Interviews of the Margaret MacVicar Memorial AMITA Oral History Project, MC 356
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June Matthews – class of 1962

Interviewed by Kira Buttrey, class of 2023

October 5, 2020

Margaret MacVicar Memorial AMITA Oral History Project

June Matthews (SM Physics 1962 and PhD Physics 1967) was interviewed on October 5, 2020 by undergraduate Kira Buttrey (SB Biological Engineering 2023) via a videoconferencing app. Professor Matthews was at her home in Lincoln, Massachusetts and Ms. Buttrey was at her apartment in Cambridge.

Professor Matthews grew up in Southern California, where she and her mother shared a fascination with astronomy. This interest, and her interest in the physical sciences more broadly, led her to complete a B.A. in physics from Carleton College, in Minnesota. While an undergraduate, she thought she would transition into astronomy for graduate school. However, after completing her Carleton senior thesis project in nuclear physics, she remained in that field throughout her prolific and distinguished academic career.

Professor Matthews earned her Ph.D. in physics at MIT in 1967. After post-doctoral fellowships at the University of Glasgow and Rutgers University, she returned to MIT in 1973 to join the physics faculty. In addition to teaching, she conducted research in intermediate-energy nuclear physics, working with experimental collaborators at the MIT-Bates Linear Accelerator Center and the Los Alamos National Laboratory. Professor Matthews served as the Physics Department's Academic Officer from 1994 to 1998 and directed the MIT Laboratory for Nuclear Science from 2000 to 2006. In addition to her academic leadership roles, Professor Matthews was a member of numerous MIT committees, several of which she chaired, focused on improving student life.

Professor Matthews' research has focused on the interactions of electrons and pions with nuclei to probe their structure. Her primary interest has been in the atomic nuclei made up of just a few protons and neutrons: hydrogen-2, helium-3, and helium-4. She has studied how the protons and neutrons interact and allow the nucleus to "stick together." Her accomplishments include elucidating the high-momentum behavior of protons in nuclei, and probing how pi-mesons exchange their electric charge with nuclear protons and neutrons.

Now Professor Emerita, Professor Matthews is still involved in the MIT physics community, enjoying the weekly department seminars, even over Zoom. She looks forward to resuming travel and attending music gatherings post-pandemic.

BUTTREY: I really appreciate your being available to take part in this project. To start with, could you talk a bit about your childhood love of nature, since you've cited it as driving your early interest in science? What aspects of the natural world were you drawn to when you were growing up?

MATTHEWS: My parents were always interested in nature, driving us around California to the ocean and the redwood forest and other areas. But the other part of nature that was a big influence is the sky. My grandfather on my mother's side was an astronomer, and my mother was always fascinated by astronomy. We didn't do a lot of stargazing or study, but we were always fascinated by the nature of what lies beyond the Earth and the planets, and the stars and the Sun. I think that drove me toward the physical sciences such as physics and astronomy. I never seriously thought of becoming a biologist. That just didn't call to me as a possible profession.

BUTTREY: So you grew up in Southern California?

MATTHEWS: That's right, yes.

BUTTREY: And your grandfather who was an astronomer, that was Harlow Shapley? [Professor of Astronomy at Harvard University who deduced that the sun is not at the center of the Milky Way Galaxy.]

MATTHEWS: That's right.

BUTTREY: Was he the head of the Harvard Observatory?

MATTHEWS: That's right. Starting around 1920 through the early 1950s, he was the director of the Harvard Observatory.

BUTTREY: That's an amazing connection to have growing up.

MATTHEWS: Definitely!

BUTTREY: Did you spend time at the observatory?

MATTHEWS: Sure. Of course, we lived in California, but my grandparents had a summer home in Southern New Hampshire and we would sometimes go back there for the summer. My father was a high school teacher and could take the summer off if he wanted. (Sometimes he had another job.) Although we spent a lot of time at the New Hampshire place, occasionally we would go into Cambridge. And we certainly were given informal tours of the observatory. We were allowed to go inside and see the telescopes. Also, my grandparents lived in the director's residence, which was a large house, quite beautifully furnished and attached to the observatory, so we got to see that and spend some time there. Not as much as my mother did, of course, but my brothers and I did have some exposure and still have memories of the observatory.

BUTTREY: You mentioned that your mother was very interested in astronomy. Was that a hobby for her, or did she pursue it professionally?

MATTHEWS: Yes, professionally, but not at the research level. I think in college she had—it wasn't really a physics or astronomy major, but it was a physical science major. Then she got married and started having kids. She really loved kids, and thought that that was really what she was meant to do—raise a family.

But at some point, I think the family needed more money: high school teachers [like my father] didn't make a lot of money. I can't remember how old I was then, maybe 12, when she got a real job—a job that used her brain—at Caltech, working as a research assistant to one of the astrophysicists there. She had that job for a number of years. This was way before the days of computers. I don't know if this guy had grad students and postdocs working for him, but she was the one who did practically all of his data analysis, analyzing his observations and things that a grad student would probably do these days. This was all done by hand, with the aid of a slide rule. Again, there were no computers then.

At some point, she either got tired of it, or the job went away, or the guy [she worked for] retired. She still wanted to work in astronomy then. Also, she was beginning to get itchy feet and really wanted to travel. I was probably a junior in college then. My parents decided to separate. They didn't divorce, but they went their separate ways because they had different interests. My mother was always fond of travel, my dad wasn't as much, and she was attracted to Italy. So she went to Italy and eventually found a job in astronomy there as a translator of scientific papers. She'd learned Italian by then, and was doing some editing work. For several years, she worked as an assistant to the head of the observatory in Trieste, Italy. That was sort of phase two of her career.

