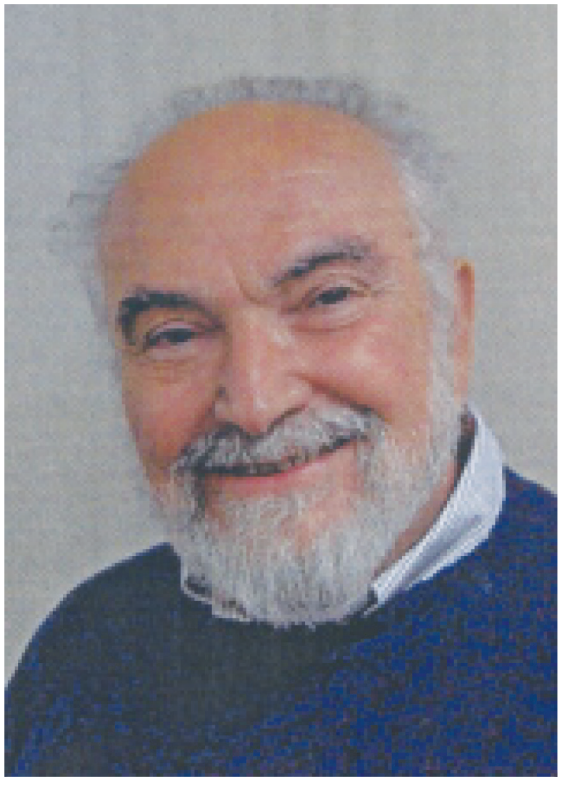

Joel L. Lebowitz

ANNUAL REVIEWS

Annual Review of Condensed Matter Physics

Human Rights and Science: Biographical Notes

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Abstract

I describe some of my activities, academic and personal, since coming to the United States in 1946 at the age of 16. It has been a long journey with many ups and downs. I selectively and briefly describe my experiences in a rabbinical school with an attached (parochial) high school, at Brooklyn College, in graduate school at Syracuse University, during a postdoc with Lars Onsager at Yale University, and in my academic positions at Stevens Institute of Technology, Yeshiva University and Rutgers University. I write at greater length about some experiences traveling to the Soviet Union (now Russia) during the period of 1978–1990, where I went to meet with refusenik and dissident scientists. There, I met Andrei Sakharov, whose fight for human rights has been an inspiration to me. I conclude with a talk I recently gave (via film) at the March 2024 meeting of the American Physical Society.

ARRIVAL TO THE UNITED STATES: 1946

The dormitory style boat SS Perch, built during the Second World War to transport troops, landed in New York at a dock on the Hudson River in mid-Manhattan, around noon on Saturday, August 31, 1946. It was full of new immigrants, mostly Jews from Eastern Europe, who survived the Nazi efforts to exterminate all Jews in Europe. They survived by hiding or by managing to stay alive in concentration camps until they were liberated by allied (American, British, and Soviet) troops in the spring of 1945. These immigrants included a group of a dozen or so "children" ages 16–18, who came to the United States under the sponsorship of an organization (I think it was the Children's Aid Society) headed by Eleanor Roosevelt, devoted to the rehabilitation of such holocaust orphans. I had turned 15 in May of 1945, just a few weeks after being liberated from the Bergen-Belsen concentration camp on April 15, 1945, by British troops. I had spent the intervening 16 months wandering through postwar Europe (a long story) and was now in that orphan children's group.

There were representatives of the sponsoring organization at the dock to take us to a temporary shelter in the Bronx—a school building turned into a dormitory. Because I was orthodox Jewish at that time, I would not travel by car on Saturday. I therefore stayed that afternoon, together with an orthodox family I met on the boat, in a house near the docking pier. This had been arranged by this family's father, who had immigrated to the United States before the war. I was picked up that evening by a social worker and taken by taxi to the dormitory in the Bronx. We passed through Times Square, which was in a celebratory Labor Day weekend mode. This impressed me greatly, coming from drab postwar Europe. After staying in that dormitory for about two weeks, I enrolled as a student at a Jewish rabbinical school called Yeshiva Torah Vodaath (School for Torah and Knowledge) in Williamsburg, Brooklyn.

The school had a double program. The mornings were devoted to Talmudic studies, whereas the afternoons were given over to high school. It provided full boarding, including summer camp, for the four or five students like me. During the fall of 1946, we were tutored in mathematics and English. That enabled me to read my first book in English: Charles Dickens' *A Tale of Two Cities*. With this tutoring, I started high school in the spring of 1947. The education in the high school was good in math and English literature but was not so great in the sciences. I graduated high school in June of 1948, passed the "Regents Exam," required at that time in New York state to get a high school diploma, with a high score, and was awarded a prize in Hebrew.

I mostly enjoyed my time at the rabbinical school—it was in some ways a continuation of my life before being taken to the concentration camp in May 1944. I particularly liked the school's summer camp in the Catskill Mountains, which was near a secular hotel where one could meet girls. Despite these amenities, I gradually lost my religious beliefs and became dubious about the relevance of the religious studies. I therefore decided to quit the rabbinical school after graduation from high school.

