CONVERSATION

A Conversation with Prof. Mildred Dresselhaus: A Career in Carbon Nanomaterials

Prof. Dresselhaus outside the Beckman Center in Irvine, CA.

To hear Prof. Dresselhaus' advice to young scientists, please visit us at the audio page of http://www.acsnano.org/.

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I met with Prof. Dresselhaus at the Arnold and Mabel Beckman Center of the National Academies of Science and Engineering in Irvine, California, at a meeting for the Beckman Scholars where we both spoke and served on a discussion panel.

PSW: In your recent scientific career, you've focused largely on carbon in its various forms—

Mildred Dresselhaus: That's not recent! I started in carbon science when I was asked to change fields from superconductivity[1] to "anything else" and that went from "anything else" to semiconductors and *then* it went to carbon.[2] That was 1961.

PSW: What was it that made you choose carbon in its various forms?

Mildred Dresselhaus: That's really easy, because my view of carbon at that time was [as] a zero-gap semiconductor. Most people didn't consider it a semiconductor; at least the fundamental carbon, which is graphene, is a zero-gap semiconductor with these linear k-bands, and *wow*, linear k-bands are just so different from anything else in science! I wanted to study that. Most people say, "Oh, that's so hard." When you get to *graphite*, then you have four bands instead of just the linear k-bands, and that's sort of a unit cell in graphene (Figure 1). I got started in it, and it was really wonderful, because in 1961, I had two children and it was really great for me that nobody else was interested in carbon. I had the field to myself.

I had four children in less than five years. I was pretty busy with children, and the reason for that was that in society at that time, it was believed that women should not have children beyond 35 years old because if you did you would have some monsters, because the probability of having abnormal births was much higher as you got older. I hurried up and had all the children I was going to have before 35.

That's how it happened and why I was so happy working on carbon from a personal standpoint. I loved it because it was so different from anything else. It's true that there were not many people interested in it. You had to have some confidence that it was okay to do that. I had a few other projects I was working on simultaneously, so there would always be some useful outcome.

PSW: Why do you think it took so long to discover fullerenes and buckytubes and all the amazing forms of carbon that we have now?

Mildred Dresselhaus: It didn't really take so long. People working in the field had some idea that they were there before fullerenes were found.[3,4] We did an experiment that showed that there had to be something like fullerenes a few years before they were found. It had to do with the energy that was released. We knew that when a laser hit a carbon surface, big chunks of something came off, and what else could it be besides carbon? There's nothing else there. We had the idea there was a C_{100}. It was [Richard] Smalley that had the genius of quantifying that it was C_{60}; that was the contribution. That there was something *like* C_{60} was in the air for a number of years before.

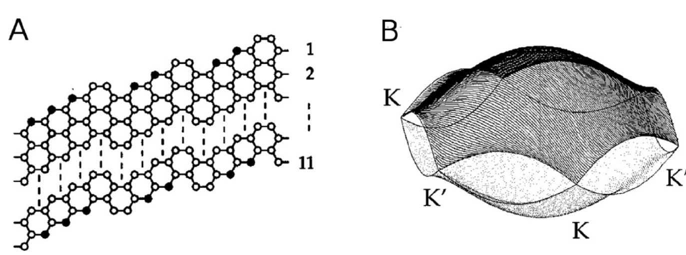

Figure 1. (A) Unit cell of a graphene ribbon. The zigzag sites are indicated by solid circles. (B) Energy band structure of 2D graphite. The valence and conductivity bands make contact at the degeneracy point *K*. Reproduced with permission from ref 6. Copyright 1996 The American Physical Society (http://prb.aps.org/).

I went to Exxon once and I gave a talk there, before the discovery of fullerene. They were the guys working on big chunks of carbon; big chunks at that time were 15 atoms of carbon, which was more than three. One of the things that I talked about when I was there was that we should go beyond 15. There's maybe more beyond because we know that if you hit carbon with a laser, a lot of stuff comes off.