Then, around 1970, she came back to the U.S. and got a job at the University of Arizona in planetary science, and she stayed there for the rest of her career as the editor of a series of books on space science. Again, she wasn't doing research; she was more doing editing and organizing meetings and things like that.

BUTTREY: It sounds like she was a very interesting and accomplished person, and a great role model for you to have.

MATTHEWS: Yes. Both of my parents were great role models. They both felt that their kids should do whatever they wanted to do. They didn't try to push us in any directions. I have a brother who is two years younger than me, and he was always interested in mechanical things such as cars and motorcycles. He decided he wanted to be a plumber, and our parents said, "OK, that's fine." He never did, though.

I also had unrealistic career aspirations, shall we say, like being an opera singer or an actress. And they said "Well, you know, it's hard to make money that way." So I was gently discouraged from things like that. But following a career in the sciences or teaching—I was certainly encouraged to do whatever I wanted to do.

BUTTREY: I think it's funny that you bring up unrealistic career goals as someone who went on to head up MIT's Laboratory for Nuclear Science! That's such an amazing accomplishment.

MATTHEWS: As you say, it's an achievement. But I would say it was a more logical or progressive achievement. I majored in physics in college, went to grad school, got a postdoc and then a faculty position, and worked my way up to various positions of responsibility and so forth. But it wasn't as if I had to have sudden bursts of talent or anything.

BUTTREY: I understand—but still!

Could we talk about that whole progression, starting off with how you ended up studying physics as an undergraduate at Carleton College in Minnesota?

MATTHEWS: When I went to Carleton, I was definitely interested in physics and astronomy because of my family background; also, math. Those were the three things that I really liked. I also liked other subjects like English literature and art and music, but I knew that I would probably major in something like physics.

The reason I chose physics instead of astronomy was because at Carleton there was a one-man astronomy department, this one professor, and he wasn't all that well thought of. I could have majored in astronomy, but it would have been sort of limiting. So I decided that I would major in physics, and maybe, if I was still interested in astronomy by the time I graduated, I could then, in grad school, switch to astronomy.

I remember that the first day at freshman orientation or whatever it was that they had—like the academic midway at MIT, where all the departments were selling their programs—and I went up to the guys at the physics table and said that I wanted to major in physics. They said, "Oh, great."

So I was pretty committed right from the beginning. I liked math, but I saw math more as a tool rather than something that would be a primary research interest. I didn't really understand what a mathematician actually did as a profession. So my choice was physics—I could have changed, of course. You didn't have to decide on day one of your freshman year. But I didn't. I stayed with physics and took some math courses, of course, and took one or two astronomy courses.

BUTTREY: You mentioned that you thought that after your undergrad studies you could potentially transition into astronomy for grad school. Were you set on going to grad school? Did you know that you wanted to make a career in academia?

MATTHEWS: I think so. Maybe not at the very beginning. But I'd sort of been brought up in an academic environment with my mother working at Caltech and with my grandfather being head of an observatory. Also, I would come home for the summer, and I had some summer jobs. I think I spent two summers working at Caltech—my mom probably got me this job or put it in a good word for me—in a new group they had just started in radio astronomy. They needed somebody to fill in for the group secretary, who was going on vacation. I started off doing mostly secretarial stuff like typing, which was fine, but then, when she came back, I switched over to more scientific stuff. Sort of like what my mother was doing, helping the grad students with data analysis.

I really liked that environment. I liked the people I worked with. There were two really wonderful professors or researchers in radio astronomy. I thought they were just both great guys. And there were two grad students with whom I became good friends.

And so by the time I was, maybe, past my freshman year in college, I was pretty sure that this was the kind of life I wanted, and I knew that if I wanted to pursue an academic career, I needed to go to grad school. I assumed that this was what I would do.

BUTTREY: How did you end up at MIT for grad school?

MATTHEWS: Well, this, again, goes back to when I was deciding on an undergraduate college. I had a very influential high school math teacher, whom I remained good friends with. I think he'd been to both MIT and Caltech, maybe one as a grad student and one as an undergraduate. He wasn't pushing me to go to one of those places, but we obviously talked about undergraduate education for somebody interested in math and science.

I had decided—and I'm not quite sure how I came to this decision (again, my parents might have had some influence)—that I wanted to start out with a co-educational liberal arts education. I would go to a place that had good science departments, as Carleton did. But I wanted the opportunity to have friends in a wide variety of fields and to be able to take English courses and music courses and languages. And I know that all those things exist at MIT, and they probably did back in the '50s also.

Now, I guess, you're required to take one humanities course per semester, and a lot of students take more. But there's not quite the same balance between science and engineering and the humanities. The desire for this balance steered me in the liberal arts direction as an undergraduate.

But then, as a college senior I thought, "Well, if I'm going to be serious about being a scientist, I need to go to a place like MIT." I applied to MIT, I applied to Harvard, I applied to Stanford, and my safe school was the University of Colorado.

I did not apply to Caltech because although they accepted women, there were very few women there at the time. And I'd heard some rumors from my grad student friends there that the one woman in the physics department was not having a very good experience. Also, I was interested in going away from home, which had also informed my decision to go to a place like Carleton as an undergraduate—to try out a different part of the country. In any case, I got admitted to those four schools. I considered others. I remember looking at the grad school catalog for Princeton, which, of course, is also a great school for sciences. And I think the very first sentence said something like, "Princeton is a graduate school for men only." This was 1960 and I said, "Oh, OK," and I closed the book.

BUTTREY: What a strange lead-in for a catalog.

MATTHEWS: I know. It was right up there.

BUTTREY: Who was that supposed to convince?

MATTHEWS: Right. It didn't really bother me that much because there were other really good schools out there that were coed, and MIT welcomed women. And Caltech, as I said, accepted women, although there were, at that time, very few. I was basically choosing between MIT and Harvard because I liked the Boston/New England area, and I thought that would be an enjoyable place to go to grad school.

