Excerpt from an Interview with Vera Rubin[1]

by David DeVorkin, September 21, 1995 at the Carnegie Institution of Washington

Section I: Education and Early Influences

Lightman: I wanted to start with your childhood. Could you tell me about any particularly influential experiences you had as a child, particularly anything that got you interested in science?

Rubin: My father was an electrical engineer. He's presently 92 and still could be holding down a job. He had a very analytical way of looking at things, and I enjoyed that very much. I think that was a very large influence. My childhood bedroom — if childhood could be about ten years old — had a bed which was under windows which faced north. At about age 10, I started watching the stars just move through the night.

Lightman: Lying in your bed?

Rubin: Lying in bed. By about age 12, I would prefer to stay up and watch the stars than go to sleep. I started learning. I started going to the library and reading. But it was initially just watching the stars from my bedroom that I really did. There was just nothing as interesting in my life as watching the stars every night.

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Rubin: When I was hanging out of the window, I also took time exposures to show the star trails, which I read about somewhere. My parents were enormously supportive, unbelievably supportive. I mean, I could not have built a telescope by myself. But it really was my project, but anything I needed my father would manage to help me do.

DeVorkin: Did he have a shop at home?

Rubin: No, he really didn't. But in Tacoma Park there was a little woodworking shop, and we would have little pieces of wood cut for us there. He had a jig saw and a few things, but not really much. It was really minimal. Probably around age fourteen or fifteen I started going to the D.C. Amateur Astronomer Club, which met once a month, and my father would go with me. He didn't think it was proper for a young girl to go alone. I heard Harlow Shapley; I heard Donald Menzel. It was the first time I ever met these people. I have no idea whether I was a legitimate member. Sometimes it met at Science Service, and once or twice it met at the Baird Auditorium.
And most often it just met in people's houses. And so the point of these photographs is that I must have taken them downtown like to a Ritz Camera type place, wherever you took film. But I know it wasn't near us. And my mother went to pick them up, and the man told her they hadn't printed them because all they had on them were lines [pictures of star tracks]. Even though she really never got very close to what I was doing, she didn't care if I did it, I would not have even thought she would know what it was I wanted, said she got all excited and told the man that's just what we were looking for and to please print the film.

[1] To access the interview transcript in its entirety, see http://www.aip.org/history-programs/niels-bohr-library/oral-histories/5920-1.

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DeVorkin: So your parents were very supportive.

Rubin: They were enormously supportive, although he really tried to talk me into becoming a mathematician. He thought astronomy was too impractical. He really did. And I've told this to several people, and he even saw this in print during his lifetime. And he would laugh because it made him a "baddie," sort of, that he said that I should not be an astronomer, I should be a mathematician. But he really did say, you know, you'll never make a living as an astronomer. Why don't you do something more practical like mathematics.

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Lightman: Other than this influence from your father, who was an electrical engineer, do you remember any books that you read that had an impact on you at this age?

Rubin: I read a lot of books, such as [James] Jeans's book, The Universe Around Us and Eddington's early books. But I was already hooked. It really came from the sky. In the late 1930's, I remember, there was an alignment of five planets. That impressed me. I didn't realize at that time how likely such a thing was. Then there were several auroral displays. It was those things that really [captured my interest]. It was the visual experience more than what I read in books.

Section II: College at Vasser (audio available at https://www.aip.org/history-programs/niels-bohr-library/oral-histories/audio/33963)

Rubin: That's right. I entered college in 1945. In fact, it turns out that Ralph Alpher worked in this building in the late 1930's as a secretary. They would not hire women here until during the war. Even the secretaries were male. I have since learned that he got out of high school very young and wanted to work for a year before going to college, so he worked here as a secretary.

Lightman: Did you know when you were in high school that you wanted to go into astronomy, or was that later on?

Rubin: Yes, by high school I knew I wanted to study astronomy. I knew I wanted to be an astronomer. I didn't know a single astronomer, but I just knew that was what I wanted to do.

Lightman: Did you know that it was a career possibility or have some sense of that?

Rubin: Yes, I knew about Maria Mitchell, probably from some children's book. I knew that she had taught at Vassar. So I knew there was a school where women could study astronomy. So, yes, it never occurred to me that I couldn't be an astronomer.

Lightman: Is that why you went to Vassar?

Rubin: Yes.

Lightman: Because of Maria Mitchell?

Rubin: Yes. That and a lot of other reasons. I needed a scholarship and they gave me one. I didn't apply to many colleges. There were not an enormous number of colleges where a woman could study astronomy. But I knew about Vassar because of her.

