

NBWM
Joel Lebowitz
Video Conference

by David Zierler
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DAVID ZIERLER: OK, this is David Zierler, oral historian for the American Institute of Physics. It is June 8th, 2021. I am delighted to be here with Professor Joel Lebowitz. Joel, it's great to see you. Thank you for joining me today.

JOEL LEBOWITZ: You're very welcome, thank you.

ZIERLER: Joel, to start, would you please tell me your title and institutional affiliation?

LEBOWITZ: I'm the George William Hill professor of mathematics and physics at Rutgers University, New Brunswick, New Jersey.

ZIERLER: Now, who is or was George William Hill?

LEBOWITZ: He was a great mathematician of the late 19th and early 20th century, an American. He worked for the US Naval Observatory. There is something called the Hill equation, which describes the motion of planets. He was an undergraduate at Rutgers.

The story is that when Poincare came for a visit to the United States and was asked whom he would like to meet a most important person on his list was George William Hill. So, when I was made what they call a Board of Governor's professor, I was given a choice of names, I chose the name George W. Hill. It is also the name of the building where the math department of Rutgers is located.

ZIERLER: Now, your affiliation is joint between the departments of mathematics and physics?

LEBOWITZ: Correct, that's right.

ZIERLER: And in terms of your teaching responsibilities where you have graduate students, where do you consider more your home department, math or physics?

LEBOWITZ: Math. My office is in mathematics. My budget is in mathematics. But, in actual fact, as far as graduate students go, I think I've had more physics graduate students than math graduate students, or approximately the same, since I came to Rutgers. I really am in both departments. I occasionally have to go to two meetings a week, not just one meeting—

ZIERLER: [laugh]

LEBOWITZ: —a faculty meeting.

ZIERLER: Joel, are you still involved with the Center for Mathematical Sciences Research?

LEBOWITZ: Yeah, I'm officially still director of this center. The University just recently had a review of centers, and they gave us a good mark. [laugh]

ZIERLER: [laugh]

LEBOWITZ: They did say that we have to think about successors. [laugh] Given my age, that's not unreasonable.

ZIERLER: [laugh] Joel, a nomenclature question, how do you understand the difference between mathematical physics and physical mathematics?

LEBOWITZ: Well, I don't really know what physical mathematics means. I don't think I have heard that expression much. As for mathematical physics, I mentioned in my Heineman talk a

quote from Freeman Dyson. "Mathematical physics is the discipline of people who try to reach a deep understanding of physical phenomena by following the rigorous style and method of mathematics."

I believe that that's a good definition of mathematical physicists. We don't always succeed. Many mysteries remain, which I guess is good. [laugh] Anyway, yeah, so, I don't know what physical mathematics would really mean. There are certainly physicists who use mathematics, and are very, very good at it.

My own training has been in physics departments. I got my PhD in Syracuse in the physics department, and I got my bachelor's degree at Brooklyn College also in physics. I don't think I had mathematics as a specialty in my bachelor's degree, at least not that I remember.

ZIERLER: Joel, in your research, when is it more appropriate to draw on the formalism of mathematics, and when is it more appropriate to draw on intuition for physics?

LEBOWITZ: I think the latter is really, at least in my case, the more important one. Of course there's much to be said for mathematical beauty, I don't remember who was being quoted, that given the choice between truth and beauty [laugh], you choose beauty. Maybe it wasn't truth and beauty; maybe just experiment and beauty, something like that.

ZIERLER: And what are you wor...?

LEBOWITZ: I think physics is intrinsically much more interesting than mathematics because it deals with the real world. Mathematics is beautiful, abstract, and as Wigner was saying, the utility of mathematics in physics is astonishing. You must know that article—

ZIERLER: Yes.

LEBOWITZ: —by Wigner, I think that's a marvelous article.

ZIERLER: And currently, Joel, what are you working on these days? What's interesting to you?

LEBOWITZ: Well, as I said at my talk yesterday at this Granada Seminar, basic questions of statistical mechanics, why it works, how it works, and, more important, though not so easy, how to apply it to problems like biology and even sociology. I haven't gone into what you discussed with Ed Witten about information theory and foundations of the laws of physics. I cannot say I really understand that. And I find that a lot of the hype about information theory is more hype than reality. But people like Ed, who are very knowledgeable and deeply understand physics, think that there is something deep going on there. I'm ready to listen, and wait to see what will happen.

ZIERLER: Joel, how have you done in the past year and a half in the pandemic? Has it been difficult for you? Has it given you more opportunity to work on long-standing problems?

LEBOWITZ: Well, I would say probably neither [laugh] in some sense. As a theoretical physicist or mathematical physicist, I have not been affected so much, especially with availability of Zoom communications. I run a seminar every week on Wednesdays, which I find very interesting, you know, people from Japan, from California, from Russia. Yesterday I gave a talk at this Granada Seminar, which is something that had been held every two years for many years, and I was a frequent visitor there. Today I got a long message from somebody, with whom I've collaborated with, in Bangalore, India, raising many questions about what I was talking, and proposing we have a Zoom session soon to discuss it. So, in that way, the pandemic has not

affected my work so much. On the other hand, I do miss seeing people, seeing friends. I have not been to my office at Rutgers for 14 or 15 months at all.

I did give a course on Zoom, both during the fall and spring semesters. But I cannot say I liked that very much. I like the seminars on Zoom, but teaching on Zoom I did not like particularly.

[laugh] You couldn't see the students, and, you had no idea if they were reading the newspaper or were doing something else. [laugh]

ZIERLER: [laugh]

LEBOWITZ: Well, you do not get the kind of feedback you have in person, but it's not so bad now. In the last five or six weeks I've started to go to lunches at the Institute for Advanced Study. I meet there with some colleagues. So, I would say as far as science goes, at least in my particular case, the pandemic has had both positive and negative effects.

ZIERLER: Yes, yes.

LEBOWITZ: There are three of these statistical mechanics conferences that I organize twice yearly which had to be canceled. I'm hoping that the one in December will be able to take place in person, so I am looking forward to that. But I have Zoom meetings with collaborators, colleagues. I would guess almost every day of the week there's a different bunch. It's an amazing technology that you can do that, like we can have this interview. Where are you located at the moment?

ZIERLER: I'm not far from you. I'm in New Jersey, myself.

LEBOWITZ: Which part?

ZIERLER: In North New Jersey.

LEBOWITZ: Well, that's amazing. [laugh]

ZIERLER: Yeah. But I could be anywhere.

LEBOWITZ: [laugh] Yes, exactly.

ZIERLER: [laugh]

LEBOWITZ: I think it's the poor experimentalists who really suffer.

ZIERLER: That's right.

LEBOWITZ: They cannot do what they can do in the lab. But theoreticians—actually, what is your background? What do you do?

ZIERLER: I'm a historian of science.

LEBOWITZ: Where did you get your degree?

ZIERLER: Temple University in Philadelphia.

LEBOWITZ: I see, and how long ago was that?

ZIERLER: Let's see, in 2008, I received my PhD.

LEBOWITZ: I see.

ZIERLER: But, Joel, I'm interviewing you here. Let me ask the questions—

LEBOWITZ: [laugh]

ZIERLER: —about you. Are you still—

LEBOWITZ: OK.

ZIERLER: —involved with the Committee of Concerned Scientists?

LEBOWITZ: Oh, yes, very much so. I'm a co-chair of the committee.

ZIERLER: What's been happening there? There's so much to be concerned about in science. What's been some of your work there?