I'm not exactly sure why I chose MIT. One of my Carleton professors had been a student at both MIT and Harvard, so he was he was able to tell me sort of what they were like. Maybe MIT was more welcoming. They offered me a scholarship, but that actually was not an issue because I had won an NSF scholarship. I could have used that anywhere.

I sometimes, particularly back then, thought how different my life might have been if I'd gone to Harvard. I would have had different friends, a totally different experience. But anyway, I chose MIT. There were some bumps in the road, but I never regretted the decision or thought of going anywhere else.

BUTTREY: Thank you for recalling all of that.

You mentioned that Caltech didn't have many women students, even though they admitted women. Do you recall how many women were in the MIT physics world when you were a grad student?

MATTHEWS: I don't. I'm sure these numbers exist. The year I entered, there were, I think, at least two or three other women—which isn't a big number—in addition to me. And there were several women in a class that had entered a year or so before.

At that point, Bexley Hall, which no longer exists, was a residence for women students and married students. This was just a couple years before McCormick Hall was built. I'd applied for a room or an apartment in Bexley, and I think I was on a wait list for a while. I got in, and that was good because I didn't have to go apartment hunting when first I arrived. [McCormick Hall is the two-tower women's dorm MIT built during the 1960s. Given the single-sex housing conventions of that time, its construction enabled the Institute to admit many more women students than it had previously. Katharine Dexter McCormick '04, a suffragist, philanthropist and influential member of the MIT community, funded most of the research needed to

develop the birth control pill. She was also an outspoken advocate of the inclusion of women students at MIT and was instrumental in funding the dorm's construction.]

My first roommate was a woman in civil engineering. But then she moved out or graduated or got married or all of the above. And then, for the next year, I roomed with a physics student, one of the women who were a year or so ahead of me. So that's how I got to know a few more. I certainly wasn't the only one. People say, "Oh, were you were the first woman in physics at MIT?" By no means; there were several before me.

BUTTREY: Can I ask for the names of your roommate and the other student you just thought of?

MATTHEWS: Yes. My first roommate, the civil engineer, was named Joyce Tien [Dr. Joyce Chieh-Chun Wang, MCP '61]. She married a man named Leon Wang. Many years later I had an undergraduate UROP student, a senior thesis student, whose name was Mark Wang. He also worked for me as a grad student. He went home at spring break, and found out that his mother had been my roommate!

BUTTREY: That's crazy!

MATTHEWS: Yes, a crazy coincidence. And then my roommate in my second year, her name was Audrey Buyrn [MIT B.S. in physics 1958; MIT Ph.D in physics 1965; worked in the MIT Laboratory for Nuclear Science from 1971 to 1975; later held science-related governmental roles]. I never really kept in touch with her. We got along pretty well as roommates.

There was a student I just thought of who entered the same year as I did, who was Elsa Garmire [PhD in physics 1965; went on to teach at Dartmouth College]. She was married to a guy named Gordon Garmire [MIT Ph.D 1962; worked on space travel-related research]. And then there was a woman who was from India. I don't remember her name.

I don't know how many women there would have been. But none of that really bothered me because, first of all, I'd gotten well used to being one of the only one or two or few women in a class; in my physics classes as an undergraduate, there were just two or three other women.

But I had plenty of friends. This is something I tell people a lot. They go, "Oh, weren't you lonely being one of only a few women?" Although I was one of only a few in physics, I had plenty of women friends in other fields. One of my best friends from college was an English major who went to grad school at Harvard/Radcliffe. We saw each other a lot. So just like at Carleton, I had plenty of friends of both genders. Not so many women in physics, but more women than men in other areas. So that was fine.

Thinking again of friends, the advantage, if you could call it that, of being one of very few women, is that I had more boyfriends or more dates than I could handle, which was different from at Carleton, where the ratio was around 50-50. But at MIT I certainly had plenty of social life.

BUTTREY: Was there a social life at Bexley?

MATTHEWS: Not as much as there was in later years. I know Bexley developed a mystique of its own after it became an undergraduate dorm. But there was one of the wings of Bexley that was all women students, mostly grad students. We didn't socialize too much because we were all busy, as grad students, working on our classes and on research.

And then in the other part of Bexley, I'm not sure how it was divided up, were married students, and they were, I guess, doing their own thing. I only lived there two years, one with Joyce and one with Audrey, because then McCormick opened. All of us—the women in Bexley, and the women in the women's dorm over in Boston on [120] Bay State Road—were offered spaces. But I turned it down, because one nice thing about Bexley was that it was like apartment living. You cooked your own meals, you had your own suite, your own kitchen, and I wasn't ready to go back to a traditional dorm. At that time, one of my former Carleton roommates was living in Boston, and her roommate had just left to go somewhere else. She was looking for a roommate, so I moved in with her and lived over on the Fenway [in Boston].

BUTTREY: That's a nice area, at least now.

MATTHEWS: Yes, it is a nice area. So I don't think Bexley had that much of a social life when it was either women or grad students, or married couples. But it must have changed because I kept hearing about strange things going on there, especially when I came back to MIT on the faculty. Bexley, at that point, was an undergraduate residence.

BUTTREY: I've heard many of the same things, I'm sure. There is a lot of lore around its time as an undergraduate dorm.

Can you tell me what your research in grad school focused on?

MATTHEWS: Well, backing up a little bit, I had originally thought that I might go into astronomy or astrophysics, as I told you. But I did at Carleton what would have been a senior thesis project in nuclear physics. And I think before that, I'd written some sort of a class paper or a term paper in the area of particle physics. I really became interested in that field.