To earn some money for food and shelter, I got a job putting wicks into cigarette lighters. I was not very good at this and was fired after two weeks, so I was easily convinced by the head of the rabbinical school to return there. He hoped to convert me back to religion. I stayed in the Talmudic program for another year while taking evening classes at Brooklyn College (BC). There was no tuition at BC, and admission was automatic if your high school grades were above a certain minimum.

After leaving the rabbinical school in the summer of 1949, I got a part-time job as a waiter in a deli-type restaurant in the Greenpoint section of Brooklyn; I had already done some stints as a busboy at hotels in the Catskill Mountains. This lasted for a few months and helped me make the transition to independent living. In September of that year, I became a full-time student at BC, intending to become an engineer. To support myself, I worked five evenings a week, Sunday through Thursday, as a waiter in a small kosher restaurant on the East Side of Manhattan: the Tel Aviv Restaurant on Rivington Street. I got this job through a cousin of mine who was a frequent

customer in that restaurant. The restaurant was owned by two immigrants from the same area (the Carpathian Mountains) where I was raised. This was part of Czechoslovakia before 1938, then part of Hungary, and is now part of western Ukraine. The owners also acted as cooks and as waiters but felt that it was undignified for them to accept tips, so there was a sign "NO TIPS" on the posting board. I gradually managed to cover up the "NO" part of the sign and enjoyed getting tips.

BC had a very good faculty at that time, in both the sciences and the arts. Many of the professors were recruited during the depression years in the 1930s when jobs were scarce. I was particularly influenced by one of my physics teachers, Melba Phillips. Melba got her PhD with J. Robert Oppenheimer, then a professor at the University of California at Berkley, in 1933. She continued to work with Oppenheimer and in 1935 they published a joint paper that described the Oppenheimer–Phillips effect in nuclear physics. There were very few women PhDs in physics at the time. (I recently read on the Internet that in Melba's family, a surviving nephew was disappointed that she was not mentioned in the Oppenheimer movie.)

Phillips converted me from engineering to theoretical physics by opening my eyes to its beauty. She was an outstanding teacher, earning many awards in that field. She was fired from BC in 1952, just after I graduated, for refusing to cooperate with a committee of the US Senate, which was on a witch hunt for communists or leftists during the red scare of the early fifties, the notorious McCarthy era. Melba was unemployed for several years but then held many university positions focusing on science education. We became good friends after her retirement, when she lived in New York.

I had many other excellent teachers at BC. I learned in my mathematics courses what it meant to "prove" something. That has stayed with me and helped me become a mathematical physicist.

GRADUATE SCHOOL: 1952–1956

Before I graduated from BC, in June 1952, I had applied to and been accepted as a graduate student in the physics department of Syracuse University, with a research assistantship of $1,500 for the academic year 1952–1953. I was to work with Professor Peter Bergmann, whose book on statistical mechanics I had studied at BC with Melba Phillips.

Melba was a friend of Peter Bergmann, and they were also later coauthors of a book. Bergmann lived in New York, where he taught a course at Brooklyn Polytech [now part of New York University (NYU)]. Brooklyn Polytech was also the place where Peter's wife, Dr. Margot Bergmann, worked as a researcher in crystallography. They lived on Riverside Drive (near 142nd Street), in a spacious apartment overlooking the Hudson River, with their two young sons.

I visited Peter in that apartment during the spring of 1952, when I was trying to decide on a graduate school, and was very much impressed with his informality and generous personality. It was this that made me decide to go to Syracuse, rejecting offers of assistantships from Columbia and Yale Universities, as well as many other schools.

Before making that decision about graduate school, I'd had an interview with Professor Polykarp Kusch, then chair of the physics department of Columbia University (and a 1955 Nobel Prize winner). He tried to convince me to go to Columbia, pointing out their distinguished faculty, including Isidor Rabi and Hideki Yukawa, both Nobel Prize winners. I remember (vaguely) Kusch telling me that I would be able to take classes with Yukawa, even though nobody understood him. I also visited the physics department at Yale University, where I talked with graduate students working on high-energy physics who were not very happy. So, I went to Syracuse.

I spent the summer of 1952 learning to drive, having bought a used car for $50. I finally passed the driving test (after two failures) and was getting ready for my solo drive to Syracuse. Meanwhile, I was still working part-time in the Tel Aviv Restaurant, going to the beach in Coney Island, and reading Khinchin's book on statistical mechanics, as well as novels by Dostoyevsky. I was also going

out on dates with my future wife Estelle Mandelbaum. Estelle lived in the Bronx, more than an hour's subway ride from where I was living in Brooklyn, and this gave me lots of time to read on the subway. There were often only a few passengers in the subway car going back from the Bronx to Brooklyn after midnight.

Finally, the day arrived for my departure to Syracuse. This came with some trepidation, because I was still a novice driver and had never driven outside New York City. But all went smoothly, and I arrived at Syracuse a few days before the fall semester began. I rented a furnished room near the university and registered for courses in physics and mathematics. The teachers I remember best were Peter Bergmann, Mel Lax, and Gene Gross in physics and Kai-Lai Chung in math.