LEBOWITZ: Well, I think at the moment we have just been writing letters about people in Russia, people in Belarus, people in China, people in Iran, people in Israel. We have been very active in the sense of writing letters to different places. Very concerned about a medical doctor researcher, Djalali, Iranian and Swedish, who has been in jail in Iran for five or six years, and is officially under the sentence of death. It can be carried out any time. Just today, there was a letter to Belarus about 12 students and one professor who are imprisoned and on trial over there for peaceful demonstrations. So, there's a lot of—unfortunately—a lot of things going on still.

ZIERLER: And, Joel, you see a strong intersection between human rights and the ability to perform scientific research freely?

LEBOWITZ: Yes, human rights, of course, involve not only scientists but everyone. But a scientist can be more influential in helping colleagues in a fairly broad sense, and scholars and students are the concern of the Committee of Concerned Scientists. I think the students in Belarus, they're not particularly science students but the situation is the same.

There is—I can't remember now the name—a human rights lawyer in Russia who was again, put on trial for obviously false reasons, etc., like this all the times. And the Committee of Concerned Scientists has been writing letters about him.

And this Djalali in Iran, he's a scientist, epidemiologist who lived in Sweden but had joint citizenship, and went back to Iran for a conference. He has been imprisoned there and sentenced to death. Presumably, they want to exchange him for somebody Iranian convicted for planning a terror attack in Belgium. But, at the moment, he is in prison, and in very poor condition.

ZIERLER: Well, Joel, let's take it all the way back to the beginning. Let's go back to Europe, and let's start first with your parents. Tell me about them and where they're from.

LEBOWITZ: Well, my mother was born in the same village where I was born, Taceva. And when I was a little boy, her parents still lived there. Her father died when I was 7 or 8, maybe 9. But she had two sisters and her mother who lived there.

My father was born in a nearby town named Chust. And he had, I guess, one brother and two sisters and a mother who lived there when I grew up. Chust was about 25 miles from the place where I was born in Tech, name in Yiddish; Taceva in Czech when I was born.

ZIERLER: What was your father's profession?

LEBOWITZ: He had a store, a textile store, in the village. You could walk from one end to the other end of the village in about half an hour or less than that. So, the distance from our home to the store was probably about three minutes [laugh] of walking. It was not very far.

By the standards of our village, we were middle-class, we were not among the rich ones, and not among the poor ones, I guess, but reasonably well off as far as the standards went over there. When I was very little, we lived in a two-room apartment which was rented. After I was 7 or something like that, we moved into a larger three-room apartment. So we were already middle-class—

ZIERLER: [laugh]

LEBOWITZ: —at that time.

ZIERLER: Joel, did your parents come from more secular or more Orthodox backgrounds?

LEBOWITZ: Both of them came from very Orthodox backgrounds. In fact, 95% of the Jewish population in both places would be considered very Orthodox

ZIERLER: Were they more Hasidic or more Litvishe?

LEBOWITZ: I guess a little bit more Hasidic. They were not very Hasidic but my grandparents on my mother's side belonged to the Wishmitzer Hasidim, whereas my father's side was more Beltz. His father, whom I never knew because he emigrated from Chust to America before I was born, was certainly Orthodox. Everybody in those circles was. But I don't know how much he was Hasidic.

His oldest son, who also lived in the United States, my uncle, would say he was not really very Orthodox at all. I mean, he would have a kosher house more or less but I don't even know if he traveled on the Sabbath or things like that. I don't remember anymore. But certainly in the

village where I grew up, and where my father grew up, and my uncles and grandmother lived, the large majority of the Jewish population was very Orthodox.

ZIERLER: Joel, what was your first language?

LEBOWITZ: Yiddish.

ZIERLER: Did you speak Czech or did your family speak Czech also?

LEBOWITZ: No, the population in the village where I was born was mixed. It was about a population of roughly 10,000, of which 50% or so were Hungarian and spoke Hungarian, and 30% were Ruthenian and spoke Ruthenian, and 20% were Jews who spoke Yiddish. There were only a few Czechs altogether over there.

Before and during the First World War, our village was part of the Austrian-Hungarian empire, the Hungarian part. So, besides Yiddish, the next language was Hungarian, and Ruthenian. But I certainly didn't know anything but Yiddish when I was a small child.

I started going to school when I was aged 7 and the teachers spoke Czech there. But I think none of the students really understood it [laugh] because they were either Yiddish, Hungarian, or Ruthenian speaking.

ZIERLER: You went to cheder or to public school?

LEBOWITZ: I went to both, to cheder from the age of 3, to public school from the age of 7.

The public school was compulsory. I probably wouldn't have gone there if it was not compulsory. I don't think my father or mother went to any secular schools. But, they could speak Hungarian and maybe also some Ruthenian. But Yiddish was the only language spoken at home.

ZIERLER: The shul was small? It was like a shtiebel or it was larger?

LEBOWITZ: There were both kinds. The one my father attended formally was large, by our standards. It maybe had about 300 members or something like that. There was, in the same building a smaller shul, and then there were a few specifically Hasidic shuls.

We were 2,000 Jews, there must've been about 400 adult males going to the shul. The majority of those, maybe 60, 65% went to the big shul. When I went to Ukraine in the early 1990s, I took a trip back to my hometown, and the shul had been converted to a sports arena.

There were only a few Jews left over there, whom I did not meet. I did meet some relatives, second cousins in Chust, who were still there at that time.

ZIERLER: Joel, do you have a specific memory as a young boy when it became dangerous for the Jews?

LEBOWITZ: I remember as a boy, maybe age 7 or 8, hearing adults talk on the sidewalk about Hitler in Germany. And I remember actually thinking that this was some kind of an animal, some wild beast over there and then in 1938 Germany invaded and took over Czechoslovakia.

Initially, the part of Czechoslovakia called Carpathia, where I lived, was taken over by a fascist Ruthenian group. I was in second grade, and a few months into the school year we started being taught in Ukrainian instead of Czech. This only lasted for a few months, and then the Hungarians came in. I remember at one point my father was taken to the police station and roughened up, beaten up over there. I don't know for what. He was only kept there for a day or something like that.

Once Czechoslovakia disappeared, and the Hungarians came in, there was official anti-Semitism. Various kinds of restrictions gradually increasing on Jews. I continued going to school, often worried about being set upon by some of the Hungarian kids with dogs, and things like that. But life continued more or less the same until the early '40s, when restrictions gradually got more and more severe until 1944 when we were actually deported to concentration camps, to Auschwitz.

ZIERLER: How did that happen? What was that day like?

LEBOWITZ: It was kind of a gradual thing. It started out I think during the Passover holiday in March of 1944. There was a proclamation. Proclamations were still made by an official walking the street with a drum and reading an official proclamation, I do not know what this official's title was.

The first proclamation was that all the Jews who lived on the right side of the main street had to move over to the left side. My grandmother and aunt who lived on the right side of the street, the right side going in a certain direction, had to move in with us, and that lasted for a few weeks. Then there came another proclamation that all Jews had to move to a certain area of the town, which was then surrounded by barbed wire, and was a kind of a ghetto. You couldn't leave the area without some kind of permission. And that lasted for a few weeks.

And then one day, we were told to pack, and marched off to the train station, and put in wagons over there, and were taken to Auschwitz. So, it was a kind of gradual thing. Meanwhile, we had been hearing but not quite believing what was happening in Poland, where Jews were being killed in masses. But it was certainly almost unbelievable in some sense.

Elie Wiesel grew up about 20 miles from the place where I lived, in a place called Sighet which was Romania. At some point in the early '40s, a certain part of Romania, including Sighet, was taken over by Hungary. Wiesel describes in his biography and some other books he wrote what the situation was like in those times—pretty much similar to what it was in our place.

ZIERLER: 1944 is relatively late. Did you understand that going to Auschwitz meant likely a death sentence?