And so I came to MIT. I had an NSF fellowship, so I didn't have to be a research assistant in any given field. But I was pretty sure that I was interested in nuclear physics, so I went around and talked to the various faculty in that area, in experimental nuclear physics—I was not interested in theory—and settled on a group that worked at an accelerator that no longer is there. In fact, the building that it was in is no longer there, Building 20, which was torn down to build the Stata Center.

I just liked the people I met who worked at this linear accelerator. What we were studying was the interactions of electrons with nuclei, and trying to understand the structure of various substances, how they emit neutrons, how they break apart, how they're put together. I essentially continued with that area throughout my whole graduate career.

At some point, the three researchers—there was one professor and two research scientists—found the MIT LINAC too limiting because it was quite low energy. So they started proposing experiments at other labs. I think they did an experiment at Yale. I didn't work on that. But there was a machine at RPI [Rensselaer Polytechnic Institute], and another one of my colleagues was doing his thesis there, and I went to help with that.

My own thesis research, when it came around time to do that, was actually even farther afield. It was at a new machine that had just been built at the University of Saskatchewan, in Canada, so I spent some time out there. But again, this was sort of the same idea, using electrons and photons to probe the structure of nuclei.

BUTTREY: Were you doing the same type of research post-graduation as well? I've seen that you had post-doctoral fellowships at the University of Glasgow and Rutgers University, and then visiting professorships at Carleton, Yale and Oberlin.

MATTHEWS: Well, all of those were actually a little bit different. Glasgow, yes. In fact, I went there because, again, like Saskatchewan, they were just building a new lab, and they were looking for postdocs and people to help build up the research program, which was very much along the same lines that I had done for my PhD.

The idea was to do those experiments, but do them better with better equipment. That worked out really well. In fact, I stayed in Glasgow longer than I thought I was going to because the research was going well and I was enjoying it.

After maybe around three years, I realized that I needed a real job. I started making inquiries for jobs in the U.S., because although I enjoyed living in Scotland, I thought, "Well, I'm not going to live here forever." I wanted to come back to the United States. I didn't have too much luck finding anything that suited me. Of course, this was well before the days of the internet or Skype or Zoom or anything. This was all being done remotely by mail and so forth. It was hard to job-hunt that way.

I would go back to the U.S. for a vacation such as Christmas, or sometime in the summer, and try to work in a few interviews. But again, there was nothing that I found very attractive until I got this offer from Rutgers, which was actually out of my field. It was still nuclear physics, but it was a different area. But they were willing to take me on as a postdoc, and it was really good experience to do something a little different for a year. I didn't know how long I was going to stay. They sort of warned me because at that point—I don't remember how old I was, but I was older than a normal postdoc. They said, "Well, you know, this is not a permanent job. There's no route to the faculty in this job." I said, "That's OK."

I think I impressed them by getting an experiment done and published within a year. Then I left, and the reason I left was that I got the offer from MIT to join the faculty. For some reason, whether it was because I was in the U.K. or because the job market had picked up here, I had several offers to choose from.

This was around 1972, I think. Around then. And after thinking things over, I decided that MIT was the best. Even though I'd be coming back to a place I'd been before,

and all my other moves had been to totally new places, I decided that this was what I wanted to do.

Again, there's sort of a theme in this. Not so much at Rutgers, but MIT had finally, after doing all this research as visitors at different labs—Yale and RPI and Saskatchewan—proposed to build a larger particle accelerator, which was eventually funded. It became the Bates Laboratory. [The Bates Linear Accelerator Center, renamed the Bates Research and Engineering Center, is a multi-purpose physics research center in Middleton, Massachusetts.] It's not operating now as an accelerator, but the lab still exists. I was in at the beginning to help build up a new research program there, which is probably why I was hired. They were trying to increase the staff on account of having this new facility.

BUTTREY: Given that you were there at the lab's beginning, did you get to play amajor role in shaping how Bates progressed?

MATTHEWS: Yes. I know one of the questions I asked when I was being interviewed for the MIT job—I hadn't been offered it yet, or I guess maybe I had and I was trying to decide whether to take it—was whether I would have the opportunity to do my own thing, shape my own research program. Or, would I be expected to join one of the existing groups, because there were several other senior faculty there who were doing their own research, including my former thesis advisor. I did not particularly want to go back to working with him. He was a great advisor, an extremely smart man. I learned a lot from him, but I didn't care to work with him after my PhD.

Anyway, they said, "Oh, no, you don't have to work with him or anybody. We want you to start your own program." So that was made clear at the beginning. And that was one of the things that attracted me to the job, because I had ideas of things I wanted to do, equipment to build. This was, again, in more or less the same field of using electrons to study the structure of nuclei.

At each step—MIT, Saskatchewan, and Glasgow, and now back at MIT—the equipment was better, the techniques became better. I had to build most of my own electronics in grad school, but when I came back on the faculty, we were able to buy stuff that was much better than anything I built. And computers, of course, were really coming into play, so I was able to push this field further ahead.

And because of that, I did have an influence. It was, shall we say, like being a big fish in a small pond. I became quite well-known for what I was doing, although it wasn't groundbreaking research, the kind that wins Nobel Prizes and things. But within its own realm, I became one of the leaders in the field, and that was very satisfying.

BUTTREY: Do you find the research fun? That's not necessarily a word I would usually use to ask about professional research. But just listening to you talk about this, I see you smiling.

MATTHEWS: Well, some of it is just really hard work. That's an interesting question, because there were certain aspects that I would say I enjoyed. And then there were certain things that I would say really were fun. I'd always enjoyed working with electronics, and although I grumbled about having to build all these circuits for my PhD thesis, I enjoyed doing that. I enjoyed figuring out how to do it.

And then the other thing that I enjoyed doing was writing computer programs. Of course, a lot of students do that these days. It's not such a big deal. But that was more pioneering then. We were using computers in new and different ways.