Peter Bergmann's main area of research was general relativity. He had been a postdoc for Albert Einstein in the late 1930s at the Institute of Advanced Study in Princeton; both Einstein and Bergmann were refugees from Hitler's Germany, so they would talk in German. I remember a dinner in New York in the 1980s. I invited the Bergmanns and Melba Phillips to a restaurant near Melba's apartment in a nice public housing unit in downtown New York. During dinner, Margot Bergmann told, in a hushed voice, the story of how one Sunday morning in Princeton, when Peter was a postdoc of Einstein, she woke up late and, walking to the kitchen, she saw Einstein and Peter at the sink washing dishes. The reason for the hushed voice was that she thought it somewhat undignified to tell of Einstein washing dishes.

Most of Peter's students were working on general relativity, but I and one or two other students were working on nonequilibrium statistical mechanics, a secondary interest of Peter's at that time. We all shared desks in a small prefab building next to the physics department. I became friends with Ted Newman (of the Kerr–Newman effect) and also with Joan Hirshberg, who was working with Mel Lax. Joan was a sister of Richard Feynman and I heard a lot about him, maybe even met him, at her house. Joan later switched to astrophysics, where she aided in the discovery of what causes the aurora borealis.

My first scientific paper was published in *Physical Review* in 1954. It was my Master's Thesis done under the direction of Mel Lax, with whom the paper was coauthored (1).[1] It described an approximate scheme for obtaining the frequency distribution spectrum of a harmonic crystal by combining information about its readily obtainable moments and its singularities: the latter a recent discovery by Leon Van Hove. Mel Lax, who later moved to Bell Labs and The City University of New York, was a student of Herman Feshbach. He had the idea of teaching classical mechanics as a limit of quantum mechanics when $h \to 0$, which was not a good pedagogical idea, in my opinion.

While a graduate student, I also wrote a paper with Gene Gross (of Gross–Pitaevskii fame). Gross was a student of David Bohm who, like Melba Phillips, was fired by Princeton University for not cooperating with the witch hunting US Senate committees.

My PhD Thesis was published in two papers jointly with Peter Bergmann. It describes the time evolution and stationary states of an open many-body classical system interacting with one or more temperature reservoirs. The reservoirs are assumed to be infinite, and their interaction with the system is via instantaneous collisions in which the incoming reservoir particles always have the same distribution. The time evolution of the probability density of the system in its phase space then satisfies a master equation. The stationary state of the system is given by a Gibbs-type ensemble in the phase space of the system: It is an equilibrium, canonical or grand canonical, Gibbs ensemble when the temperatures of the reservoirs are the same and a nonequilibrium one when they are different. To prove the approach to a stationary state in the latter case, I used a deep

[1]All my scientific papers I mention can be found on my webpage, available at **https://cmsr.rutgers.edu/research-cmsr/publications-cmsr**.

theorem about Markov processes by Doeblin, which I learned from a (then) recently published book on probability by J. Doob (2). This was rather mathematical and helped guide the main lines of my future work. Determining the nature of such nonequilibrium ensembles has been a constant interest of mine.

My time in graduate school lasted until June 1956. It was scientifically very rewarding; Peter Bergmann was a wonderfully supportive advisor. He was able to support me from his grants, so I never did any teaching (a fact that had both positive and negative aspects). It was, however, personally quite difficult. This was due to the illness of my wife Estelle, whom I married in June 1953. She had to be hospitalized in the summer of 1954. This made me commute between Syracuse and New York during the academic year 1954–1955, often riding together with Peter Bergmann and once almost getting both of us killed when my car slid on an icy highway. I also remember one ride back to New York with Mel Lax, who was driving and at the same time writing equations with his fingers on the foggy windshield. It scared the hell out of me. I moved to New York City, to an apartment overlooking the junction of the Hudson and Harlem Rivers, in the summer of 1955.

I finished my thesis in the spring of 1956 when, to my great joy, I was awarded a National Science Foundation (NSF) postdoctoral fellowship to work with Lars Onsager at Yale. Onsager was already the most famous statistical mechanician at that time, although he did not get his Nobel Prize until 1968. He got the Nobel Prize in Chemistry for his derivation of the Onsager relations: a symmetry relation between transport coefficients. He should have also gotten the prize (according to many) for his exact solutions of the 2D Ising model, showing for the first time the existence of a phase transition, with a critical temperature at which the specific heat diverges, for an interacting system using the statistical mechanics of Gibbs.

MEMORIES FROM MY GRADUATE DAYS

I recount some memorable (to me) events from graduate school. In the spring of 1953, I, together with some other graduate students of Peter Bergmann, drove to Pittsburgh, in my $50 car, to attend a conference on nonequilibrium statistical mechanics. We drove all night and got to the conference, rather exhausted, in the morning, just as the meeting opened. I was sitting in the third or fourth row listening to a talk that I did not understand. After the talk there were some questions to the speaker and at some point, the chair said, "Perhaps Professor Onsager could answer this," while looking directly at the bald gentleman in front of me. I had heard something about "Onsager relations" and somehow thought they were about relatives of a dead scientist, so I was quite shocked to see a flesh and blood person responding.