Lightman: When you went to Vassar, was she there?

Rubin: No, shame on you. She died in 1889. This is off the subject, but one of the things that I have attempted to do in my life, and I clearly haven't succeeded, is to make the story of Maria Mitchell as well known as the story of Benjamin Franklin. She really is a great heritage of the American scientific scene. When Vassar was founded in 1865, she was the first professor of astronomy. She was already well known from doing astronomy.

Lightman: Who was influential for you when you were at Vassar?

Rubin: Well, Maud Makemson was the director of the observatory. I learned a lot of important, fundamental astronomy, but even there the idea of being an astronomer [was not especially encouraged]. I don't think I got enormous support for it. The feeling was that there were very few observatories and very few astronomers needed. I wasn't discouraged at all, but I can't say that I was overwhelmingly encouraged. There was a lot of encouragement there for science in general. [There was] a lot of physics and a lot of mathematics. But astronomy, even there, was a very tiny department. It was not a major department on the scientific scene.

Lightman: In addition to taking courses in astronomy, did you study cosmology at all at that time?

Rubin: No. I studied fundamental physics and mathematics — no cosmology whatsoever.

Lightman: Did you take some astronomy courses?

Rubin: Yes. I got my degree in astronomy.

Lightman: Tell me a little bit about your stay at Cornell. Did you get your master's at Cornell?

Rubin: Yes, I got my master's at Cornell. I got married when I graduated from Vassar and entered Cornell. Actually, I had been accepted by Harvard. I have a letter somewhere from [Donald] Menzel [Director of Harvard Observatory] saying "Damn you women," handwritten across the bottom. [This was in response to] a letter I wrote saying that I wished to withdraw because I was getting married and going to Cornell. He scribbled across this very formal letter, thanking me for letting him know, something like "Damn you women. Every time I get a good one ready, she goes off and gets married," or something like that. My husband was already in graduate school. My husband was getting his degree under Peter Debye, and I think it's fair to say that, although he has probably been my strongest supporter, we never even considered doing anything else but my joining him at Cornell. So I entered the Cornell graduate school in 1948 and got a master's while he was completing his Ph.D. That was even less of a department than Vassar. It almost didn't exist. There was one man — this is for publication so I'll have to speak cautiously. He had been a navy navigator during the war, and he was not a very sympathetic person.

Lightman: In what way?

Rubin: Oh, toward anyone learning [about] or being in astronomy. He actually called me in when I arrived and told me to go find something else to study. [He said] that they didn't need astronomers, and I wouldn't get a job and so forth. There was a second person in the department, Martha Stahr [now M.S. Carpenter]. She may have been the only woman faculty member at Cornell at that time. The place was almost exclusively male. I have since read in Margaret Rossiter's book[2] that at that time there was one assistant professor, so I presume it was she.

Section III: Master's Degree at Cornell

DeVorkin: Yes, because at that time, as crazy as Harvard astronomy was, there was a big difference between Harvard and Cornell.

Rubin: That's right; that's correct.

DeVorkin: To be quite frank, there wasn't much going on at Cornell.

Rubin: No, approximately nothing.

DeVorkin: Now did you realize that this was a professional sacrifice for you?

Rubin: I don't know. I really don't know. I can't remember that I thought it was. I was really very much in love with Bob. Marrying him probably was equally important, probably not more. Becoming an astronomer was always enormously important to me, unbelievably important. And I did know what Harvard was. Of course I did. You know, I read journals by then. I knew what I was getting into. On the other hand, I guess I've just always been enormously optimistic. It probably was a different time. It didn't seem to me that I was wrecking my chances to be an astronomer by going to Cornell.

DeVorkin: Yet it was a question of opportunity. The kind of astronomy you would be studying, was that important to you yet?

Rubin: No, well I don't know. I think I knew what I was giving up; I think I really did. I met Menzel. Menzel came to Cornell while I was there. I'm jumping ahead. The Cornell astronomy department was in a shed behind the physics building, a wooden shed, just a linear thing with two little offices and a blob at each end—one was the lab and one was physics. And I remember Menzel coming into my office and sitting down and talking to me, and he said, "There are compensations about coming to Cornell." He meant my life, my personal life. Like the Swarthmore woman's paintings, "there are compensations." This became a way to sort of understand this.

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DeVorkin: First of all, tell me a little bit about Martha Stahr. She just joined the staff a year before you got there.

Rubin: Let me start out by telling you about Shaw.