LEBOWITZ: No, we certainly did not, there had been labor camps for Jewish men since earlier in the '40s. They were under Hungarian auspices, and they were really labor camps. So, instead of being taken to the army, the Jews were taken to the labor camps. But they were given permission sometimes to come back home for a few weeks visit. One of my uncles was there. So we were thinking more in terms of that kind of a situation where we would be taken to some labor camps. Some people had premonitions, and, on the trains going to Auschwitz, cried and screamed. But I think we thought it was going to be terrible but we didn't think of directly going to extermination camps, as far as I can remember.

ZIERLER: Were you separated from your family immediately?

LEBOWITZ: Yes. The whole transport was separated into different parts. In one part were women with children and old people. In the other part were men who looked healthy and could work. I was 14 years old, and I went into the latter part with my father, and then I was asked how old I was. I lied and said I was 15 because I intuitively felt that this would keep me with my father.

My mother and sister went on one side with my grandmother and my aunts, while I went with my father. My father and I were together for several months, from the end of May until September. We were together in a satellite camp from Auschwitz where we worked mostly in the fields. And then went together to Auschwitz to the main camp. I am not sure any more on what basis my father and I and two or three other inmates from that satellite camp were sent to the main camp.

My father, partly to avoid having to work on the Jewish holidays, Yom Kippur, he was also not in great health, went to the hospital. From there, he was taken to the crematorium to be gassed and killed. That was in September 1944. I stayed on in the camp until we were marched out at the beginning of January 1945 when the Russian troops were coming near to Auschwitz.

ZIERLER: When did you learn of the fate of your mother and sister?

LEBOWITZ: Well, it was no secret once we got to the camp that there were gas chambers where those unfit to work were gassed and burned in the crematoriums.

The entrance to the main camp had a sign "Arbeit mach das leben frei" to which the refrain among the inmates was "in crematorium number drei".

When we got to Auschwitz from the Satellite camp, part of the main camp was fenced off and had woman prisoners. One was not sure whether one's mother or relatives had survived and were also in a camp or murdered. The full extend of what happened did not become known until after May 1945 when Germany was defeated. When we started seeing people, relatives or acquaintances who survived, the first question was, who survived and who didn't.

An aunt of mine, my father's sister, did survive with her daughter and son, and lived until 2005.

But my mother and sister did not survive. I think they were immediately taken to the crematorium to be killed as soon as we arrived, together with grandmother and aunts, uncles and cousins. Actually from my mother's immediate family nobody survived.

From my father's family, one of his brothers and his sister did survive. And, I guess, about eight or nine cousins on my father's side. I believe that I'm the last one of the cousins who survived from before the war. There is one cousin I haven't been in touch with. She lives in Israel, and I actually don't know if she's alive or not. I haven't seen her for about six or seven years.

ZIERLER: Joel, growing up, did you have *yirah shamayim* and *emunah* in *Hashem*?

LEBOWITZ: Did I have what?

ZIERLER: *Yirah shamayim* and *emunah* in *Hashem*, a fear of heaven and a faith in God?

LEBOWITZ: Yes. It was like—what was the expression you used? I didn't recognize it.

ZIERLER: *Yirah*, fear—

LEBOWITZ: Yeah, *yirah*, I understand.

ZIERLER: —*shamayim*—

LEBOWITZ: Yes, OK, I understand. I didn't catch it.

ZIERLER: —*emunah*—

LEBOWITZ: *Emunah*, oh, yes, I grew up, as I said, very Orthodox, went to the cheder and Yeshiva, I did not really become secular until coming to the United States. When I came to the

United States in 1946, I still had sidelocks, peyos. But my American cousin, one of my uncles had come to the United States, a little bit after my grandfather, my father's father. My cousin was not religious at all, and he took me to the barbershop in Queens where they cut off my sidelocks but I still went to a religious school for several years after I came to the United States.

ZIERLER: Joel, what role do you think the Nazis played in your decision to become secular, if any?

LEBOWITZ: Since it's very hard to know what would have happened [laugh] without the Nazis, I cannot tell.

ZIERLER: In other words, maybe as a young boy, if you had scientific sensibilities that made you doubt the existence of God or things like that?

LEBOWITZ: It certainly might have. I don't remember having any specific doubts before going to Auschwitz. Whether I might have had or not, I don't know.

ZIERLER: What did you do immediately after Auschwitz was liberated?

LEBOWITZ: I was liberated in May 1945, in the Bergen-Belsen concentration camp and stayed around Bergen-Belsen, for another six or seven weeks. For a few weeks I was in a hospital set up by the British, just because of being really what we used to call a skeleton man. I probably weighed less than a hundred pounds. After a few weeks the British set up a provisional holding camp for people from the concentration camps. After a short while we were sent back to Czechoslovakia, which was under Russian occupation. The trip to Czechoslovakia was by British military buses.

After arriving in Czechoslovakia we were put on overcrowded trains heading East. I do not remember who was in charge. After some unpleasant experiences with Soviet troops I finally arrived in Budapest.

In Budapest, the capital of Hungary, which was also occupied by Soviet troops I met other concentration camp survivors and became fully aware of the murder of almost all of my family by the Nazis. While in Budapest we were taken care of by the American Joint Distribution Committee. There I met up with the uncle and aunt who survived, my father's brother and sister.

I stayed in Budapest for ten days or so and then headed "home" to Taceva which was now part of the Soviet Union. Of course I found almost no Jews there.

Meanwhile my aunt had set up house, with her two surviving children in the town of Szatmer, Romania. So I went to live with them for the rest of the Summer. In the Fall my aunt left for Czechoslovakia but I stayed behind in a Hasidic Yeshiva in Romania until the Spring of '46.

In April or May of 1946 I went from the Romania to Czechoslovakia, illegally crossing several borders without being caught, anyway, and planning to go to Israel via Belgium, with the idea of learning a diamond-polishing trade. This was big in Belgium because they had the Congo where they got the diamonds, and Israel was developing a diamond industry. Then I got caught at the border of Luxemburg and Belgium.

I think it's described actually probably in this article which I sent you that was published *European Journal H*.

After being detained briefly I went back to a displaced person camp in Germany, stayed there for several months in an Orthodox Yeshiva. From there I went to the United States arriving there by

boat on Labor Day weekend of 1946. By that time I was not so religious, as I said. I did not object to having my peyos cut off when my American cousin took me to a barber shop in Queens but still stayed in Yeshiva for another two years and got my high school diploma in the Yeshiva before leaving Yeshiva and going to Brooklyn College.

ZIERLER: When did you realize that you had special talents in mathematics? Was that even in Europe, or that only developed in the United States?

LEBOWITZ: Well, I think I was quite good at math in public school. The teacher would ask questions, and I would yell out answers. I was also tested by relatives asking how much is 9 times 13, etc., kind of questions like that. I was good at this stuff. So, I was considered as a bright boy.

But obviously I did not know real mathematics. I remember reading one science book, I think it was in Yiddish which particularly impressed me. It told stories about people diving into the sea, the air pressure keeping the water out of a diving bell.

In high school in the US there were “Regents” examinations on which I got a very good grade in mathematics. In Brooklyn College, I had some very good teachers and I became sort of converted to theoretical physics after first planning to do engineering, just for a living.

ZIERLER: When did you realize that you could pursue math in graduate school, and perhaps even become a mathematician as a career?

LEBOWITZ: By the time I finished college I was applying to graduate schools in physics and my teacher, Melba Phillips, and some other teachers were very supportive in that. And I did pretty well in the mathematics at the school. So, I guess it was sort of gradually during college. I

started in Brooklyn College, going just at night, still being at the Yeshiva during the daytime. Then I switched to full-time, and I did alright in the science courses and mathematics. It was during the last two years of college, when I got converted to a theoretical physicist.