I also remember listening to a physics colloquium by Mark Kac, then a professor of mathematics at Cornell University, where he described his recent work with Ted Berlin on the mean-spherical Ising model. He referred to Onsager's exact solution of the 2D Ising model as, "then Onsager threw all the heavy machinery at it and the problem collapsed."

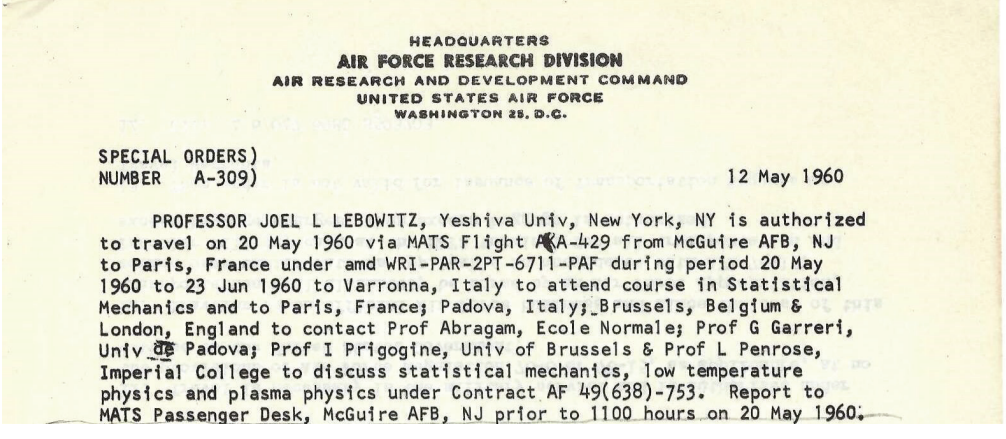
Another memory of those days was sitting in a car with Peter Bergmann and J. Doob, whose book (2) I already mentioned, while someone was driving us from Syracuse, where Doob just gave a math seminar, to Cornell, where he was going to give another. After the seminar at Cornell, we were invited by Freeman Dyson, a professor at Cornell at that time, for drinks. After the drinks, we went to dinner at an Italian restaurant in Ithaca. Freeman treated me as an invited guest and paid for my dinner, for which I have always been grateful to him. I later got to know Freeman quite well and we even published several joint papers.

I also remember Henry Linschitz, a professor of chemistry at Syracuse at that time, who later moved to Brandeis University. He used to say, "Thermodynamics is easy. I have already learned it six times." This resonated then and still does with my feelings about thermodynamics, especially in the way it is being applied to black hole physics and cosmology these days.

Peter Bergmann did not attend the Pittsburg conference I mentioned before, but a year or so later he organized a nonequilibrium conference in Syracuse. There I actually met Onsager as well as Ilya Prigogine and Martin Klein (maybe also Herb Callen). It was my meeting Prigogine at that conference that led to him inviting me to a conference in Brussels he was organizing in August of 1956. I was working that summer at the IBM Watson Lab on West 115th Street, next to Columbia University, where I had been hanging out a lot during the academic year of 1955–1956 when I was still a graduate student at Syracuse but lived in New York. I had met Peter Linhart during that period. Peter was a physics graduate student at Columbia and had a part-time job at the Watson Lab. This led to my getting a summer internship at the lab, in a group led by Peter Price, while waiting to start my NSF postdoctoral fellowship at Yale with Onsager. I worked at the lab on electron transport in semiconductors but do not remember obtaining any publishable results.

At first, I did not expect to be able to go to the Prigogine conference: Travel by air was expensive. Then, one afternoon at the Watson Lab, I received a phone call from someone working at the Air Force Office of Scientific Research (AFOSR). On the call, I was told that I would be able to fly to Europe via military air transport service (MATS) to participate in the conference organized by Prigogine. The reason for that offer was that the conference was cosponsored by the AFOSR, hence the MATS. In due time I, together with a few other physicists going to that conference, flew with MATS from McGuire Air Force Base in New Jersey to Paris, via a stop at Shannon Airport in Ireland. This was my first flight, and I remember us sitting for safety with our backs to the direction in which the place was flying. It was my first trip back to Europe since arriving in the United States in 1946. I do not have my "travel orders" from that time, but I did find one from an AFOSR sponsored trip in 1960,[2] which I reproduce here (see **Figure 1**).

I stayed in Paris for two nights at a hotel across the Seine River from the Eiffel Tower (Hotel Leopold Premier). From Paris, I traveled by an express train, which took four hours (now it takes about one hour), to Brussels. That conference is now counted as the second international conference on statistical mechanics sponsored by the International Union of Pure and Applied Physics. I was perhaps the youngest attendee but nevertheless participated actively in the discussions. The participants I remember include Robert Brout, Markus Fierz (who was a collaborator of Wolfgang Pauli), Léon Van Hove, Bernie Alder, and Giorgio Careri.



HEADQUARTERS
AIR FORCE RESEARCH DIVISION
AIR RESEARCH AND DEVELOPMENT COMMAND
UNITED STATES AIR FORCE
WASHINGTON 25, D.C.