So I enjoyed those technical hands-on things. I enjoyed, also, just sort of finding out stuff. I needed to figure things out. I don't know if I had what I would call real breakthroughs. But it's like, "Oh, yes, that's how that works."

Now the fun part, the thing that comes immediately to mind, are things that are associated with some of the people I worked with. Especially two men I worked with. In particular, the guy I worked with in Glasgow, Robert Owens, who became a really good friend and is unfortunately now no longer living. But we stayed friends even after I left Glasgow, and he occasionally came over to MIT, and we worked on experiments at the Bates Lab together. And he was, I would say, one of the best physicists I've ever known. But also, we shared just a lot of common interests, and he was somebody I enjoyed being with as a friend as well as a colleague.

And the other person is somebody I met in Los Alamos. In the 1980s, my research changed directions. I started working at the Los Alamos Meson Physics Facility (LAMPF) and built up a program there. And this all happened because this other guy, Peter Gram, called me up one day and asked whether I had any grad students who were looking for a thesis.

And, in fact, I did. There was one student who was without a thesis topic. This friend of mine said, “Oh, I have a really good project. Would you like to work on this?” He had an experiment that was about to be scheduled to run in Los Alamos. And he had another senior colleague, a professor at the University of Wyoming [Glen Rebka]. He also became a friend. But they didn’t have any students. So I proposed this to my student who was looking for a project. And he said, “Yeah, it sounds great.”

And my friend, Peter, who is still living, fortunately, and I became good friends. We had a lot of things in common. We enjoyed working together and sharing stuff outside of physics, books we’ve read, ideas and so forth.

This brought a lot of fun into the research because we enjoyed figuring things out together.

I can think of other examples, too, on the personal side, that made the research fun. I think that if I’d been totally isolated because of just working completely on my own or didn’t have these friends, it wouldn’t have been as much fun.

The other enjoyable thing that we haven’t talked about, at least not in any detail, is my grad students. I’ve had a whole bunch of really wonderful grad students, some of whom I became friends with, some whom I had things in common with, and others that I didn’t outside of physics. But just being around younger people and seeing them develop—and of a lot of them had skills that I didn’t have, and were really talented people—that also made research fun.

BUTTREY: Did you teach as well?

MATTHEWS: Yes. At MIT, it’s different in different departments. All the physics faculty taught one course per semester unless you had a research leave or something. One course could either mean a lecture, or it could mean, maybe, two or three recitation sections. The contact hours depended upon whether you were the main lecturer or recitation instructor.

They assigned me to some sections of 8.01 [Physics I: Classical Mechanics]. And I didn’t really work my way up, but I’ve taught practically everything there from 8.01, 8.02 [Physics II: Electricity and Magnetism] (the freshman courses), 8.03 [Physics III: Vibrations and Waves], sophomore courses, up through graduate specialty courses in nuclear physics. And I’ve taught the Junior Lab [a two-semester sequence in

experimental physics] for juniors and seniors. So I've had a wide variety of teaching experiences, some in my specialty and some in totally different areas. And I enjoyed teaching. Sometimes I felt that I wasn't as good at it as I could have been, but students seemed to like me and to respond well.

BUTTREY: That's great. How did you split your time between researching, teaching, and administrative work? I know that you were on many committees.

MATTHEWS: Yes, I was. I never remember that as a huge burden. It's hard to say how I managed to fit it all in because, of course, I had to prepare for classes. The research sort of came in fits and spurts. The experiments, whether they were at Bates or at Los Alamos, were scheduled for two or three weeks, or maybe a month, and then the rest of the time would be analyzing data or preparing for the next experiment.

The data analysis was almost completely done by the grad students. I would advise them, we would talk, and we would have group meetings and share ideas. But for some of the time, I didn't have to go to a lab every day, so I could concentrate on teaching. And it usually worked out quite well that during the school year, I would probably be mainly teaching. But in the summer, since the Physics Department didn't have any summer classes, that's when I would be concentrating on research. It was probably sort of a 50/50 balance, but it wasn't half of each day or even half of each week.

BUTTREY: How did your job change when you were named the director of the MIT Lab for Nuclear Science in 2000?

MATTHEWS: Well, obviously it changed. For one thing, at that point, I didn't have any teaching responsibilities, although one of those years when I was LNS director, I did volunteer to teach a seminar course that no one else wanted to teach. I thought it was important to do.

I still had students then, so I was still keeping up with research. But as you can imagine, there was a lot of administrative stuff. First of all, I had to learn how to do the job. I didn't have much of a clue. But my predecessor, who was a good friend at MIT, really helped. In fact, he had told me, I think, the spring before I was appointed, that he was stepping down [from the LNS director job]. This was Bob Redwine, who then became the dean for undergraduate education. He thought I was one of the

people that would be seriously considered as his successor [as the LNS director]—which, in fact, turned out to be true.

Bob was invaluable in giving me advice on how to deal with people in Washington, D.C. with the funding agencies—who were the good guys, who were the bad guys.

But there were a lot of things that I just had to learn. Although we had a fiscal office that was very competent in handling the finances of the lab, I still had to know how the money was being spent. So like any sort of administrative job, you come in in the morning, and there are a bunch of crises to handle, and you try to deal with them.

It was interesting, and I think I did a reasonable job. I'm not sure that I did a great job. But I did it for three years, which was the original term, and then I was encouraged to re-up for another three years, so I had six years. I could have stayed on for another three years after that, but I figured that six years was about enough. It was time to get back to research and teaching.

BUTTREY: That is a long time for a job with such a high level of responsibility. Being the administrator and having to go after funding, what was that like? And how did you frame the research in order to get funding?