ZIERLER: What was it? Was it a class? Was it a professor? Was it a concept?

LEBOWITZ: An important part was being good at it, or being reasonably good at it, and having quite a few very good professors, particularly Melba Phillips, who was a theoretical physicist, a student or a postdoc of Robert Oppenheimer. And I enjoyed her class, and she was supportive.

But, also, the other teachers—it was really a very good group of teachers in Brooklyn College. During the Depression, Brooklyn College managed to hire some very good people to its faculty, and it was very stimulating. My decision to become a scientist was gradual. It was not a sudden vision or anything like that as far as I can remember.

ZIERLER: What were the most interesting theories in physics at that point that captured your attention?

LEBOWITZ: I can't say I remember. There was a course in college level atomic physics. I don't think we actually learned the Schrödinger equation in college. With Melba Phillips I took the courses in electromagnetism and statistical mechanics. And there was a course in nuclear physics with a guy named Rarita who had worked with Schwinger at some point. He was doing the nuclear physics.

I think it wasn't until getting to graduate school that I became really aware of modern physics. My thesis advisor, Peter Bergmann, worked in general relativity and statistical mechanics. In graduate school I also did some work with Eugene Gross on some quantum mechanical problem.

But my thesis, both master's thesis and PhD thesis, were on classical subjects, not involving quantum mechanics.

Of course, in graduate school, I took courses in quantum mechanics. But most of my life has really been in the classical domain, as you would say, and only occasionally did I do something in quantum mechanics. But I wasn't really, and I guess I'm still not, a real quantum mechanics type of person. I was more interested mostly in what could be called classical physics.

ZIERLER: Joel, how mathematical was your thesis research?

LEBOWITZ: Oh, it turned out to be quite mathematical in the end, involving probability theory. I still use my thesis in some work at the moment. Surprisingly some people have started using it a few years ago actually for doing computer simulations. My thesis actually contained mathematical theorems which are still relevant. During my thesis research I read a probability book by Doob and used it to prove some results about nonequilibrium statistical mechanics.

ZIERLER: What did you do after your thesis? What kinds of postdocs were interesting to you?

LEBOWITZ: Well, certainly statistical mechanics, I was able to get an NSF postdoctoral fellowship to work with Lars Onsager at Yale University. I forget now whether he already had the Nobel Prize or did not, but he was certainly a very important figure in statistical mechanics. He was actually officially in the chemistry department, maybe with some joint appointment in physics or something like that. And, so, I spent a year as a postdoc with Lars Onsager at Yale, which was not as good as I had hoped. I was interested in Onsager's work on statistical mechanics, nonequilibrium statistical mechanics, but he was at that time interested more in liquid

helium or quantum mechanics. So, I did some work with him on that, but nothing of much interest.

Onsager was somebody who proved things, like the exact solution of the Ising model, and things of that kind. And then I got my first job at Stevens Institute of Technology in the physics department but, again, I was more mathematically inclined. Beginning with my thesis, I was really using serious mathematics and I took advanced graduate math courses when I was at Syracuse with some very good mathematics people at that time there.

ZIERLER: Joel, more broadly, what were some of the major questions in statistical mechanics at this time?

LEBOWITZ: People were not absolutely clear about phase transitions in equilibrium statistical mechanics. There were still questions of whether the same Hamiltonian gives you both the gas and the liquid. It was not totally clear. It was only when Onsager solved the Ising model, which showed mathematically that you can have a phase transition for a system with specified Hamiltonian. It wasn't that the Hamiltonian changed. It is just that the behavior of the system changed as a function of temperature. When I was still a first-year graduate student. I remember Mark Kac giving a colloquium at Syracuse. He described the Onsager solution of the Ising model saying in picturesque language that Onsager threw all the machinery at it, and the problem collapsed.

And then there was, I remember an internal seminar, where a graduate student was describing the work of Lee and Yang about the location of zeroes for the Ising ferromagnet. Both of these mathematical results certainly affected my interest in rigorous mathematics applied to physical problems. My thesis was about nonequilibrium statistical mechanics trying to describe the

stationary states of systems in contact with thermal reservoirs at different temperatures. Those were certainly the open problems: phase transitions in equilibrium systems, and time evolution and steady state of nonequilibrium systems. Still pretty much the same problems today. [laugh]

ZIERLER: Joel, what were your motivations in creating the statistical mechanics conferences starting in 1959?

LEBOWITZ: Well, I was at that time at Stevens Institute of Technology and some colleagues there had organized a one-day meeting about general relativity. It was actually students of Peter Bergmann, Jim Anderson and Ralph Schiller, and that gave me the idea that it might be nice to have something like that in statistical mechanics. That was where the idea of the meetings originated. I was at Stevens from '57 to '59, and the first of the statistical mechanics conferences was at Stevens in the Spring of 1959.

Stevens was close to Bell Labs, and so people I was working with at Bell Labs came to the meeting, I can't remember now the details. Once I left Stevens to start what was then the beginning of a graduate school in science at Yeshiva University in New York, I just continued it. When asked about the longevity of these meetings I like to quote from the opening of the "Tale of the Monkey" which describes how the Buddhist teachings were brought from India to China. The book starts out with the statement that even a 1,000-mile journey begins with one step. Just follow one step after the other.

And, so, when I got to Yeshiva, I decided to continue the one-day statistical mechanics meeting. At that time Yeshiva didn't have any rooms suitable for the meeting. Our new graduate school had only some rooms in a prefab. So, the first meeting was actually held at Stern College for Women, which was part of Yeshiva University in downtown Manhattan. And then I had one of

the meetings at the Albert Einstein College of Medicine of Yeshiva University. The graduate school then moved in to a whole floor of a building at the corner of Amsterdam Avenue, and 188th Street, and we actually started having the conferences there.

I tried to get some records together for the 100th conference which took place in December 2008. In the beginning we used to send out postcards, telling people, that a statistical mechanics conference will take place on a given day. And people came from Rockefeller University, NYU, Stevens, Bell Labs, etc. I remember C.N. Yang, at Stonybrook at that time, tried to persuade me to move the meeting there, but I resisted.

And it was just a one-day affair in the beginning. It was people giving short talks, yeah. And then later the graduate school became the Belfer Graduate School of Science at Yeshiva University, and we moved to a new seven-story building which had plenty of room for conferences which became quite large. Statistical mechanics was flourishing at that time with Ken Wilson's renormalization group, Kac, Uhlenbleck, and Hemmer's derivation of the van-der-Waals equation of state, and the development of new techniques for getting critical exponents, and experiments measuring these exponents.

So, the meeting got to be quite large, and sometimes there wasn't even enough time for people to give five-minute talks. I had to ask potential speakers to give me their IQ, intensity quotient. How desperately do you want to give a talk? If your intensity quotient was low, you just stood up in your seat, and spoke for two or three minutes. So, that got to be quite something.

At that time, people would come down from Cornell with a bus. At Cornell there was Michael Fisher and Ben Widom, who had many students, all very active, people also came from Boston. I remember Gene Stanley, as a graduate student or postdoc at MIT, was talking about low

temperature expansion for rotator models. I also remember Rodney Baxter describing his exact solutions of various spin systems. It was quite exciting [laugh]. David Chandler, a chemist from Berkeley, who unfortunately died young, remembered sitting in this lecture hall with a gentleman falling asleep on his shoulder, snoring. It turned out to be Lars Onsager. [laugh]

ZIERLER: [laugh]

LEBOWITZ: There were many participants but it was all just a five-minute or shorter than five-minute talks. Then I decided to expand it and have an extra day for long talks with rigorous results, and so that got into a two day meeting. Later when I moved to Rutgers after being at Yeshiva for 18 years, from 1959 to 1977, I moved the meeting to Rutgers, and, again, people—some people objected to this, it being much more difficult to get to Rutgers than it was to get to New York City.