SPECIAL ORDERS)
NUMBER A-309)

12 May 1960

PROFESSOR JOEL L LEBOWITZ, Yeshiva Univ, New York, NY is authorized to travel on 20 May 1960 via MATS Flight AKA-429 from McGuire AFB, NJ to Paris, France under amd WRI-PAR-2PT-6711-PAF during period 20 May 1960 to 23 Jun 1960 to Varronna, Italy to attend course in Statistical Mechanics and to Paris, France; Padova, Italy; Brussels, Belgium & London, England to contact Prof Abragam, Ecole Normale; Prof G Garreri, Univ de Padova; Prof I Prigogine, Univ of Brussels & Prof L Penrose, Imperial College to discuss statistical mechanics, low temperature physics and plasma physics under Contract AF 49(638)-753. Report to MATS Passenger Desk, McGuire AFB, NJ prior to 1100 hours on 20 May 1960.

Figure 1

Travel orders from Air Force Office of Scientific Research, May 12, 1960.

[2]My research was supported by grants from the AFOSR during the period of 1958–2018.

Bernie Alder talked about the new molecular dynamics simulations of a system of hard spheres, which agreed with the solutions of the Boltzmann equation. Van Hove talked about obtaining an irreversible master equation for many-body quantum systems in the (Van Hove) limit in which the coupling constant describing interactions, g, goes to zero while the time t goes to infinity, so that $g^2t = s$ remains finite. A rigorous quantum master equation for the density matrix was later derived, for a system coupled to a heat bath, by Brian Davies, in the Van Hove limit: See my article with H. Spohn (3) who was my postdoc at the time.

POSTDOC WITH ONSAGER

Following the conference, I started my postdoc with Onsager at Yale. For family reasons, I continued living in New York and spending much time hanging out with graduate students at Columbia University. These included Leon Cooper of the Bardeen–Cooper–Schrieffer theory, Gerald Feinberg (tachyons), and, as already mentioned, Peter Linhart. During that time, I also attended a course by T.D. Lee on statistical mechanics and another by Meyer Shapiro, a world-famous expert on medieval art. I enjoyed both of these courses immensely. I had already heard as a graduate student about the connection between the Lee–Yang zeros of the grand canonical partition function and phase transitions, and they still fascinate me greatly. From Meyer Shapiro's class I remember his explanation of the saints around Christ in the windows of Chartres Cathedral, of which he showed slides. I still remembered some of it when I got to see Chartres with my own eyes, five or six years later.

Peter Linhart and I, together with some of the other physics graduate students, would often walk a few blocks north on Broadway from Columbia to 125th Street for lunch. There, we had a choice: We could eat at a Chinese restaurant then favored by T.D. Lee or we could go to Frank's, a restaurant on 125th Street east of Broadway in Harlem with white waiters and a mostly black clientele, which was something quite unusual in those days.

During the academic year of 1956–1957, I would drive to Yale once a week and spend the whole day talking to Onsager and to the graduate students working with Henri Fairbanks and Cecil Lane in the low-temperature physics group. This was the area Onsager was interested in at that time, but it was something entirely new to me. There was much interaction between Onsager and Lane, who was doing experiments on "second sound" and rotating superfluid helium. Onsager and I published a joint paper (3a) on the angular momentum of a rotating ideal Bose gas—it was not of great significance. I did, however, greatly enjoy occasional lunches with cocktails at Onsager's home. Onsager was very proud of his martinis. He was also proud of his botanical knowledge; he had a farm in New Hampshire where he would spend summers.

I should mention here that when applying for an NSF postdoctoral fellowship, in the fall of 1955, I drove to Yale to get Onsager's consent to be my mentor. While I was there, Onsager took me to meet Oliver and Joan Penrose. Oliver had just written an important joint paper with Onsager about the superfluidity of interacting bosons (4). He was now a postdoc of Onsager living in a semibasement apartment in New Haven. This meeting led to many joint publications and a lifelong friendship with Oliver, still enduring now.

I remember driving up from New York, together with Oliver, to see Onsager in New Haven many years later. During lunch, with martinis, I told Onsager about my work with Elliott Lieb proving the existence of the thermodynamic limit for Coulomb systems. An important ingredient in our proof was the possibility of packing a cube with spheres of different sizes leaving an arbitrarily small empty space. Onsager's response was, "This is what they taught us in engineering school, 'graded sand makes the best concrete.'" It was during the same visit that I asked Onsager what he thought of the attempts to study the brain via electrocardiogram. His response was, "That

is like studying how the telephone system works by monitoring the fluctuations in the power consumption of the telephone lines." That was before cell phones and AI.

On another occasion, I ran into Onsager at a conference and asked him whether he thought that metastability, a problem I was working on with Penrose, was an equilibrium phenomenon or not. He responded, after some thought: "There was a factory in Canada where they manufactured glycerin. One winter, the pipes froze—pause—they moved the factory." The implication of the story was, once the pipes froze, they kept on freezing every winter—because of the frozen seeds left there—so metastability is not an equilibrium phenomena.

FACULTY POSITIONS

In 1957 I got my first academic position at Stevens Institute of Technology in Hoboken, New Jersey. This was close enough to my apartment in New York that I could drive there, going through the Lincoln Tunnel under the Hudson River, in about thirty minutes. It was there that I started the semiannual Statistical Mechanics Conferences in May 1959. Their history is described in Reference 5.[3] The 126th meeting is scheduled for May 2024. (It would have been the 131st if not for COVID.)