They had a mass spec [mass spectrometer]. That was the forerunner of what Smalley did. They had two peaks; one that was anomalously big at C_{60}, and there was another one that was anomalously big at C_{70}. It was right in there already! It just took genius to recognize it, that it was something strange. It was like even−odd effects in physics.

PSW: Have you been surprised at the continuing variety of materials, states of matter, and properties that continue to be pulled out?

Mildred Dresselhaus: No. Carbon has atomic number six. It's low in the periodic table, it's stable, it's in everything living, and it seems to be something that has some special chemical as well as physical properties. I wasn't surprised. Now, we have nanotubes and fullerenes and graphene. Graphene was a construct that goes back to the beginning of time. The genius was that somebody actually figured out how to make a single layer.[5] Nobody had that in mind; that part came as a surprise. After it was discovered, to find all these properties, that was really great.

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PSW: Is there a lesson for us to keep looking for more?

Mildred Dresselhaus: Well, what's next? There are a lot of things. There are all these edges; we've worked on edges.[6] What's different about a layer is that it's not infinite. It has symmetry breaking edges, and the edges are some kind of construct that we've never really paid a whole lot of attention to. We know how to make these edges now so that they're atomically perfect. It's not every edge, they're symmorphic [a fixed crystal lattice symmetry point upon any symmetry operation]. The ones that are non-symmorphic are not stable, so there's something about that's kind of amazing.

There is a lot more still to be done, even with what we know. I don't know that that's going to be a great frontier, but there's been very little work (Figure 1). That's really a linear chain of carbon atoms. It's really more a chemistry than a physics problem.

Then, there's a whole bunch of stuff in the atmosphere. You go out to Mars or someplace like that and in the outer atmosphere there are carbon fragments. What are they? Why are they what they are? There's still a lot to be done in this field that's just calling for attention.

PSW: In your talk today, you mentioned early discussions on carbon fibers. Can you elaborate on those?

Mildred Dresselhaus: Well, I was introduced to carbon fibers by [Morinobu] Endo; he had made them a while before. His synthesis of very nice carbon fibers goes back to 1972,[7] and the French knew about it. In fact, the Russians were doing this back in 1954 or some ancient time like that.[8] The rest of the world didn't know about this literature because it was in Russian and not all of us could read Russian, and it wasn't publicized. We found out about that many years later. Even the work that Endo was doing in 1976 [was overlooked]. He had a wonderful paper with Agnes Oberlin that had something that looked just like a carbon nanotube, arguably one atom thick (Figure 2).[9] The electron microscopes weren't really

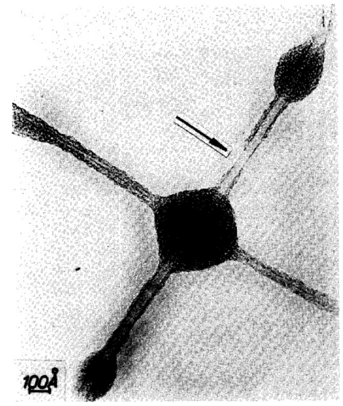

Figure 2. Atomically thin carbon nanostructures were reported by Oberlin and coworkers in 1976. Reproduced with permission from ref 9. Copyright 1976 by Elsevier B.V. (http://www.elsevier.com/locate/jcrysgro/).

that good at the time, so maybe it was two atoms thick. You could argue and say that it was one atom thick. That was pretty exciting stuff, but people didn't really follow very much on that. Then, in 1991, it was kind of rediscovered.[10,11]

When you went to a carbon conference, you would hear a whole bunch of different French groups. It was mostly happening in France, maybe in Japan also, but not in the U.S. There was one guy at Argonne National Lab that was very prominent in the field, but he died very young, and his work wasn't picked up by anyone else in the U.S. There were French groups that were following up on this very small number of carbon atoms and thicknesses of walls. There was [an early] paper from Smalley. The first paper was just a lot of different tubes; it wasn't single-walled nanotubes at all.[12] It was 1993 that the IBM group and NEC Laboratories came out with individual single-walled nanotubes.[13]

The world was ready for it before, and it had come and did come at an earlier time, but nobody picked up on it. Then, when they came up with it, everybody started running with this interesting topic. By that time, the world was sensitized to carbon nanostructures as being interesting. It took a long time, and I don't know exactly what the difference was. Maybe it was a lot of publicity that the two labs that were very prominent were not universities, but industrial labs, and they were advertising their accomplishments; it could be that.