[2] Margaret W. Rossiter, Women Scientists in America: Struggles and Strategies to 1940, John Hopkins University Press, 1982.

DeVorkin: Fine.

Rubin: I'll get that off my chest. The day I walked into the department he told me to go find something else to study. They didn't need astronomers. He was the most despicable person I've ever come in contact with in my life. He was a Naval navigator or something, and he believed that everybody in the world was really bad. I mean, if a student asked to have an exam changed because a parent died, no matter what the problem, he would accuse them of lying.

DeVorkin: It wasn't gender specific?

Rubin: No, not at all, not at all. So my first job was assisting him in the elementary astronomy course, which conflicted with first year graduate physics. I could not take classical dynamics because it conflicted with when I had to be showing slides and things. So Feynman talked me into taking quantum electrodynamics instead of classical dynamics from him. With my two years of college physics, which hadn't been so great, I started at Cornell with quantum electrodynamics.

DeVorkin: This sounds like pretty shaky ground. So Shaw's word was law. I mean, you couldn't negotiate?

Rubin: On principle, he wouldn't do anything for anyone because they were just trying to take advantage of him. I mean, no matter what you asked, the answer would be "no." So I had to assist in the afternoon classes, the labs, every afternoon. And if the nights were clear, I had to walk to the observatory. And then Friday night was an open house. So Monday, Tuesday, Wednesday, and Thursday I was occupied that way.

I think by the spring of my first year, my parents were coming. And I asked him once if I could not show up on that Friday night cause my parents were coming, and he sat me down and told me how everything I was doing was not good enough, and I wasn't spending enough time, and I got the students out of the lab too soon, and he said "no" I had to be there.

DeVorkin: Not too pleasant.

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DeVorkin: You said that Shaw gave a competent astro-chemistry course. But here you have another person who seems to be just as defensive or abusive as your physics teacher was. Did this help move you more into the physics department maybe at this point?

Rubin: No. It was really very much the same. My reaction was just "He just doesn't understand." I want to be an astronomer, and I just dismissed it.

DeVorkin: Were there other astronomy students?

Rubin: There were other people in the classes. I don't think there was anyone else getting a degree.

DeVorkin: In the first year that you are mentioned [in Observatory Reports] is for the '48-49 academic year. It only mentions staff changes, which is typical in the annual reports. But he says that in '48-49 that

you joined the staff as a graduate teaching assistant, you had charge of the laboratory work in introductory astronomy. Stahr taught advanced courses on the galaxy and on external galaxies. So this was your introduction to any memories of that.

Rubin: Yes. That's right. I remember the course on galaxies. We had to read the Hubble Atlas. I had to learn about galaxies, learn names, learned about Cepheids. It was sort of a contemporary course. You might do it in a freshman contemporary course now, but it was more contemporary than Russell, Dugan, and Stewart.

DeVorkin: Was this a terminal masters program or did they have a PhD. at Cornell?

Rubin: I don't know. I certainly had made the decision to get a masters because Bob was ahead of me and would finish his PhD. in a couple of years. So I knew I was only interested in a masters. I don't know whether they gave a PhD.

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Rubin: No, I must have taken quantum electrodynamics and then maybe classical mechanics next because the normal sequence would have started with classical E & M. I took classical mechanics under P. Morrison, and I took a course under G. Cocconi on cosmic ray physics, which was a nightmare.

DeVorkin: Why?

Rubin: Well, I was the only non-physics student in the class. I didn't know these other people. I would walk in and take the class and leave. He had just come from Italy, and you couldn't understand him, and he would write with one hand and erase with the other hand. And then he would give us problem sets, and there were about ten of us in the class. And the other nine people, I assume, all worked together because they would all do their problems the same way. And then he would hand me my paper without a word. Except, he didn't say right, wrong, or indifferent. He would just say, "Why did you do it that way?" And the reason I did it that way was because it was the only way I knew how to do it. I did not like the course.

Section III: PhD at Georgetown University

Rubin: [George Gamov, her PhD advisor, had been] brought there by Merle Tuve, director of this department, but I really knew none of that. He contacted me and wanted some details of my master's work, which I gave to him. Then I asked him if I could come hear this talk, and he said 'No' because wives were not allowed in the Applied Physics Lab.

Lightman: Wives were not allowed in the Applied Physics Lab?

Rubin: That's correct. It was a classified place, and there was security. So it was very easy to keep people out.

Lightman: Did you think that he was just saying that non...