It was also at Stevens that I started working with Harry Frisch, who was a postdoc at Syracuse while I was a student there, and Howard Reiss, on the scaled particle theory of hard spheres. Approximating the work needed to create a spherical cavity in a fluid of hard spheres, the theory yielded a highly accurate equation of state for hard spheres for densities below the fluid–crystal transition. This equation turned out to be identical to what one gets from the solution of the Percus–Yevick equation for the radial distribution function of hard spheres, using the compressibility relation. I still do not know the reason why.

Another collaboration and friendship that started while I was at Stevens was with Jerry Percus, who had left Stevens for NYU just before I got there. Jerry lived in New York, not too far from where I lived, so we used to meet in his house and also in coffee shops and parks. We were young and life was mostly good.

In the fall of 1959, I moved from Stevens to the newly created Graduate School of Science at Yeshiva University (YU) in New York. This later became the Belfer Graduate School of Science of YU. The Dean of that newly created division of YU was Abe Gelbart, a mathematician whom I knew as a physics graduate student at Syracuse University. Abe was a complex person, able to attract distinguished scientists like Paul Dirac and Freeman Dyson and others to visit for extended periods. To get an idea of the atmosphere of the school in its early days, I quote from *The Cosmic Landscape* by Lenny Susskind (6, p. 65); Lenny describes his first day as a new faculty member at YU:

> The entire school was housed in this one-time ballroom. It would have been very depressing except that several people were having a lively physics conversation at one of the boards. What's more, I recognized some of them. I saw Dave Finklestein, who arranged my new job. Dave was a charismatic and brilliant theoretical physicist who had just written a paper on the use of topology in quantum field theory that was to become a classic of theoretical physics. I also saw P.A.M. Dirac, arguably the greatest theoretical physicist of the twentieth century after Einstein. Dave introduced me to Yakir Aharonov, whose discovery of the Aharonov-Bohm effect had made him famous. He was talking to Roger Penrose, who is now Sir Roger. Roger and Dave were two of the most important pioneers in the theory of black holes. I saw an open door with a sign that said Joel Lebowitz. Joel, a very well-known mathematical physicist, was arguing with Elliott Lieb, whose name was also familiar. It was the most brilliant collection of physicists that I had ever seen assembled in one place.

[3]Much information, including programs of old and recent meetings, can be found on my website, available at **https://cmsr.rutgers.edu/**.

Abe was also rather prickly and got into fights with some of the mathematicians on the faculty whom he had recruited. This led to his resignation as Dean in 1974. Serious financial problems then led to the dismantling of Belfer, as all those on the faculty who could find other positions left.

Just before that happened, I got a Guggenheim Fellowship and left for a year's sabbatical in 1976–1977, at l'Institut des Hautes Études Scientifiques in Bures-sur-Yvette, France. Bures is a suburb of Paris, and I lived in an apartment in Paris near the Luxembourg Garden, from where I could take the metro to Bures. This was my first sabbatical, and I enjoyed it greatly, both scientifically and personally. While there, I accepted a position at Rutgers University beginning in September 1977, where I still am. While at Bures, where David Ruelle was the residing mathematical physicist, I also had good contacts with scientists in neighboring institutions. Édouard Brézin and Cyrano De Dominicis at Saclay, Bernard Jancovici and Loup Verlet at Orsay, etc.

My colleagues at Yeshiva who had left before me included Yakir Aharonov, Peter Bergmann, Elliott Lieb, Dan Mattis, and as already mentioned, Lenny Susskind. Long-term visitors included, besides those seen by Susskind, Oliver Penrose, Loup Verlet, Cyril Domb, and Freeman Dyson. My PhD students included Michael Aizenman, Shelly Goldstein, and Eduardo Weissman. Highlights of my scientific work at Yeshiva include the solution of the Percus–Yevick equation for a mixture of hard spheres, the elucidation of time asymmetry in the quantum measurement process (jointly with Aharonov and Bergmann), proof of the correct mean-field limit for the vapor–liquid transition (with Oliver Penrose), the existence of thermodynamics for Coulomb systems (with Elliott Lieb), various inequalities for spin systems, etc. My most cited paper from that period is none of the above; it is about efficient Monte Carlo simulations (6a). This was a joint work with A. Bortz and M. Kalos and arose from simulations on phase segregation in quenched binary alloys. My contributions were entirely theoretical. I never actually did any programming of a computer.

The semiannual statistical mechanics conferences I started at Stevens continued at YU. There were altogether 36 statistical mechanics conferences at Yeshiva, so when moving to Rutgers, I had to organize the 38th conference there in December 1977. My appointment at Rutgers was primarily in the mathematics department, with also a position in physics. It was the former that provided the secretarial and other support for that conference as it continues to do now, with some help from physics. I encountered some resistance from Michael Fisher and others about moving the meetings from New York to Piscataway, New Jersey, but it all worked out.