We need a historian of science to go back and to try to figure this out.

PSW: Do you see a future for graphene or nanotubes in technology?

Mildred Dresselhaus: At this point, it's not exactly known what's going to happen. One thing that is clear is that for a conducting transparent material, graphene is pretty wonderful and nanotubes are pretty good, too. Maybe not as good, because with graphene, you can get it more uniform and thinner. Now, the main way to [produce single atomic layers] is with Scotch Tape,[5] but it's not going to be Scotch Tape forever. It's going to be synthesis. We and chemists have been moving that field. My next-door neighbor on my floor at MIT, Jing Kong, an associate professor, is one of the prominent people in that, and we talk about this all the time. Her synthesis method[14] has become very popular. There are a number of other groups that have done related work. I haven't tried to evaluate who's doing the best and who's done what. The whole confluence of works of all these people is having a big impact.

PSW: I have seen some beautiful transfer printing work, too, that looks like it could be scaled up in an interesting way.[15,16]

Mildred Dresselhaus: I think that the scalability of making large quantities of graphene, one and two layers [is important], if you are going to do something like silicon technology, like we have today in integrated circuits. It makes a big difference whether it's one or two layers because the electronic properties are different. We'll get to that point.

PSW: When you were starting your career, to whom did you look for advice and inspiration, and how did you manage that path that was very uncharacteristic at the time?

Mildred Dresselhaus: It's very complicated, because at the time I started my career, I was a woman in physics. I got my Ph.D. in 1958. That's when I took my Ph.D. thesis exam, but at the University of Chicago, you only got your official date when the paper was published. It was published in 1959,[16] so it sounds like my degree was in '59, but actually the work was done in '58. That's when I submitted the work.

So, I was doing something. I was happy that I passed my exam and I got a Ph.D. from that, but the truth of the matter is that when I was doing it, there was one person that was the mentor of everybody doing condensed matter physics, and he didn't believe that women should be doing physics. I decided that, "I'm doing physics, I better not talk to him," because it is not useful to have somebody tell you you can't do physics because of some reason that's out of your control. I couldn't help it that I happened to be born female. I still wanted to do physics, so I ignored him, and in the University of Chicago system, it was okay because the Ph.D. was granted on the basis of published work. *Physical Reviews* accepted my papers, and they were published and so I qualified for the Ph.D. after I took the exam.

We had a lot of exams at the University of Chicago because this was the Enrico Fermi system. The first exam, half the people that took it failed, and the second exam another half failed. So, it was just a quarter that went on into degree candidacy, and then you had to get through the thesis part. It was a pretty selective process. It was not bad, because at that time, there were no jobs. If you weren't pretty talented, you didn't have much chance of ever using this education. Then, we had Sputnik in 1957. All of a sudden, there were many jobs, but people were not yet in the pipeline. It took a while to get people in the pipeline; that didn't really happen until about 1960. If you look at the statistics, there were no people in physics. Then, it [suddenly] goes up.

I overlapped with Enrico Fermi for one year. I arrived at the University of Chicago in 1953, and he was obviously somebody I wanted to study with. I had already taken a quantum mechanics course before, so technically on my *cv*, I had had this course. But that was the course he was teaching that year, so I took it again.

Going back to my talk [to the Beckman Scholars] and Enrico Fermi, his lectures were as simple as you could make physics ever be, and then the problem set really went into the meaning of what each of these lectures was all about. I think for publications as we do them now, the idea that we should publish the simple things as well as more detailed things is good advice.

