foundation for a two year program or three year program. The program I'd outlined was that we should spend a year preparing and then for two years do the program.

Sands: So he got this grant, on the order of a million dollars, as I remember. Oh, there was support for faculty preparing for a year in advance, faculty devoting their time to helping do the thing, you know, and providing secretarial help and so on. It was not the idea of a book at the time. It was just to re-do the two year program in introductory physics. It was a two year introductory physics course which had to be taken by every student at Caltech, engineers, chemists and so on. So we got the money, and I was a little upset when he decided to make Bob Leighton the director of the program. The reasons were obvious. He thought I was too radical, would not introduce as much conservatism as should be into this; he was worried about having a too far out program. So there were Leighton and I and Victor Neher, who was an old famous colleague of Millikan. Neher had been heavily involved in laboratory equipment and courses at Caltech and done a marvelous job. He loved to make experiments for students to work with, so he was given responsibility of re-doing a whole set of experiments for the undergraduate labs. He invented the air track, which is now ubiquitous. Everybody has air tracks. And other interesting things. And Leighton and I were to spend a year outlining the new course. Well, that was a disaster. I would come each week to the meeting, we had weekly meetings, and we'd work. I would come in and say, "Well, now, this is the way we can treat this subject, introduce the quantum effects and so on." And he would come in with essentially the same old thing, saying, "No, we shouldn't change it much because they wouldn't be able to understand that." And so on. So we were at loggerheads for six months, and I was beginning to despair that anything useful was going to come of this, until one morning, one night or whatever, I had a brilliant inspiration: Let's get Feynman to give the lectures. Then we can present alternatives to him, but he will be the final arbiter and make the selection as to what would be in. So then both Leighton's and my stuff would go, but Feynman would decide and do it. And we would discuss with Feynman about what we thought might go, that the students could accept, would be capable of doing, and so on, but let's make use of Feynman's genius. That was my inspiration. Leighton was very cold to the idea. He said, "He's never taught any undergraduate physics. He only lectures to graduate students. He hasn't had any contact with freshmen. Besides, he wouldn't be able to speak at their level." And so on. Bacher had the same idea. "We can't afford to have Feynman do that because he's too important for our graduate courses in quantum mechanics and quantum electrodynamics and particle physics and so on. That would be a waste of time."

Aaserud: Had you asked Feynman in advance?

Sands: Yes, I think the first thing I did was to not go to Leighton or Bacher, but to go to Feynman and say....

Aaserud: ...to get his approval....

Sands: "Now, listen, Richard." I said, "You've spent 40 years of your life trying to understand nature, and now here's your chance to distill it down and make it available. And you know, no other great American physicist of the modern age has done like the Germans did—like Born and Sommerfeld and so on—really pull his knowledge together and make it accessible in a pedagogical way. This is your chance to make a big impact." And he seemed to catch on to that idea and was interested. He got interested. The next day he came back to me and said, "Yeah, well, maybe we could do this." So clearly he was getting fired up. But then I had the opposition. So then I went to my allies, like the Lauritsens, and they thought that would be great. They just thought that would be tremendous. Then I went to Neher, the experimentalist, a member of the triumvirate. He thought it would be tremendous, because they had heard Feynman lecture in departmental colloquia and so on, and he was always just crystal clear. So finally I was able to prevail and convince Feynman and convince the administration and convince Leighton that this was the way to go. And then of course, our original idea had been that as the lectures were being done, we would tape them, and make notes for the students. We had various people help us at first, but that wasn't working very well because everybody had a different approach. People would try to change it a lot and Feynman would be unhappy and people wouldn't understand what he was saying and modify it. And so, it ended up, towards the end of the first year, that Leighton and I were doing most of the writing, trying to convert the oral transcript into chapters of a book. Or chapters for the notes which we got to the students as quickly as possible, usually in a few weeks.

Aaserud: Were the lectures transcribed?

Sands: Yes. He wore a little radio microphone. We had tapes made and the secretary transcribed them. So as it approached the end of the first year—or over halfway through the first year—it was clear that this really was material for a book, that we really should try to put it out in a book, and so we spoke to various publishers. Various publishers had already approached us, had heard about the program, Caltech being famous, Feynman being famous, and had said, "Let us know whenever it's time for a book." So we said, OK, make a proposal. I think it was in April or May that we had a proposal from one company, that they would have copies available for our students the next fall if we went with them. That was crucial. That's why we chose that particular publisher, Addison Wesley, because they had photo type setting or something in the house, and then made a couple of creative suggestions which allowed the speed. For instance, the idea of having a big wide margin and putting all the figures in that. That was an original creation of Addison Wesley for that particular set, and now it's quite common. And at first, I had trouble, because when I sent them the material, the editor would come back with all kinds of changes which took out the spontaneous colloquialisms, the natural language which we had intentionally

kept. So I had to make some very stern words to the Addison Wesley editor, and then they understood and things went smoothly. And then during the second year, when Feynman was giving the second year's lectures, Leighton took responsibility for doing the first year, giving lectures and running the course. And so I had complete responsibility for volumes 2 and 3, working with Feynman on what material was going to be included, writing the material from the transcriptions, working up and getting the whole thing published.

Aaserud: How was the result as compared to your original conception of it, when you discussed it before giving it to Feynman?

Sands: I was very pleased. I was very pleased with the results. I didn't think it would have much wide application, because the Caltech students are a special brand. I mean, they're not the average university student in the country, so I felt that what we were doing would not necessarily be widely applicable, but that it would have an influence. People would see what was happening at Caltech and try to adopt some of the things, so in that respect, I was very pleased. I was very pleased that it seemed to have much more impact than I'd thought. Of course, it still never got used in the United States to any large extent, except by graduate students preparing for their examinations, but it was widely used outside the United States, England and various Commonwealth countries. Now there have been translations into 14 different languages and there are still translations being made this 20 some years later.

Aaserud: Yes. I used it myself in Norway.

Sands: Now, the thing I was unhappy about, though, was, that when we did the lectures, when we decided to publish the lectures, this was clearly not a textbook which had been my original concept. It's a set of lectures and doesn't have all the things a textbook should have, such as emphasis, exercises, outlines or pointing to what's key or not, because students have a hard time understanding, is this little peripheral comment important or is this important?

Aaserud: Yes, it's difficult for self study.

Sands: Yes, so I had intended to adopt the technique which was used very successfully by Samuelson in his famous economics book, which is the lecture book and a second book approximately equal in size and importance which had worked out examples, outlines, emphasis, summaries and exercises, and I had intended to work on that after the book was finished. And that never got done, unfortunately.

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