NEW YORK ACADEMY OF SCIENCES, COMMITTEE OF CONCERNED SCIENTISTS, AND HUMAN RIGHTS ACTIVITIES

Moving to Rutgers in September 1977 also coincided with my being elected by the New York Academy of Sciences (NYAS) as President-Elect for the year 1978 and President for the year 1979. The NYAS, founded in 1817, was located then on East 63rd Street between 5th and Madison Avenues in an old mansion. It was a kind of scientific club with most of its activities consisting of organizing conferences and publishing the proceedings. These were mostly in the biological sciences but also with a few in the physical sciences. This was quite profitable financially, and the NYAS also had income from membership dues. These members were also mostly in the biological sciences or medical doctors. The members' rewards consisted of one (or two) free conference proceedings and a plaque to hang on their office walls. At some point the NYAS also published a monthly magazine with *Scientific American*–type articles for members. I was drawn into the activities of the NYAS by Lloyd Motz, an astrophysicist at Columbia University. I became the chair of the physics section around 1974 and started organizing monthly lectures on Friday evenings. These lectures were preceded by nice dinners with cocktails and wine at the beautiful NYAS building. They were free to the dozen or more guests invited by me or the speaker that evening. The attendance at the lectures themselves was much larger, depending on the speaker.

I remember having Freeman Dyson, Julian Schwinger, Ilya Prigogine, and other famous scientists as speakers. The NYAS was doing well financially at that time and could afford the expenses incurred by these lecturers. I remember my companion (later wife) Ann listening to Dyson talk about robots mining the planets and thinking (because of his British accent) that he was talking about row boats. I also remember a gaffe I made after a lecture by Julian Schwinger, who was a Nobel Prize winner, about the newly "discovered" quarks. Schwinger was a great speaker and very convincing. After his talk I wanted to be amusing and recounted a story I heard from Shneior Lifson, a well-known physical chemist, at a dinner in Michael Fisher's house. It went something like this: There was a very large department store in Amsterdam whose salespeople wanted to organize themselves into a Catholic labor union. One of the salesmen, however, was Jewish. So, they asked the owner to fire him. The owner said he could not do it since he was his best salesman, but if they could persuade him to retire that would be OK. So, they invited the local bishop to talk to this guy. The bishop came and had a private talk with him behind closed doors. When the bishop came out from the meeting walking very fast, his attendants who had been waiting for him, ran after him asking, "Did you succeed?" "No," said the bishop, "but he sold me a double bed." I don't think Schwinger was very pleased with this comment on his talk, but I think it is OK to repeat it now half a century later.

My association with the NYAS also inspired my interest, at that time, in the human rights of scientists, an activity that I saw as something the NYAS could and should promote. I joined the Committee of Concerned Scientists (CCS), a group formed in 1972 to help Jewish scientists in the Soviet Union who applied to emigrate to Israel. These scientists were typically fired from their jobs and at the same time denied permission, with few exceptions, to leave the Soviet Union. The reason given for refusal of exit visas was that they had some valuable (military) secret knowledge, which in fact was not true. They came to be called refusenik scientists, and they were in urgent need of help, both material and intellectual. They solved the former by taking on jobs like tutoring students and having spouses who worked. To keep up some way in their science, they organized seminars in their apartments. Their "Sunday Seminars," held in private homes, became well known in the West.

In 1974 and in 1977, CCS organized an international refusenik conference. They invited scientists from the United States and other Western countries to go to Moscow as tourists and meet with the refuseniks and give talks at their seminars. I was not aware of these meetings until 1978, but when such a meeting was planned for December of that year, I signed up via CCS to participate.

I quote here from my foreword to a collection of articles presented at the "fourth" such conference, in April 1980, which I edited. It was published by the NYAS in 1981, where my foreword appeared (7, p. vii):

> The Fourth International Conference on Collective Phenomena took place, like the preceding ones, not in a well-appointed academic lecture hall but in a cramped living room in a Moscow apartment under the discomforting surveillance of the KGB (the notorious Soviet secret police). Yet the room, indeed, the whole apartment, was full to overflowing with both local and foreign scientists, and the papers presented, as can be seen in this volume, were of high quality and interest.
>
> These conferences are an extension of the Moscow Sunday Seminars begun in 1972 by a group of refusnik scientists so that they could keep up their professional work despite their exclusion from all official scientific activities. The Seminar soon gained a reputation for its high standards and for the devotion of its participants to science. It has taken place in the Brailovskys' living room since 1977 and has been visited by scores of Western scientists. [These included Arno Penzias who went there directly from Stockholm, where he got his Nobel Prize.]
>
> The Seminar's first attempt to organize an international conference in 1974 was foiled by the Soviet authorities—the organizers of the conference were arrested and held for 15 days. Meetings did take place in 1977, 1978, and 1980, though under the surveillance of the authorities, with some intimidation of the local participants.

Figure 2

Joel at the portable blackboard with Andrei Sakharov at the refusenik seminar in December 1978. Photo provided by Jim Langer.