There were two reasons that my contact with him was special. I believe I was the only girl in the class. I was six weeks older than his daughter, so he made an association. I used to walk to class at the same time in the morning that he walked, and he would always see me and come across the street (I was too shy; I would never come across to see him), and we completed the trip to the classroom together. That was the time that we would talk to each other. He and his wife had a policy of inviting the students [to dinner] once a month. She would make a wonderful, wonderful Italian dinner for us, and it was kind of like the Beckman Fellows getting together and getting to know each other. It was like that for us to get to know his family and to know each other. It was a networking opportunity, and that's the way he taught physics.

The other thing about it, with him, it was about what you did; it wasn't what you looked like. Being a women in physics didn't matter because, in Italy, women had been doing science for many years, but he was much more liberal than most. Not everybody [had the same experience] because I have talked to other Italian women, they have told me how difficult it was to be an Italian woman in physics. [My only experience was with] him, and from him it was what you did and not what your sex happened to be. You can't control that.

PSW: Did you have to find the position at Lincoln Labs by yourself, or did you ask others for help?

Mildred Dresselhaus: After my Ph.D., there was no money yet in physics. It's true that we had Sputnik, but it takes a while for the funding agencies to rev up and produce funding for young people. I was lucky and had a NSF [National Science Foundation] fellowship and that had been in the pipeline for quite some time, because Harry Truman, when he gave his 100th anniversary speech for the AAAS (American Association for the Advancement of Science),[17] one of the things that he said was that we should have was a National Science Foundation. It was started in 1947. We were very lucky that we had a National Science Foundation. They gave a graduate fellowship and a postdoctoral fellowship. They weren't providing big money, but it was plenty to get by. Life wasn't so expensive in those years.

I had the post-doctoral fellowship, and then [I thought], "What next?" "What next" was very difficult for us because at that time they had something called nepotism rules. I got married in 1958 to Gene Dresselhaus, so then there were the two of us, in the same family, and we were at Cornell. My husband got a job there, so I went there with my NSF fellowship. I was supported by my fellowship. I could take it wherever I wanted, so I wound up at Cornell.

PSW: With no advisor?

Mildred Dresselhaus: I never had an advisor, no. I didn't expect to have an advisor because I had done my Ph.D. thesis much more independently than anybody else had. We were all supposed to publish papers, but for most people, they had some person that they could talk to. The men who were doing condensed matter physics at the same time as I did, they could talk to this advisor who was available, but I didn't want to hear that I shouldn't be in physics, so I didn't go talk to him. I did it by myself, which was good because we were supposed to be independent and I value being independent. I didn't take that as a negative so much.

I think my postdoc was pretty disappointing; I didn't really advance the field so much at that time.

Here we were at Cornell. I just finished my NSF post-doc, and I had a husband who was on the faculty, and they had nepotism rules. I offered that I would do physics and not be paid, so I wouldn't count, but they didn't even want me to do volunteer physics. Maybe they wanted to get rid of the two of us, maybe. We both left, and we

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looked for a job where we could be in the same city. We weren't particularly looking for a job at the same place. There were two areas that we could both work, and it turned out that we could both get jobs at the same place in both places.

One of the opportunities was in the Boston area. We both got offers from Ben Laks, to work at Lincoln Labs, and he encouraged us to work together. He wanted to hire the two of us, independently, and then he said, "Okay, you two can work together if you like." We worked together a little bit when we got there, and sometimes we didn't work together; it was a combination of the two things. The other offer was IBM Watson Research Lab,[18] and IBM also offered the two of us jobs. That was when they were right across from the Columbia University on 115th Street [in New York City]. We were offered positions to be there, and then a little bit later, they built the IBM Watson Research Center, but that was several years after we had our offer. We could have been two of the very early people starting at IBM, but we decided to go to Lincoln Lab. Maybe that was less flashy at the time, but we made that choice.