ZIERLER: [laugh]

LEBOWITZ: But, anyway, so, it just continued. I still remember one time when I was spending a sabbatical in Paris at the IHES, Institut des Hautes Études Scientifiques, and flying back for the meeting, thinking, OK, that maybe I would stop at 50. But I just kept on going. I just put this one foot in front of the other—

ZIERLER: [laugh]

LEBOWITZ: —and keep on.

ZIERLER: Joel, when did you first meet Elliott Lieb, and decide to collaborate with him on the Coulomb forces?

LEBOWITZ: Oh, we had met earlier at an American Physical Society meeting in Washington, or at least I heard him talk over there. I was impressed, and then I got him to join Yeshiva University, and he was a faculty member there. We started collaborating at that time. And then on the Coulomb system, I think we started collaborating when we were both at a conference in Irvine, California, in 1968. By that time he had already actually left Yeshiva. He was at Northeastern University in Boston.

ZIERLER: What were some of the advances at that point? What was significant about your collaboration with Elliott?

LEBOWITZ: I should have mentioned it earlier that at that time, the proof of the existence of the so-called thermodynamic limit was certainly one of the important open problems. This limit corresponds to where the number of particles goes to infinity, the volume and the energy go to infinity while the number of particles per unit of volume and the energy per particle stay finite. The question was: does this limit actually exist and give you correct thermodynamic functions with the right properties. It is only in that kind of limit that you get phase transitions. When the system is finite, we never get mathematically singular behavior such as exists at a phase transition. It was van Hove and then Ruelle and Fisher who were able to prove existence of the thermodynamic limit for systems with short-range potentials like Lennard-Jones potentials, yes, so, that was an exciting development actually at the early stage.

In these proofs by van Hove in one dimension, and by Ruelle and Fisher in higher dimensions, it was very essential that the interaction potential decay faster than the inverse of the distance between particles raised to the power of the space dimension. But, of course, the Coulomb interaction does not decay that fast, and everybody understood that for real systems the Coulomb

interaction is the crucial one. So, it was very much an open problem at that time to prove the existence of the thermodynamic limit for Coulomb systems. This is what Lieb and I did in 1968.

ZIERLER: Joel, did you cross paths with Lenny Susskind at all during your Yeshiva years?

LEBOWITZ: Oh, yes, very much so. I mean, I think I was the chair of the Yeshiva University physics department at the time when Lenny came. Lenny describes his coming to Yeshiva in his book. He mentions all the people who were there and David Finkelstein was the one who brought Lenny to Yeshiva.

David Finkelstein was a brilliant theoretical physicist, no longer alive, who worked in general relativity, foundations of quantum mechanics, plasma physics, and even did experiments. He died some years ago. He had moved to Georgia Tech, Atlanta. But he is the one who brought Lenny to Yeshiva. I forget for how many years Lenny stayed at Yeshiva but we have remained friends since then. In fact Lenny recently talked at a seminar I run on Zoom every Wednesday.

ZIERLER: Did you get an early view on string theory through Lenny at that point, and was string theory interesting to you in the early years?

LEBOWITZ: Not really, no. I don't even know if Lenny was doing string theory at the time when he was at Yeshiva. I went to Yeshiva in '59 and Lenny must have come somewhere in the middle '60s to Yeshiva where he only stayed two or three years before he left first for Tel Aviv and then to Stanford.

I don't remember any scientific discussion with him at Yeshiva. I believe that my first scientific discussions with Lenny were many, many years later at the Newton Institute in Cambridge when

we were both there for different programs. We talked there, I guess, on statistical mechanics, black holes, and things like that. That was much, much later.

ZIERLER: Joel, tell me about your work on Ising spin systems at this point. You were using Monte Carlo simulation.

LEBOWITZ: I never did myself any Monte Carlo calculation. I had collaborators who were the ones doing simulation of spin systems. But before then already, I did inequalities extending the Griffith, Kelly, Sherman and other inequalities. I remember that it was 1974.

In terms of what we were talking about before it was the existence of the thermodynamic limit, and actually proof of convergence of low density expansions that I was working on then. The proof of convergence of these expansions was done by Hans Groeneveld, a young fellow from the Netherlands and also separately by David Ruelle and Oliver Penrose. I remember driving from New York to Princeton, where David Ruelle was a visitor at the Institute for Advanced Study, to discuss things with him. I think it was the first time I met him. I had several people in the car with me. I was interested in understanding Ruelle's papers, which were very mathematical and fairly difficult to read by physicists like me.

I remember being asked to referee a paper of David Ruelle's for the *Journal of Mathematical Physics*, and writing in my referee report that he should write it in such a way that it would be understandable to what I called SMMIT, an acronym for the "statistical mechanics man in the street". Ruelle was much better at explaining it in person in terms, which I didn't know at that time, of Banach spaces.

This was the first time I was at the IAS. The second time I went there, it was also to visit David Ruelle, at that time there was another visitor at the institute it was Jean Ginibre, who had just proven some very nice inequalities later known as the FKG (Fortuin, Kasteleyn, Ginibre inequalities). And I became quite interested at that time in inequalities for the Ising model.

Coming back to your question about my work on a Monte Carlo algorithm. This has been very popular in terms of references. I had also, at the same time, gotten involved with the Courant Institute where they had, at that time, one of the biggest computers for scientific research. It was a CDC 6600 or some name like that. And I started working on various problems with Mal Kalos and with some graduate students and postdocs. Then we came up with this algorithm for a Monte Carlo simulation, which speeded things up.

We were mostly interested in phase segregation in Ising spins or in general binary solids. You start out with a homogeneous system, and then you suddenly lower the temperature. The system will try to segregate into two different parts. In the case of the Ising model it will break up into a region with positive magnetization and a region with negative magnetization, or if you had a binary solid like tin and aluminum it will segregate into an aluminum rich and a tin rich region. We carried out computer simulations on that. That was the origin of that paper, and the algorithm. This was all classical.

On the other hand the existence of thermodynamic limit for Coulomb system was quantum mechanics. I also did together with Aharonov and Bergmann something about time symmetry in quantum mechanics, which has been of some interest. Aharonov has been developing various aspects of that work.

ZIERLER: Joel, tell me about the circumstances that allowed you to meet Andrei Sakharov.

LEBOWITZ: Before I moved to Rutgers in '77, I had been active in the New York Academy of Sciences. They used to have every month a seminar in physics. While I was still at Yeshiva an astronomer at Columbia, Lloyd Motz, got me organizing these seminars. I remember having people like Freeman Dyson, Julian Schwinger, Ilya Prigogine, and people like that giving seminars.

From '76 to '77 I went for a sabbatical in France at the Institut at Hautes Études Scientifiques where Ruelle was and came back in '77 to the position at Rutgers. The Belfer Graduate School of Sciences at Yeshiva University was more or less closing down because they didn't have any funds. It was after the Six-Day War in Israel, and people who were supporting Yeshiva University switched their support more to Israel, so that they were running out of funds. And, so, I looked for a job, and got one at Rutgers.

In 1977 I was nominated by the board of the New York Academy of Sciences to run for president of the New York Academy. There wasn't much competition in those times, so if you got nominated, you got elected. I was president-elect in '78 and then president in 1979.