Rubin: No, he meant wives. Wives were not allowed. It was through that contact that she [started] talking. I spent about one day at Georgetown on a thesis problem that Carl Kiess had given me. It had to

do with faint lines on the solar spectrum. They had a Roland grating at Georgetown, and they did some solar spectroscopy. By the end of the day, I decided that wasn't the thesis problem that I wanted to work on.

Section IV: Discovery of Dark Matter

Lightman: Let me ask you about your work on the rotation of individual galaxies. What motivated you to start doing that work?

Rubin: Oh, that I remember. That I know. That's current enough, because that's really the early 1970s. Again, there were two reasons. One was that I had come to work here, and Kent Ford had built a very exceptional spectrograph. He probably had the best spectrograph anywhere. He had a spectrograph that could do things that no other spectrographs could do, and what you need for a program like that is a good spectrograph. Then, of course, the other side of the question is what you are going to look at. … I decided to pick a problem that I could go observing and make headway on, hopefully a problem that people would be interested in, but not so interested [in] that anyone would bother me before I was done. I chose to study the rotation of M31, and that was what really started that work.

Lightman: You told me a negative reason why you did that. Is there a positive reason why you chose to do that particular kind of work?

Rubin: Surely. Yes, I did tell you the negative reasons. It was my old interest in galaxy dynamics. Before the 1970s, [since] large telescopes were so few — although, by the early 1970s, good image tube spectrographs could make small telescopes behave like large telescopes — much of astronomy operated, and had to, in a realm in which much was inferred. You observed a few facts. Astronomers have been incredibly clever, throughout the history of astronomy, of inferring what they would see if they had a slightly bigger telescope. I really think that's almost the history of big telescopes. Then you get the big telescope, and you go back and you see that what you thought you'd get is, in fact, correct. But then you take the next step, and you make another inference. So people had inferred what galaxy rotations must be like, but no one had really made a detailed study to show that that was so. Inner parts of a few galaxies were pretty well known from the work of Margaret and Geoff Burbidge, but outer parts were not. So, partly, it was my old interest in galaxy dynamics, and then also the realization that with the instrumentation I had available to me, I could really do this. And truly, it was this other aspect of picking a program which I thought was a very valuable one to do, but one that was not so in the forefront of astronomy that everyone was doing it.

Lightman: And breathing down your neck.

Rubin: That's right. It's only fairly recently that I realized that I must just like doing things that other people are not doing. Partly because of the way I get to a telescope, which is relatively seldom.

Lightman: When you first began finding evidence for dark matter, I guess that was a little bit later...

Rubin: Yes, it was the extension, right after the M31 [program], the Andromeda program.

Lightman: Do you remember, did [the dark matter] come as a surprise to you?

Rubin: It's very hard to tell. It's very hard. I think I learned slowly. Well, I guess the answer has to be yes. Of course it was a surprise, because if it hadn't been a surprise, we would not be sitting here talking about it.

Lightman: Do you remember anything about how the community [responded] — unless you wanted to say more about [the last question]?

Rubin: What I was going to say is we would take about four spectra a night. That's about all we could fit into 2 or 3 hours, and I would develop each one. We would take turns guiding the telescope.

Lightman: Was this with Kent Ford?

Rubin: Yes, Kent Ford and I. I would develop the plates. He built the instruments, and I sort of did the science, but we always observed together because we both liked to. I do remember my puzzling at the end of the first couple of nights that the spectra were all so straight. My first ideas were, by today's ideas, just totally wrong. The first thing that came to my mind when I looked at these very straight spectra was that there must be some kind of feedback mechanism. If the stars got too fast, they were slowed down, and if they got too slow, then they were speeded up. It just didn't look like a random occurrence. The idea of a distribution of matter that would just give you that [velocity distribution] really didn't enter my mind at first. I remember consciously thinking that, and that's about all. So it just shows that your intuitive ideas or whatever, the first thing you think of, in that case, is apparently just irrelevant. It doesn't have that much to do with [my current thinking] in that case. I was really thinking more in terms of observables than the distribution of matter.

Lightman: When you did realize that it meant something about the distribution of matter and dark matter, do you remember, as you began talking to people about this, what the reaction was?

Rubin: The reaction was two-fold. In fact, historically, we've left something out. After I finished the early M31 work, which was in the early 1970s, then I went back to the large scale motion problem in the mid-1970s.[14] My going to the rotation curves was, again, to get away from the controversy of the large-scale motion [research]. Therefore, I really loved it, because the rotation curves were so flat. Observationally, it was such a nice program. All you had to do was show someone a couple spectra, and they knew the whole story. In a sense, it was a wonderful observing program, because when you [ask] what were people's reactions, there was never any doubt on anyone's part that these rotation curves were flat. You didn't have to show them measurements. You didn't have to argue. All you had to do was show them a picture of the spectrum.