I would say that it worked out pretty well for me, better for me than for Gene. I was working at the Lincoln Lab and my career developed in a very good direction because of bad circumstances. What happened was in 1964, the U.S. Senate passed the Mansfield Amendment,[19] and that said that people working at government facilities should work on topics that were relevant to the benefit of the country. How this was interpreted at Lincoln Lab (it took some time for the interpretation to be solidified, but it was eventually): in 1966, the edict came down that we had to start work at 8:00 in the morning. That was the interpretation of the Mansfield Amendment. In 1964, our fourth child was born. The youngest in 1966 was two years old; the oldest was seven. There was no way that I could make it to the lab at 8:00 in the morning and get all these children organized, and a babysitter, and everything, the whole bit. For my husband, it was okay; he could get there on time, but I couldn't get there. One of us would stay behind and take care of the children and that would be me.

I let people know that times were really tough. I didn't know how I was going to continue my career. About a year or two before, a fellow called George Pratt had moved from Lincoln Lab onto [the MIT] campus and had a faculty position in the electrical engineering department. There were a lot of people that knew me on campus because I used to do work at high magnetic fields in the Magnetic Field Laboratory, which became known as the Francis Bitter National Magnet Laboratory in later years. That was in the vicinity. It was downtown; it was like two blocks away. Well, let me put it this way: when I started working there in 1960, it was in the basement of Building 4 at MIT, but then in 1964, they built another building that eventually became known as the National Magnet Lab.

I was heavily involved in doing high magnetic field research at that time. Between that work and George Pratt, Louis Smullin, who was the head of electrical engineering [at MIT],[20] took a position that was really out of keeping with the times, and he decided that he and George Pratt would nominate me to be a visiting professor to the electrical engineering department. I was appointed to this position, and that was through a grant to MIT from the Rockefeller family. It was named after the sister [Abby Rockefeller Mauzé]. There were five Rockefeller brothers who were very prominent in both philanthropy and public service, and then there was the sister who was almost unknown and who was older than the five brothers.

Figure 3. Prof. Dresselhaus playing in a chamber music group in 2000. Image courtesy of Physics Today. Copyright 2000 by the American Institute of Physics (http://www.physicstoday.org/).

She had a big impact on society by giving money for the development of birth control pills and other benefits and opportunities for women in society and in *science*. Through her, the family donated money to MIT to establish this visiting appointment [The Abby Rockefeller Mauzé chair, an Institute-wide chair], and they appointed E. Margaret Burbidge the first year, the astronomer. She was about 10 years older than me, and then I was appointed the second year to that position. I was in that position for just a very short time, like two or three months without ever applying for anything. They offered me a permanent full-professor-level position, so I went from sort of no job to full professor. That was amazing! But anyway, I'm still there, without interruption, for all those years.

I would say the hero of all this work was Louis Smullin, who had confidence that I would do *something* in science. He had really no idea what it would be at the time because what I was working on in the very early times was the electronic structure of graphite, and there was nobody in the world really interested in that, but that was related to graphene and has become an important topic since that time. From there, it was a vector to all of these carbon nanostructures, which again were not important at that time. He had an idea that that might have something to do with electronics, which was right, but who would have known that? I will be speaking at his memorial service in about two weeks (he died in his 90s, very recently). We'll hear many people speaking about his visionary approach to science and technology because I don't think that I'm the only person who's benefited from his thinking out of the box.

PSW: He's been able to see what you've been able to do with your career, in that field. That's fantastic.

Mildred Dresselhaus: He lived through all of this, and probably some of his other protégés. I never knew who they all were because he was such a modest person. He never really talked very much about his vision.

PSW: Like many scientists, you also have music, playing violin and viola, as part of your life. Do you see some association there, inspiration, or some qualities that are helpful in both fields?