I was asked, you know, as a candidate to write what would be my priorities. What did I want to do as president of the New York Academy of Sciences? And it was just at that time when the cause of the first Refuseniks of the Soviet Union was becoming popular, or at least known. In particular, I remember at some point going to a reception for Benjamin Levit, who was one of the first established well-known scientists who was permitted to leave the Soviet Union. I got interested in that, and so one of the things I wrote what I would like to do at the New York Academy of Sciences is get involved in the human rights of the Refusenik scientists.

At that time there were conferences organized by the Refuseniks. The first one was in '77. I was not involved in that. But then I moved to Rutgers, and in 1978, there was going to be another meeting in Moscow of the Refusenik scientists. I was invited to go there by Dorothy Hirsch who was the director of the Committee of Concerned Scientists at that time. I decided to go. It was to be held in December of '78. Several people from the United States including me and Jim Langer went there. There were people from France and from Sweden. I don't remember what other place.

I went there with Jim Langer, officially with a tourist group going over the winter holidays, between Christmas and New Year, to the Soviet Union. Trips to the Soviet Union were always organized by In Tourist, a Soviet government agency for dealing with tourists. Our first stop was Leningrad, which is Saint Petersburg now. It turned out that there were some other people on the tour who carried some Jewish books to give there. They were held up briefly at the airport and the books confiscated. But I didn't have any trouble.

We stayed in Leningrad for a few days, visited the museums and did sight seeing. Then we went to Moscow. In Moscow we went to the house of Victor Brailovsky where the Refusenik seminar was to be held. There is where I first met Sakharov. He told us that he was contemplating a hunger strike because his wife, Elena Bonner, was refused permission to go to have her glaucoma treated in Italy where she had been permitted to go once before.

Sakharov then invited me and Jim to come to his house Friday evening. Fortunately, by that time, his wife had gotten permission to go to Italy for her eye treatment. So, it was kind of a celebration dinner. That's where I got to know Sakharov. I was very impressed and consider him as the greatest person I've met.

ZIERLER: When did you start collaborating with Spohn?

LEBOWITZ: Oh, Spohn came as a postdoctoral fellow just before I left Yeshiva. In fact, he came early in '76, before I went off for the sabbatical to Paris. I had met him earlier at a conference in Bielefeld in Germany. He was just finishing his PhD and was interested in coming to work with me. And then he got some German fellowship, and he came to New York, and we started collaborating then.

When I moved to Rutgers, he stayed on and had moved to Princeton. So, that's where we started. We haven't collaborated over a long time but are still very good friends. He, like many of my postdocs and students, maybe even students of my students, have retired [laugh] already. But I am still not.

ZIERLER: [laugh] It's a long career.

LEBOWITZ: Yes.

ZIERLER: Joel, tell me about some of your work applying statistical mechanics to the nonlinear Schrödinger equation.

LEBOWITZ: I used to visit Los Alamos. At some point, I was on some advisory committee for theoretical physics at Los Alamos. I was put on that committee by Peter Carruthers, then chair of the theoretical physics division at Los Alamos. During my visits I developed some good friends there.

It was there that I became aware, talking with Harvey Rose—and Don Du Bois—who were working in plasma physics about the nonlinear Schrödinger equation (NLS). The connection

between plasma physics and the NLS was based on some work by Zakharov, a Russian physicist, who later was at the university of Arizona. He developed some equations for electromagnetic waves in a plasma, which under some conditions reduced to what would correspond to the focusing NLS. So, that was the origin of my interest.

Then I worked on it at Rutgers with Gene Speer, with whom I'm still collaborating very much now, and Harvey Rose from Los Alamos. We developed a statistical mechanical model of the NLS. The problem we studied was the following: given a Hamiltonian corresponding to the focusing NLS, how would you construct a canonical ensemble for that Hamiltonian? Problem being that that Hamiltonian did not have a lower bound and the energy could become infinitely negative. The technology, which worked for proving infinite volume limits and things like that for regular systems, like fluids with Lennard-Jones potential or Coulomb systems did not apply. So, we had to work out some new way to deal with this problem. That was really interesting. And sort of on and off I've continued to come back to that problem, various aspects of it, with different collaborators.

ZIERLER: [laugh] Joel, what were some of the connections that you saw between Navier-Stokes and the Boltzmann equation?

LEBOWITZ: Well, one thinks of the Boltzmann equation as a more microscopic theory from which one should derive macroscopic equations, like the Navier-Stokes equation. In fact that was done via the so-called Chapman-Enskog expansion. Chapman from Britain, and Enskog from Sweden, developed a method to get Navier-Stokes equations from the Boltzmann equation by doing an expansion, essentially in the time between collisions. If the time is short, then to leading order, you get the Navier-Stokes equation. There is also a Hilbert expansion, which is somewhat

different but also gives the equations. But these were all non-rigorous derivations because they involved an expansion which is not convergent. And, so, what we did at some point, inspired by something from Uriel Frisch, a French physicist, who made the observation that in a certain kind of scaling, the Navier-Stokes equation should come out directly from the Boltzmann equation in a rigorous way. And that's what we were able to prove together with Anna DeMassi and Raffaello Esposito.

How to go from microscopic dynamics to macroscopic equations is one of the key problems in nonequilibrium statistical mechanics. You want to derive equations like the Navier-Stokes hydrodynamic equations. There is no rigorous derivation, in fact, at the present time of the Navier-Stokes equations from Hamiltonian microscopic dynamics. The Boltzmann equation, is an intermediate mesoscopic equation.

It's, at least for gasses, a much more microscopic description than what the Navier-Stokes equation is. The Navier-Stokes equation does not involve the velocities of the particles, while in the Boltzmann equation the velocities are very crucial. So, it's a very natural question, how to derive a macroscopic equation like Navier-Stokes from the Boltzmann equation. We were able to do that in a certain kind of a scaling limit in which we scale space and time. The velocities also had to be scaled. I think it's still one of the important open problems of how to derive these equations.

ZIERLER: What was some of your work on deriving Ohm's Law?

LEBOWITZ: That was a bit later. There was some work by Bill Hoover and Bill Moran who were at Los Alamos at that time. They considered the so-called thermostated dynamics for a particle moving among fixed scatterers, and you have also an external electric field acting on it.

If you just look at the Hamiltonian dynamics, you have a periodic box with some fixed scatterers, the so-called Sinai billiard, and you apply the electric field you would accelerate the particle and it would keep on gaining energy from the field. So you can ask what happens if you restrict the motion of a particle to a fixed energy surface. That's what's called a thermostated system. It's like you have Hamiltonian evolution but confined to a surface of fixed total energy.

Hoover and Moran studied that model numerically and they found very strange, interesting behavior. It looked like the stationary state of that system was very fractal on the energy surface.

You could map it into a two-dimensional, so-called Poincare cross-section, which was very fractal-looking. They published that paper in the *Journal of Statistical Physics*, of which I was editor at that time. I was quite interested in seeing whether one can prove something rigorously about that model.

Sinai had proven a lot of things about a particle moving among fixed scatterers. But it wasn't applicable to when you apply an electric field, and so we started discussing that. And then with the help of Nikolai Chernov, a former student of Sinai, who was at that time in California, and a postdoc of mine at that time, Gregory Eyink, we were able to prove rigorously the existence of a stationary state, and that the current in that stationary state indeed satisfied Ohm's Law. When the electric field was small, you got the current proportional to the electric field with a coefficient one could compute. So, again, this is a stationary state for a nonequilibrium system, which is a thing I worked on many times in many aspects.

ZIERLER: Joel, do you have a sense chronologically when your work on ferromagnetic Ising models became known as the Lebowitz inequalities?