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Rubin: I'm an observer. I really feel very much that I'm an observer, and I tend to relate what we know about the universe to what has been observed. So my feelings about cosmology, I think, are probably much more loose and relaxed than' [those of] many people. Maybe this does come from starting with Gamow, from my early work. I think many of the models are brilliant, and some of them probably have some parts that are right. But, personally, my attitude towards many such theories is that we're still groping for the truth. So I don't really worry too much about details that don't fit in, because I put them in the domain of things we still have to learn about. I really see no reason why we — and include all of us in this generation — should just have been lucky enough to live at the point where the universe was

understood in its totality. I think the best thing I can say is that I didn't worry about it anymore than any other facet of details that don't seem to fit. I think as telescopes get bigger, and astronomers get cleverer, all kinds of things are going to be discovered that are going to require alterations in our theories. The horizon problem doesn't exactly come into that kind of situation, but I think, science consists of just continually making better and better what has been usable in the past. So all I can say is I would put it in the domain of something that needed attention.

Section V: Thoughts on Women in Science

Lightman: Maybe a little bit of it will come out of the numerical simulations, to whatever extent you can believe those. I have a couple more questions. One of them is a question about women in science. You've already made some comments bearing on that. Do you think that your experience in science has been different because you are a woman rather than a man?

Rubin: Of course. Yes, of course. But I'm the wrong person to ask that question. The tragedy in that question is all the women who would have liked to have become astronomers and didn't. For those of us who have been successful in doing science, clearly the problems haven't been so great that we couldn't overcome them. By and large, if you ask a set of successful women, their answers would have to be that whatever the problems or differences were, they managed.

Lightman: What do you think that some of the problems are that prevented other women from going into science who might have?

Rubin: I think probably it's the way we raise little girls. I think it happens very early. I think also it's what little girls see in the world — I was going to say the universe — around them. It's an incredible cultural thing. I have two granddaughters. One of them — her mother and father are both professionals, her aunt and uncle who were visiting are professionals — she said her toy rabbit was sick. Her uncle said, "Well, you be the doctor and I'll be the nurse, and we'll fix it," and she said, "Boys can't be girls." And her mother realized that she never had seen a doctor who was a woman. By the age of two, she knew that men were doctors and women were nurses. So you may talk about role models and your thinking about colleges, but this happens at the age of two. It's a very complicated situation. I'm not sure how you handle this. I think it's a terrible problem; I think it sets in very young. Somehow or other, you have to raise little girls who have enough confidence in themselves to be different. I went to a DC public high school. I was very, very interested in astronomy, and I just could keep myself going by telling myself that I was just different than other people, that they just had different interests than I did. I wasn't really planning on telling you this, but it is so incredibly relevant. I had a physics teacher who was a real macho guy. Everybody loved him — all the males. He did experiments; he set up labs. Everybody was very enthusiastic. I really don't think he knew how to relate to a young girl in his class, and it became a terrible battle of wills. He never knew that I was interested in astronomy; he never knew that I was interested in science. The day I learned I got my scholarship to Vassar, I was really excited because I couldn't go to college without a scholarship. I met him in the hall, and probably said the first thing I had ever said to him outside of the class, and I told him I got the scholarship to Vassar and he said to me, "As long as you stay away from science, you should do okay." It takes an enormous self-esteem to listen to things like that and not be demolished. So rather than teaching little girl's physics, you have to teach them that they can learn anything they want to. When I was at Vassar, I sent off a postcard to Princeton asking them for a catalog of the graduate school. Sir Hugh Taylor, the eminent chemist who was then the dean of the graduate school, wrote me a letter saying that [since] they wouldn't accept women, they wouldn't send me the catalog. Some things are better, but a lot of them are not. My daughter is an

astronomer. She got her Ph.D. in cosmic ray physics and went off to a meeting in Japan, and she came back and told me she was the only woman there. I really couldn't tell that story for a long time without weeping, because certainly in one generation, between her generation and mine, not an awful lot [has changed]. Some things are better, but not enough things.

Lightman: She also has you as a role model, which is better than the situation you had.

Rubin: Yes, that's true. And she has other successful women scientists, but the numbers are still awfully small. I saw by the very last AAS [report] that came in, that the salaries are still down.