Mildred Dresselhaus: I actually started [in] science *because* of music. I grew up in a very poor neighborhood during the depression years, and in my neighborhood, I had no contact with anybody that had ever gone to college. I didn't know about professions and the "other side of life," so to speak, but through some fluke, I had a brother who was a child prodigy in music. My parents were very poor, and in the early times when he had his music lessons, I used go along because they didn't have a babysitter. It was only 5¢ to go on the New York subway at that time, so I used to go for his lessons. The teacher noticed that I could sing everything he played. "Maybe another talented kid?" It was through that that I got a music scholarship. I was never in [my brother's] league, but in his later years, he decided that he'd like to play with me, but during the years when he was studying, when he was a student, we were like two independent bodies. We both had scholarships, but we went at it from totally independent paths.

My music education was at the Greenwich House Music School in Greenwich Village, and I used Bleecker Street Station on the subway. I used to go on the Third Avenue El [a decommissioned elevated railway in New York City], and I would walk right across Manhattan with my violin and books, from the Third Avenue El to Greenwich Village (that's the other side of the island).

When I was 10 years old, I was once in a train wreck. I shouldn't have been going by myself on the subway at that age. There was a crash on the El. I was not hurt because I liked to look with my violin and everything from the back of the train watching how it went down the tracks. The people that were in the cars on the other side were injured. I still remember that because it was such a nightmare. They had to rescue people and I had to get from the train down to the tracks, carrying my violin and my books.

I got a scholarship and through this I met people that had an education. Most of the people at a music school would pay for lessons. To pay enough for lessons, you had to have some kind of job, so they were from a very different economic level than the people I knew from my neighborhood. It was through that that I found out about the various wonderful schools that they had avail-

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able in New York City. There was only one available for girls. I found out about that, and so I said, "Gee, that would be a good opportunity for me." I mentioned this to my local school; I was then in junior high school, so I was 13 years old, something like that, and the teacher said, "Oh, you have no chance. Nobody from our school ever got into any of these special high schools." My brother had done that because The Bronx High School of Science was one of a number of schools open for boys (they had quite a few), but for girls, they had only one and it was a lot smaller than any one of the individual ones for the boys.

Anyway, if nobody would help me, I would help myself. I studied for the exam, and math was so easy. Of course, I never knew anything, but you could get the exams by [sending away for them] (we didn't have [Internet] yet). You could write away in the mail, and get these exams sent to you and that was the most fun thing in my life, to study math. I enjoyed that thoroughly, and when the time came to take the exam, I could do all the problems.

My English was marginal. When I got to Hunter College High School, eventually, math was always easy, but in English I had *negative* scores my first semester. I didn't even make 0%! But soon thereafter I got to be good at English, and that was the most useful thing. Math is easy to teach yourself. English is, at least for me, less easy to teach, because I didn't even have an idea of where to start. The good education I got in English, languages, and liberal arts has lasted me my entire life. If I had gone to Bronx Science like the boys, I wouldn't have had that. There were some benefits being in this little tiny school, and making my own way.

Would you like to hear what happened after that? I was in high school; I finished the "what next?" We had a guidance counselor, and the guidance counselor said, "You have no money. You don't have any future. There's not much you can do, so you should go to school teaching because there are three things women can do: teach school, be a secretary, or be a nurse." Obviously, being a school teacher was the best for me, so I went to Hunter College to become a school teacher. That's how I wound up there. That was a pretty good profession. I've been doing a lot of teaching because I used to earn my pocket money by doing teaching. I was pretty happy with that outcome.

Then, after freshmen physics, I had Rosalyn Yalow (Rosalyn Yalow is a Nobel Laureate[21]), and she couldn't get a job, so she was teaching at Hunter College, and we intersected. She couldn't get a job, but told me I should be studying physics. *Seriously*! I know it doesn't make sense, but of course it's history that she did finally get a job right after she finished teaching me. She taught maybe two or three classes. She taught for maybe one year, a very, very short time, then she got the job in the Veterans Administration. They had a veterans' hospital, and somebody there had the idea that a physicist would help with radioactive something or other—she started that! She was hired to study what radioactivity might do, either beneficially or not. Radiation damage was known already at the time from World War II. That was in the 1940s when we met up, because in February '48 I started at Hunter College. I met her at the beginning of February '49. She said, "Why don't you major in physics? It's easy for you. You could do education and you could do physics at the same time." I wasn't sure that I wanted to do physics, so I did math also, and I did chemistry, and education some. I did a whole bunch of things. They graduated you when you had a certain number of points, so I remember that my last semester I arranged things to be one unit short so I could go another term and get all this free education. I didn't know a whole lot, but I had completed majors in all these different areas.