LEBOWITZ: I think that was towards the end of my stay at Yeshiva, and that has also caught the interest of people working in field theory. It proved inequalities for four-point correlation functions. I don't know who first called it Lebowitz inequalities. I hope I was too modest to call it. [laugh]

ZIERLER: [laugh]

LEBOWITZ: So, somebody else must've called it. I remember Tom Spencer and Chuck Newman who were at that time both at Courant coming to Yeshiva to discuss those inequalities. That was nice. [laugh] I like those inequalities.

ZIERLER: [laugh] Joel, I'm curious how you applied Gaussian fluctuation in your work on random matrices.

LEBOWITZ: Oh, again, that's an interesting story in some ways. Mehta, an Indian physicist who worked in France, wrote a book on random matrices, one of the early books. He visited Rutgers, and got me interested, and we discussed about it. I was particularly interested in random matrices corresponding to one-dimensional systems interacting with a two-dimensional logarithmic Coulomb interaction.

For the Coulomb system in two dimensions, so-called Jellium, you have just one component: a system of electrons in a uniform positive background. Ginibre showed that the correlation functions for that system are the same as you obtain from Gaussian random matrices. This is true at a particular temperature when you could compute the correlation functions by considering Gaussian random matrices.

Independently Bernard Jancovici in Paris proved an equivalence between the Coulomb system and Gaussian random matrices. When Mehta visited Rutgers, I got interested in that connection and I suggested to Ovidiu Costin, a graduate student at that time, to prove that for a one-dimensional system with logarithmic interactions which corresponded to random matrices that the fluctuations were indeed Gaussian by looking at the moments. If you have a Gaussian process, then the first two moments are sufficient to describe the whole process. The higher correlation functions can be described in terms of the one-body and two-body correlations. I worked with Ovidiu on that, trying to see if we could prove such a result. And we, mostly Ovidiu, succeeded in showing that the fluctuations are Gaussian.

ZIERLER: Now, at the same time, you came to work with Freeman Dyson on non-Gaussian energy level statistics. How did that come about?

LEBOWITZ: My working on the problem came from a discussion with Sinai in Moscow. [laugh] I was visiting Moscow, still mostly for the Refusenik things, but meanwhile I had met Sinai before in Paris when I was there from '76 to '77. And he was telling me about looking at the distribution of lattice points in thin shells in two dimensions.

Take a circle of radius R_1 and another circle of radius R_2 bigger than R_1 and ask how many lattice points there are in that annulus. That was a question that Sinai was working on. When I came back, I asked a postdoc to investigate this numerically. I expected at first that in the limit when the radius becomes large that the distribution of lattice points in a thin shell would be Gaussian.

Now, the problem of the number of lattice points in a disc of radius R with the center its origin goes back to Gauss, he proved certain results about the variance as you look at a different R .

What we were doing different was looking at discs, not with a center that's origin but with the center at an arbitrary point in the unit square. To leading order the number of lattice points is just equal to the area.

Then the question is what is the variance when R is the radius becomes very large. And, as I say, we thought it was Gaussian. In I told Freeman what we were doing and the kind of results we were getting, and then I remember getting a letter from him saying, "No, they are not Gaussian at all, these fluctuations."

He could compute the third moment, which should have vanished for a Gaussian. It didn't vanish. He was able to show that it did not vanish, this one. So, then we got to really working more seriously on it, and enlisted Pavel Bleher, who was at that time a visitor at the institute, and he had spent some time at Rutgers before then. He was a student of Sinai's, and I had met him already in Moscow at one point.

So, that was the origin of that problem. [laugh] Yeah, an interesting connection. I remember going to lunch in the house of Sinai in Moscow when he was telling me about the problem he was working on, so, there's a lot to be said for personal meetings. [laugh]

ZIERLER: Yes.

LEBOWITZ: [laugh]

ZIERLER: Joel, what was some of the value in applying a macroscopic description in phase segregations?

LEBOWITZ: This is a classical problem. You quench a binary alloy and you see the system segregating. This is usually described macroscopically by the Cahn-Hilliard equation. We were trying to solve that equation, which is difficult, and mostly what we could do was simulations.

I had a connection with the Courant Institute, where they had this big computer, and I just wanted to see what was happening during this segregation. Guided by the simulations we were able to find some interesting scaling relation connected with the Cahn-Hilliard equation. This is a problem in nonequilibrium statistical mechanics: you start with an alloy at high temperature, the system is uniform, and then at low temperature, the system segregates. Such a phase segregation is of interest in metallurgy.

Steel is among such alloys which have a tendency to segregate. This is not very good for airplane wings. You don't want the alloys to segregate and cause cracks. So, that was a problem which John Cahn studied. He was a metallurgist and also brilliant mathematically.

You mentioned earlier physical mathematics. The Cahn-Hilliard equation would be an example. It gives an approximate but good description of what happens when you quench a system which will segregate. For me, it was just basically a problem of nonequilibrium statistical mechanics.

ZIERLER: Subsequently, Joel, what was some of your work on stochastic dynamics?

LEBOWITZ: I have worked on various aspects of such systems starting with my thesis where I studied a system in contact with several temperature reservoirs. We had to model the contact with the reservoirs. We did this by having a stochastic interaction. The reservoir was modeled as being infinite. Particles from the reservoir came and interacted with the system and then went

away so that the reservoir was stationary and had a stochastic influence on the system. My thesis is essentially a description of that situation.

Later I worked with Spohn and Katz on stochastic lattice gas models of fast ionic conductors. I heard of these at some conference about ionic conductors where one kind of atom can move quite freely in the solid. Then together with Spohn and Katz we devised a stochastic model describing that.

We had particles on a lattice which could jump from one lattice site to another one in a random way. These types of models were developed in probability theory first by Frank Spitzer from Cornell and then studied by Tom Liggett from UCLA, and others. Liggett wrote a book, *Stochastic Dynamics* where he studies the so-called simple exclusion process, namely, you have some particles on lattice sites which, with a certain rate, jump to neighboring sites if those sites are empty. That model came purely from the mathematical side. I forget now how I had gotten a copy of Liggett's book before it was published.

I was thinking of that model to describe these ionic conductors while driving from Princeton to New Brunswick with Spohn. And then Katz did computer simulations on these models. I have been very much involved with stochastic lattice models since then.

Many things that you cannot do for Hamiltonian dynamics, like derive macroscopic equations, such as the diffusion equation, for example, you can do for stochastic lattice models. Many people, particularly Raghu Varadhan from NYU and his collaborators have done a lot of work on these models going from microscopic to macroscopic.

These lattice systems are now used to model various physical situations like the spread of an epidemic. I am right now working on something called facilitated exclusion processes. That's where in order for a particle to jump to a neighboring site, it has to have an occupied neighbor.

ZIERLER: Joel, I'm curious what you have seen as solvable and not solvable in looking at nonequilibrium systems.

LEBOWITZ: They're mostly non-solvable. [laugh]

ZIERLER: [laugh]

LEBOWITZ: Fortunately there are some exceptions. These lattice models can be in a stationary nonequilibrium state by having them in contact with reservoirs at different chemical potentials or densities. And the question was, what about fluctuations in those systems? In equilibrium systems, fluctuations are related to the entropy, via the so-called Einstein relation, the probability of something happening on the macroscopic scale is related to the variation in the entropy of the system.

In nonequilibrium, there is no a priori theory of such fluctuations, and, in particular, so-called large deviations when you have not just fluctuations but large changes. Then, if the system is in contact with two different density reservoirs, you have in the steady-state a uniform density gradient. But you can ask the question, what's the probability of having a very large deviation from that uniform gradient density profile, such as a gradient in the opposite direction?