MATTHEWS: Well, it was actually easier than it sounds, because the Laboratory for Nuclear Science had been funded by the Department of Energy for many years. And there was a three-year block grant that funded all the different groups, some high-energy physics, some low-energy physics, nuclear physics theory, et cetera. So there was a tradition of funding. And LNS was quite well thought of because to begin with we were MIT, we had good students, we were productive. It wasn't as if I had to go out scrambling for money every year.

Because it was a block grant for all the separate research groups, each group wrote its own proposal, so I didn't have to do a whole lot of proposing for funds. I received these documents from the people in the different research groups, I read them, I edited them and made some changes. But at the end, it was very much a group effort.

And then the DOE would conduct a panel review. They would come in, spend a day with us, or two days, and each group would make an oral presentation of the funds they wanted. That's how it started. But of course, we never got all the money we

asked for. There are some people at DOE who always tried to fine tune things and say, “Oh, that’s more important than this. And we’ll give that person money, but not this person,” and try to micromanage, which none of us really liked. But that’s what they did.

I thought it would be a more daunting job, but it really wasn’t that bad. Also, one thing that I hadn’t mentioned is that before I was director of LNS, I was the group leader for one of these research groups, so in terms of the funding and the proposal-writing process, I was doing some of this work for one of the groups already. I knew a little bit about the procedure.

BUTTREY: You’ve spent much of your adult life at MIT. What are some of the most significant changes you’ve witnessed, whether for students, faculty, or the Cambridge community?

MATTHEWS: That’s a big question, of course, and an interesting one. MIT changed in different ways, usually for the better. It would be easier if I can break it down.

BUTTREY: Of course—that was such a wide-ranging question!

MATTHEWS: First of all, the student body changed in several ways. At some point, after I’d been there for a while, the admissions policy was changed. This sounds good, and on the whole it was. Nowadays, everybody talks about diversity, inclusiveness and all that, and that’s all good. But the Institute wanted to bring in a broader group of students, not just by gender and race, but by economic background, fields of interest and so forth. MIT started reaching out to a broader collection of high schools and trying to identify students who might not have thought of coming to MIT, but who could succeed here. They didn’t want to bring in people that would have no chance of succeeding. On the whole, this was good. People in physics actually complained because we were used to getting the super-bright nerds, great in physics and no social skills whatsoever. But now if you didn’t play football or another sport, or play the violin, you weren’t admitted to MIT, even though you had 800s on your SATs.

So there was a period where it was thought that the academic level of students had been compromised. This was quite a long while ago, the 1980s, I think. Whether this was actually true, I don’t know. There were studies made and there were some shakeups in the admissions office.

At sometime during this period, one of these committees that I was on was CUAFA, which is the Committee on Undergraduate Admissions and Financial Aid. I think I was chair for a couple years. We tried to get the faculty more involved in admissions: "If you don't like the students the admissions people are admitting, read admissions folders and help them make their decisions." Faculty were generally just too busy to do that.

On the whole, I think that the positives outweighed the negatives because we did get a broader range of students in terms of interest. I think economics as a major really grew. I'm almost certain we got more women [students], and probably more minorities, too, although it's always been a struggle to identify the minority students who can succeed. You have to look beyond their high schools and say, "Oh, this guy or this girl has real talent."

So the demographics changed among the student body. One thing that I know is when I first came to the faculty in the 1970s, biology as an undergraduate major was practically nonexistent. Perhaps they didn't encourage undergraduates. But now biology is, of course, one of the most popular majors. The biology requirement is one of the GIRs [General Institute Requirements for all MIT undergraduate degrees] that came in a few years ago—we didn't used to have that. And biology, I don't know the statistics now, but at least during some years, it had more women majors than men. Computer science, of course, has also grown enormously.

BUTTREY: And women?

MATTHEWS: When I came to MIT in 1973 there were at least 30% women. You looked around your recitation or your lecture or whatever, and you didn't get the feeling that there were just four or five lonely women. There was a good number of both guys and girls, and they were all normal-looking people.

But I think the number of women probably grew to almost 50% over the time I was there. And I think MIT also, on the staff and faculty side, has become more, shall I say, caring. When I arrived [to teach at the Institute], there was no faculty orientation. I just arrived. I arrived, actually, in middle of the year, January. It was IAP then, so I didn't have to teach right away. But I was just thrown right in and someone said, "Well, why don't you teach this class with this person?" I was really left to fend for myself, and that was fine. I managed, and the department was very friendly and

so forth. But now I think there's a lot more emphasis on work/life issues—on balancing one's professional life with one's personal life. This includes raising children. I'm sure there are more liberal policies for parental leave now, and I think MIT has become a more caring place.

BUTTREY: For everyone, not just the faculty?

MATTHEWS: Yes, I was about to say that. I think particularly for grad students, because many of the grad students are married, and some of them have children, and some of the women who are in grad school have children. I can't imagine having a child at the same time as doing graduate work, but these young women have a lot of energy and they can do it—and they get support services.

So, I think that overall, the students, undergraduates, graduate students, staff, and faculty have more resources available to them if they need help or support. And I think MIT's policies, even unwritten policies, are more, shall we say, humane than they used to be because people's ideas as to what's important have changed. There's more emphasis on the whole person.

There are other ways in which MIT has changed, of course—new buildings and things like that. I'd hate to have the job of writing a history of the place in the last 50 years because there really have been many changes!

BUTTREY: I agree, and thank you for those observations. Not everyone on the faculty has the perspective of having been a student at MIT, too.

Are there changes you would like to see MIT make moving forward, even culturally?