So, what was next? Rosalyn said, "You should apply to graduate school. You must apply to graduate school." So, that's what I did. I was going to apply to graduate school and math seemed like the thing I was going to do, and then I saw this advertisement on a bulletin board about the Fulbright program. It was the beginning of the Fulbright program, and so I applied for it. I was a lucky winner, and that's how I became a physicist. I got a Fulbright Fellowship to study at Cambridge University. I was totally unqualified to go there because I really didn't know enough physics to get in or to be a really good student. I completed many gaps in my undergraduate education when I was there; then, I filled in all the pieces.

When I felt I *knew* some physics was when I came to the University of Chicago and I passed those exams. The exams were very selective at the University of Chicago. They had a syllabus. That was the way Enrico Fermi taught. He had all the things that we were supposed to know to be good physicists. I studied all of those topics. I had done self-study since childhood, so this was okay for me. I was happy doing that; I learned some physics from that. I must say that that's the way to do it because his idea was, when you get past these exams, you know enough to go into a Ph.D. thesis in any subfield of physics. He had that idea, as I found out later in studying more about him, that he had made major contributions personally himself to every single subfield of physics (including computational physics, which was a new field and he was a co-author on the first paper in the field).

That education was very, very useful for me because it was hard for women to get up in the world. Later, when I was head of the Office of Science at the U.S. Department of Energy, knowing all these fields of physics made me very comfortable when I talked to these high-energy guys and all these other people. I could talk to them; I could ask them questions, even though I wasn't up on the most recent things happening.

PSW: You do a great deal of public service. Can you comment on the skills required?

Mildred Dresselhaus: I want to start out commenting on *why* I do public service because that's very important and I didn't mention that. (I'm very sorry that I didn't mention it to the Beckman Scholars.) I had my whole education, starting from my music education (which started everything), my high school education (which was superb), and my Fulbright Fellowship, graduate school, postdoc—*everything* was free. My Hunter College education, in particular, emphasized that, "We give you all this free education and it costs $5 a semester for everything." That included tuition, books, laboratory fees, anything you could think of; it was $5 a semester. There's a hitch: "After you graduate, someday in your career, you're going to pay this back." I believe that [the funding for] Hunter education plus the G.I. bill were the biggest and most positive investments that the U.S. ever made in education. They should do a lot more of that because many of these people went on to careers that contributed very significantly to science and everything else going on in the world.

I have no skills for public service. I have the motivation that came from my free education for my whole life, which I could never have had if it didn't come free. So, there's a motivation for public service that comes from that. The only training that I ever had was raising a family, and the organization that one develops by being a mother, housewife, and having a substantial career is on-the-job training. I've had offers to go to business schools and whatever, but I never had an interest in doing that.

PSW: What advice do you have for young scientists?

Mildred Dresselhaus: My advice to a young scientist is to pursue science because it's a wonderful life. If I had to do it all over again, I wouldn't change hardly anything. Somehow, I did the right things and I'm very happy with it. A life in science is the most wonderful life you could have, and it wasn't impossible to combine that with a complete family life and to continue my music (Figure 3) because I did that also. My children are all wonderful music players because that's one thing we did as a family project. It's something we do now! We love to get together and make music and talk about science.

> A life in science is the most wonderful life you could have, and it wasn't impossible to combine that with a complete family life and to continue my music, because I did that also.

[Literature citations and figures were added after our conversation to assist and to direct the reader to relevant publications.]