Together with Bernard Derrida and Eugene Speer, we were able to solve that problem for one-dimensional stochastic lattice models. This work was later expanded very much, particularly by Gianni Jona-Lassinio from Rome, and collaborators who did it for general so-called diffusive

systems and by Derrida with other collaborators doing it for large deviations in the current. This was something which, unfortunately, one cannot do for systems evolving according to Hamiltonian dynamics or according to the quantum Schrödinger equation.

ZIERLER: Joel, what is canonical typicality? I'm not familiar with that.

LEBOWITZ: Well, usually, one derives the canonical ensemble for a subsystem of a large system described by a microcanonical ensemble. Maybe you're familiar with that?

ZIERLER: Mm-hmm.

LEBOWITZ: OK. Here is what you do. You start out with a microcanonical ensemble for a large system. Then you look at the subsystem and integrate out over the degrees of freedom of the rest of the large system and you get a probability distribution for the subsystem. If the interactions between the subsystem and the rest of the large system is weak, you get essentially a canonical distribution for the small subsystem. That's a standard thing you read in the textbooks on how you derive the canonical ensemble from a microcanonical ensemble classically. In quantum mechanics you start with a micro-canonical density matrix for the large system then look at the subsystem, and trace out, instead of integrating out, the remaining degrees of freedom. More symbolically, if a large system A and a system B have the joint microcanonical density matrix you can trace out over A, and get a density matrix for B which, again, if B is small and the interaction is weak, is just the canonical density matrix. Thus to get the canonical distribution for the subsystem, you have to trace over the density matrix for the large combined system. What canonical typicality says is that in fact if you randomly pick a wave function for the joint system A and B, and then you look at the pure density matrix, which corresponds to a particular wave function for the joint system A and B, and you trace out over the large part A,

you will get the canonical density matrix for the subsystem, namely, this is true of almost all individual wave functions.

I don't know if this explains it or not—namely, that something is typical for individual wave functions instead of just true in some average sense. It's not true for every wave function, but it's true for a large majority of wave function with respect to some kind of a probability distribution you have over the wave functions.

ZIERLER: I'm curious what value you have seen in taking a macroscopic quantum systems approach to thermal equilibrium.

LEBOWITZ: I'm not sure I understand the question.

ZIERLER: Well, when you're coming to understand thermal equilibrium, why look at it from the perspective of macroscopic quantum systems?

LEBOWITZ: Well, that's what the world is made out of. The macroscopic [laugh] world is made—

ZIERLER: [laugh]

LEBOWITZ: —of macroscopic [laugh] quantum system. It's not [laugh] a choice but that's just the way it works.

ZIERLER: And what have been some of your findings in this research?

LEBOWITZ: Now, what are the findings? As I say, we found canonical typicality. In more recent work, we noted that you can characterize a quantum system as being in equilibrium by looking at the macroscopic description of a large system. A system is in equilibrium if particle

density, energy density is more or less uniform. This is so for both classical and quantum mechanical systems with some extra problems for quantum systems because different quantities do not commute with each other. So, you have to take that into account.

But you can also characterize a quantum system by looking at a small subsystem of it, and asking that the subsystem have a density matrix which is close to what it would have if the large system was described by a canonical density matrix. While classically if the large system is microscopically specified by a phase point, giving all the positions and velocities, then if you look at any subsystem, it will also have its positions and velocities specified. Quantum mechanically, you can have your large system described by a wave function, while its small subsystems will be described by a density matrix. You can then ask how close is the density matrix for the small subsystem to what you would have gotten from a canonical or microcanonical density matrix for the whole large system. We distinguish MATE from MITE—MATE is macroscopic thermal equilibrium. MITE is microscopic thermal equilibrium. What we mean by that is that small enough subsystems behave as if their density matrix comes from a microcanonical distribution of the large system, even though the large system is described by a pure wave function. This is sort of canonical typicality.

ZIERLER: I'd like to ask, since we've brought it right up to the present in your research, I'd like to ask a few broadly retrospective questions for the last part of our talk. And one is I know you have long been interested in the connections between science and morality. What have you learned about these connections and the ways that science might influence our understanding of morality, and how morality might influence how we should do science?

LEBOWITZ: Well, what should I say? Mostly what I've learned is that scientists are not necessarily moral. [laugh] That's one of the painful lessons.

ZIERLER: Or at least not any more moral than your average person?

LEBOWITZ: Exactly. Well, yeah, I don't think they're any worse, but they're not any better.

[laugh]

ZIERLER: [laugh] They're just people.

LEBOWITZ: [laugh] They're just people, exactly. Now, what I've learned is that science can and should give a perspective on the world, including on people. It should be a moral perspective in the sense that it makes no sense scientifically to have racism or things like that. That simply makes absolutely no sense. Science should enlighten things in that respect. It should show us that what people have in common in terms of the possibility of appreciating the universe is so much more important than what they have in differences. This should lead to a defense of human rights and acceptance of diversity.

This was certainly an important theme of Sakharov and also of Einstein. And I guess I was particularly impressed when reading about Max von Laue who was one of the very few German scientists—non-Jewish German scientists—who stood up clearly in his opposition to Nazism, which I learned from a book about scientists under Nazism. So, I do think that science should teach us to be moral and care about human rights. That's about all.

ZIERLER: Absent its supernatural aspects, do you see your early heritage in Torah Judaism as particularly informing your views on morality in science?

LEBOWITZ: No, I don't think so. But, obviously, one grows up in a certain environment. I don't like at all those fanatical observant Jews, their attitude towards racism and others, in Israel or in the United States. I'm not very comfortable with that at all, including some members of my own family. [laugh]

I think one can pick and choose. There are things in the Bible and in the Jewish tradition which are very humane and very good. But there are also parts which are not so good. How come you are so familiar with Judaic things?

ZIERLER: I'm a fellow Jew. What can I say?

LEBOWITZ: Did you grow up religious or are you religious?

ZIERLER: A little bit of yes and a little bit of no in both ways.

LEBOWITZ: I see.

ZIERLER: Joel, in all of the ways that you've been honored, is there any recognition or prize that means most to you?

LEBOWITZ: Well, a good question. [laugh] I've enjoyed all of them. [laugh] Maybe getting the Boltzmann Medal [laugh] was one of the things that I enjoyed most since Boltzmann is one of my scientific heroes.

ZIERLER: Yes.

LEBOWITZ: Anyway, I—

ZIERLER: I think in all of your research, since you've worked on so many things, is there one idea or concept or even problem that connects everything that you've been interested in?

LEBOWITZ: I think it's probably almost all in the relation between microscopic and macroscopic behavior. How does observed macroscopic behavior come about from the microscopic dynamics. This question goes from phase transitions in equilibrium systems to living beings, particularly to living beings, which is certainly something very, very much an open problem I'd like to understand.

ZIERLER: Joel, last question, looking to the future, of all of the things you've worked on, those that seem unsolvable to you or perhaps are fundamentally mysterious, what are you most curious about and also most optimistic about them being understood either in the near-term or the long-term?

LEBOWITZ: Well, the most mysterious seems to be life, living things.

ZIERLER: [laugh]

LEBOWITZ: A problem that might be solvable by somebody bright—is the phase transition in a hard sphere system. When you increase the density of a system of hard balls it seems to undergo a transition from a disordered fluid state to a crystalline state. And this is pure geometry. There's no temperature involved or anything. It's just density and pure geometry. It's something I would like to understand, and I wouldn't be totally surprised if I look at the next issue of *Physical Review* or something like that or some mathematical paper that somebody has actually been able to prove that. I think that would be most likely of the unsolved problems. [laugh]

ZIERLER: [laugh] Joel, it has been a great pleasure spending this time with you. I am so happy we were able to do this. Thank you so much.

LEBOWITZ: Thank you so much also.

[End of recording]