MATTHEWS: Something that people have always talked about at MIT, and I felt this on the faculty, was that everybody is almost too busy—and, I was going to say, under pressure. Well, it's pressure, but it's sort of self-imposed pressure, even if you already have got tenure and you're not worried about that. But everybody's always trying to achieve as much as they can. Changing the subject a little bit—as you know from my CV, I've spent some sabbatical time at other places, such as Carleton, for a semester. Then I was at Oberlin for a semester because I was beginning to question—though I had tenure at the time and didn't even need to think about leaving—whether MIT was the right place for me. When a friend at Oberlin said, "We're looking for somebody to fill in for a senior professor who's gone into another field," I thought, "Well, I'll see

what a small liberal arts college is like." But for various reasons, it turned out not to be a good fit. I came back to MIT.

BUTTREY: Did you miss the high-pressure, "go, go, go" environment?

MATTHEWS: No, I didn't! In fact, I actually found Oberlin to be fairly—I won't say high pressure, but I was teaching an introductory electricity and magnetism course, and I was doing it all myself. I didn't have a TA or other resources. Also, I wasn't doing any research, and I missed that.

Why Oberlin wasn't a good fit is a complicated issue. But getting back to MIT, one thing that I had always thought was, "OK, you're in a college. You're in an academic environment. There should be time to sit down for coffee or tea in the afternoon with your colleagues and discuss academic issues, or discuss philosophy or discuss physics. There should be more leisure, not necessarily in the sense of sort of goofing off or taking time off, but there should be more time to think about things or have serious discussions." And there never has been.

I may be wrong about the details, but I think at Princeton, for instance, the Physics Department, a smaller department than ours, has a daily coffee or tea hour at which everybody gets together, and they all come, and they sit down and they talk about stuff. We don't do that. Even if somebody tries to set something up, people don't show up because they've got to spend time in their lab, or they have a meeting or a committee or have to see a student. It's been hard to get people to take a break, even just a refreshing break.

An example of this is that LNS, the Laboratory for Nuclear Science, got some money to fund a lounge—a room, very nicely furnished. In fact, a former student at MIT who went off and made his fortune in the financial world funded that, and the room is named after him [Robert Lourie].

The whole idea was started by another friend at MIT who had spent some time at Oxford, which I had done also. One thing they have at Oxford—and I think probably many English universities and probably others too—is what they call the Senior Common Room, a room where there's coffee, there are newspapers, etc. People just come in and they meet somebody. They'll sit and chat. In earlier days, they probably smoked cigarettes or cigars or whatever.

We decided we would try to make a room like this at LNS and see if it would be used. Unfortunately, it's hardly ever used. For a while, they had free coffee there, and that was nice. I'd go in and get myself a good cup of coffee. And probably there was a refrigerator. You could come and bring your lunch. And they did have newspapers. I don't know if they still do or not. But it did not serve its purpose. It was just not part of our culture here for people to take some time off to have coffee with colleagues. Or if you do it, you set it up in advance. It doesn't happen naturally. I think the room is used by the office staff. So I don't think it's totally empty.

In any case, this train of thought started when you asked me whether there are changes that MIT could make. I would say, yes, if there were a way to build in more what I call "academic leisure time." Maybe departments other than physics, or other than the laboratory sciences, or other than engineering do this more—some of the humanities or science departments. When I was thinking way back about the pleasures of an academic life, that's always something that I thought would be nice, but it never really happened at MIT. Maybe it's just totally different from our culture of super-achievement and being super busy and driven and under pressure, either self-imposed or peer pressure. Some of that is good—they say some amount of stress is good. But sometimes I felt the stress was too high.

I think that this is recognized, which is why, getting back to what I said earlier, offices like—it's probably not called this, but the Center for Work and Life [Center for WorkLife and WellBeing], have been founded, to try to help people who need a break.

BUTTREY: I think that's a really accurate critique of MIT. I see and feel that as an undergraduate student.

The only department that I've heard of that does anything like arranging the kind of breaks you've been talking about is the Literature Department. I took a literature class and they invited all the students to a weekly tea with the professors. It was great. Not only was it a nice break in the day, but it felt like there was a lot of, as you said, "academic leisure": trading ideas. Maybe more creativity could come out of things like that. Even if they have to be framed in a super productive way, I do think that it would help advance research, as you suggested. It's great to hear you mention that kind of thing.

Something else that I wanted to ask about was the back and forth that I've heard of after the MIT Study of Women Faculty in Science ["A Study on the Status of Women Faculty in Science at MIT" is a report first submitted in 1996 by a committee formed the previous year. The purpose of the study was to analyze inequity concerns raised by women faculty in the School of Science]. Would you mind speaking about that?

MATTHEWS: I'll say a few things, but again, I don't want to get too personal about this.

I was asked to be on this committee that was formed by Professor Nancy Hopkins [MIT Professor Emerita of Biology; researched the role that genes play in longevity and cancer predisposition using zebrafish as a model] by, I think it was by Bob Birgeneau [Robert Birgeneau joined the MIT physics faculty in 1975, becoming head of the physics department in 1988 and dean of science in 1991; his research dealt with understanding properties of condensed matter], who was Dean of Science at the time. I was actually not too happy to be on the committee. Not that I don't support women in science. But I just really didn't think that this was something I wanted to spend time on because I knew that Nancy, in particular, had very strong views.

But Professor Birgeneau persuaded me. He said, "Oh, we need you as a voice of reason," or something like that. So I agreed to be on the committee, and I was only on it for one year. I probably signed the report, but I wasn't altogether happy with conclusion. Let me just back up.

Nancy Hopkins and the biology faculty, I agree, had a genuine grievance. Women professors were not treated well in that department. In Physics, and I think also Earth and Planetary Science—there was somebody from that department on the committee—we never felt there was inequity in our departments.

Nancy kept saying, "Oh, isn't your office smaller than one of your male colleagues'?" No, it's not. It's the same size. She kept looking for ways that really didn't exist, in which I, as a woman professor, was being discriminated against. She was really looking for evidence. Fine. If she wanted to try to uncover things, real evidence, of course—but there wasn't any that I could discern in our department. Biology, yes. Chemistry, yes. I won't say much about other departments because it's been a long time and I don't remember.