— Paul S. Weiss

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REFERENCES AND NOTES

1. Dresselhaus, M. S.; Dresselhaus, G. High-Frequency Studies on Superconducting Tin. *Phys. Rev. Lett.* **1960**, *4*, 401–403.
2. Dresselhaus, M. S.; Mavroides, J. G. The Fermi Surface of Graphite. *IBM J. Res. Dev.* **1964**, *8*, 262–267.
3. Kroto, H. W.; Heath, J. R.; O'Brien, S. C.; Curl, R. F.; Smalley, R. E. C_{60}: Buckminsterfullerene. *Nature* **1985**, *318*, 162–163.
4. Smalley, R. E. Discovering the Fullerenes (Nobel Lecture). *Angew. Chem., Int. Ed. Engl.* **1997**, *36*, 1594–1601.
5. Novoselov, K. S.; Geim, A. K.; Morozov, S. V.; Jiang, D.; Zhang, Y.; Dubonos, S. V.; Grigorieva, I. V.; Firsov, A. A. Electric Field Effect in Atomically Thin Carbon Films. *Science* **2004**, *306*, 666–669.
6. Nakada, K.; Fujita, M.; Dresselhaus, G.; Dresselhaus, M. S. Edge State in Graphene Ribbons: Nanometer Size Effect and Edge Shape Dependence. *Phys. Rev. B* **1996**, *54*, 17954–17961.
7. Koyama, T.; Endo, M.; Onuma, Y. Carbon Fibers Obtained by Thermal Decomposition of Vaporized Hydrocarbon. *Jpn. J. Appl. Phys.* **1972**, *11*, 445–449.
8. Balandin, A. A.; Rudenko, A. P.; Stegner, G. The Formation of Carbon Dendrites in the Decomposition of Alcohols on Nickel. *Russ. Chem. Bull.* **1961**, *10*, 705–712.
9. Oberlin, A.; Endo, M.; Koyama, T. Filamentous Growth of Carbon through Benzene Decomposition. *J. Cryst. Growth* **1976**, *32*, 335–349.
10. Iijima, S. Helical Microtubules of Graphitic Carbon. *Nature* **1991**, *354*, 56–58.
11. Javey, A. The 2008 Kavli Prize in Nanoscience: Carbon Nanotubes. *ACS Nano* **2008**, *2*, 1329–1335.
12. Colbert, D. T.; Zhang, J.; McClure, S. M.; Nikolaev, P.; Ikolaev, P.; Chen, Z.; Hafner, J. H.; Owens, D. W.; Kotula, P. G.; Carter, C. B.; et al. Growth and Scintering of Fullerene Nanotubes. *Science* **1993**, *266*, 1218–1222.
13. Iijima, S.; Ichihashi, T. Single-Shell Carbon Nanotubes of 1-nm Diameter. *Nature* **1993**, *363*, 603–605.
14. Reina, A.; Jia, X.; Ho, J.; Nezich, D.; Son, H.; Bulovic, V.; Dresselhaus, M. S.; Kong, J. Large Area, Few-Layer Graphene Films on Arbitrary Substrates by Chemical Vapor Deposition. *Nano Lett.* **2009**, *9*, 30–35.
15. Liang, X.; Fu, Z.; Chou, S. Y. Graphene Transistors Fabricated *via* Transfer-Printing In Device Active-Areas on Large Wafer. *Nano Lett.* **2007**, *7*, 3840–3844.
16. Spiewak, M. Magnetic Field Dependence of the Surface Impedance of Superconducting Tin. *Phys. Rev.* **1959**, *113*, 1479–1494.
17. http://www.nsf.gov/about/history/nsf50/truman1948_address.jsp.
18. http://www.columbia.edu/acis/history/watsonlab.html.
19. http://www.nsf.gov/nsb/documents/2000/nsb00215/nsb50/1970/mansfield.html.
20. http://web.mit.edu/newsoffice/2009/obit-smullin-0608.html.
21. http://nobelprize.org/nobel_prizes/medicine/laureates/1977/yalow-autobio.html.