In any case, I think to bring those grievances to the fore, with documented evidence, was a valuable thing for her to do, and I certainly don't resent her doing that.

What I did resent was her generalizing to women in science as a whole, because a lot of the report did not apply to all the science departments. It applied to certain departments, and, fortunately, I think a lot of those problems got fixed. She and others did work tirelessly on this, gave talks, and there was a lot of publicity. It was a valuable effort; I just did not feel comfortable being part of it. And, at some point after the report was released, word got out that I was a dissenter in some ways. I was interviewed by some journalist, and that's one thing I regret. I said some things that I shouldn't have. No damage was done, but there may be rumors out there that I was against the whole thing. I wasn't, really; I was just against the undue generalizations.

I resigned from that committee after one year because Nancy and I just didn't see eye to eye. I claimed that our [Physics] department didn't have discrimination problems. She said, "Oh, that can't be true. There must be." And she jumped ahead of her own data.

One thing that I think somebody on the committee pointed out, which I believe in, is that a department that is well run on the whole will not have these problems. I can't remember who that department head was. Whoever it was was a person who was fair, who was open minded, who treated all faculty with the respect that they deserved, who made good decisions regarding tenure—if the department is well-run as a whole, it's much less likely to have inequity problems.

BUTTREY: Thank you for speaking to that.

I'm wondering what you're working on now, and how COVID-19 is affecting your life. It's been such an unusual time.

MATTHEWS: Since I retired eight years ago, people say to me, "Oh, what physics are you doing?" Well, I'm not really doing any physics. I'll qualify that in just a minute. Occasionally, I get asked to referee or review a paper. In fact, I have one on my desk right now that I should look at, so I do some professional activity in that way.

In the year after I retired, which is 2012, I had three papers to finish writing, and I did all that. The first year after retirement, I was pretty much doing what I was always doing.

BUTTREY: That doesn't sound like retirement!

MATTHEWS: No. Right. But that did not last all that long. Then I was working in a sort of very low-key collaboration with a colleague from Los Alamos, not the person I mentioned before, but another guy who also became a good friend [Steve Wender] and somebody who had worked with me at Los Alamos but was a professor at the University of Kentucky [Mike Kovash]. And we started a project together. Mainly, this was happening before I retired. We carried on a bit after I retired. But at that point, I didn't have any students. I think I had hired a UROP student or two. But without graduate students, there's no way that I could carry on with cutting-edge experimental work. I really needed somebody with up-to-date skills in particle detector design, electronics, and data acquisition and analysis using modern computational techniques. I could give talks on the subject. I could do library research, but my hands-on expertise was lacking

That project has more or less fallen by the wayside because although my Kentucky colleague has grad students, they're working on other projects. Our work was almost a sort of hobby for both of us.

I have been thinking that although I'm not going to write a book, I could write a review article or something like that. And that, of course, I could do at home. With COVID, I've actually spent most of my time at home, although I come into campus occasionally. It has affected me mostly on the personal-contact level. Zoom meetings are fine. We have our weekly Physics Department lunch—Well, it used to be a real lunch. Now you can have lunch, but you have it at home, and there is a talk on Zoom. Those are interesting, and they actually try to make them sort of social. For example, you start off in a big Zoom meeting, and then you get randomly assigned to a breakout room that's like sitting around a table for lunch. You chat for a while, and then there's a talk.

Our department head, Peter Fisher [MIT Professor of Physics 1989-present day; researches topics including dark matter and wireless energy transmission], is a really great guy. He has tried hard to keep collegiality alive in the department. But seeing

people on a computer screen is just not the same as seeing them in person. And, of course, one of the things I always enjoyed—I think everybody did—was the food. The food was good. To have lunch with somebody, have a real lunch, is part of the social interaction. Also, all of the professional meetings that I might still go to are now—for the past year and I think for this coming year—virtual.

I've enjoyed some things about being at home and catching up with household projects, doing some gardening, doing more cooking, seeing the seasons change. A lot of it has been very pleasant. But even before and after I retired, I used to travel a lot. I would travel for research before I retired. And then after I retired, I would take recreational trips. Over the past year I haven't been doing any of that. That I do miss.

I have a sister and a brother in California whom I haven't seen since last Christmas, and I'd like to see them sometime, but the idea of getting on an airplane at the moment is a bit daunting. And I have friends in New Mexico whom I haven't seen for even longer, and even friends closer by, in Connecticut and New Hampshire. Either I just haven't made the effort, or they don't feel comfortable about having visitors.

BUTTREY: It's really complicated.

MATTHEWS: So apart from all of that, we haven't talked at all about music. I always used to go to a lot of concerts. Now concert seasons have been cancelled, and there's just not a lot going on. Nor can one play music with other people and sing with other people in groups.

BUTTREY: Do you play an instrument?

MATTHEWS: I play with three different groups, two of which aren't meeting at all because they're four or five people, and I do play an instrument. Another group, in fact, is just two of us, with a guy who's a flutist. We play duets. And we have fairly recently been playing in his backyard, which he feels safe about with the wind blowing, or just being outside. But, of course, when the weather gets cold, we won't be able to do that, so I end up playing by myself or listening to music. But it's not the same as playing or singing with people.

BUTTREY: I'm sorry that COVID has interrupted, or will interrupt, a lot of those opportunities to play music or listen to it live.

MATTHEWS: Well, at least I'm still well and healthy.

BUTTREY: Once again, thank you so much for taking the time to talk with me. It was wonderful to have the opportunity to speak with you.

MATTHEWS: I've enjoyed talking to you also, Kira.