It was the "third" conference, counting the one that did not take place in 1974 as the first one, for which I departed around December 20, 1978, as a member of a tourist group to visit Leningrad (now St. Petersburg) and Moscow. Another passenger with the group was Jim Langer, who was also going to the refusenik conference. An important event at the conference was my meeting with Andrei Sakharov (see **Figure 2**), the "father" of the Soviet hydrogen bomb and the recipient of the Nobel Peace Prize for advocacy of nuclear disarmament and human rights. I quote loosely from an article I wrote in 1991 (8, p. 436):

> I first met Andrei Sakharov Wednesday morning December 27, 1978 in the very crowded apartment of Victor and Irina Brailovsky, on Vernadskova Prospect, in Moscow. I was there together with my friend, Jim Langer. . .
>
> As Jim Langer recalls it in an article in the June 1979 issue of *Physics Today* "Sakharov. . .introduced himself again to Joel and me, and. . .asked us to visit him at his apartment on Friday after the end of the Conference."

I quote Jim again about our visit to the Sakharovs (8, pp. 436–37):

> At about four o'clock we left to have dinner with the Sakharovs. The temperature had dropped to –40°F; Chkalova Street between Sakharov's apartment building and the nearest subway station ran in just the right direction to catch the north wind. The keys to survival were long underwear, Russian fur hat, and enough energy to keep moving. . . .
>
> . . .We sat around the table in the small kitchen where Mrs. Sakharov served an excellent pot roast with tsimmas—carrots and plums. . . . The phone rang frequently. Once there was a serious conversation; but otherwise, these seemed to be nuisance calls [initiated by the Soviet secret police, the feared KGB]. When asked about KGB surveillance, Sakharov replied, "It is a matter of no interest to me whatsoever."

I remember how much I was struck by these words of Sakharov at that time. It was very clear that this was no boast or bravado—it was simply a fact. In some way, they summarized the man for me then and later. Here was a man who spoke absolutely the same way no matter who was listening.

I met Sakharov again in July 1979 at a refusenik Sunday Seminar held in the house of Yakov Alpert. I was on my way to a scientific conference in Tbilisi, then part of the Soviet Union, now the capital of Georgia. The next day, I had a meeting, at the beautiful old building housing the USSR Academy of Sciences, with A. Alexandrov, then the president of the Academy. I was able to get this appointment through Roland Dobrushin's efforts on the basis of my being then the president of the NYAS. Our discussion at that meeting ranged over many topics that, I said, were hindering cooperation between US and Soviet scientists. These concerned the problem of antisemitism in Soviet mathematics and the status of refuseniks and of imprisoned dissidents.

The USSR Academy of Sciences was a large powerful government organization at that time. It ran many theoretical and experimental institutes employing many thousands of scientists. It also had some freedom in electing members; these members had many privileges, including extra salaries and the ability to buy, in special stores, scarce commodities (like meat and caviar) at low costs. They could also call for chauffeured Academy cars to drive them. Being an academician was the highest level of scientific recognition.

By the time of my next visit to Moscow, in April 1980, Sakharov was in exile in Gorky. The occasion of my visit was another refusenik conference, the one described earlier, and the political situation was much worse than it had been the year before. The Soviet Union had sent troops into Afghanistan in December 1979, and Sakharov had spoken out against this move. For this and other criticisms of the government, he had been exiled to Gorky in January 1980. Détente was over, and the United States was boycotting the Olympics, which were to take place in Moscow later that year. The National Academy of Sciences of the United States had also decided to suspend its exchange agreements with the USSR Academy of Sciences. The cold war was in serious danger of becoming hot.

It was in this atmosphere that I met again with Alexandrov. Let me quote a few lines from my notes of our conversation to give a flavor of the spirit of the times. They are in the form of a dialogue and are excerpted from my article on Sakharov referred to earlier (8, pp. 442, 445):

> L. Most important, I would like to emphasize again that the reaction of Western scientists is an individual one and it shows how seriously they feel about Sakharov, Orlov and the refusnik scientists. . . .
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> A. These matters are really insignificant compared to the big political issues which determine the future.
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> L. I would like to ask permission to differ here. . . .I strongly believe the relatively small actions by the Soviet authorities with regard to Sakharov and the refusniks would change the climate in the scientific community. This in turn could then influence the political climate and could therefore help in the quest for peace.
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> A. . . .There is no truth in the charge that the Soviets are the aggressors. There must be no preconditions for scientific cooperation. If we start blaming each other, we won't get very far—certainly Secretary Brezhnev, who has gone through the agonies of the Second World War, does not want war. Even Sakharov has said that arms reduction agreements must take precedence.
>
> We should see the way to agreements. Scientists should not set preconditions for cooperation.
>
> L. Thank you very much, President Alexandrov, for this opportunity to see you again and for your offer to look into the matter of the refusnik scientists. I shall certainly convey your message to my colleagues.
>
> A. You must again remember the limited power of the Academy.
>
> L. (Standing up and shaking hands.) Thank you and Good-bye. I hope that you stay well, that things get better, and that we meet again soon.

I went again to Moscow in the summer of 1981, while Sakharov was still in exile in Gorky. I met again with refusenik scientists but could not meet with Alexandrov. On leaving Moscow, I was

met by a KGB officer at the airport who told me that they knew of my activities and that I would not be able to come again to the Soviet Union. I was indeed unable to get a visa to the Soviet Union until 1989 when the "Union" was melting away.

Gorbachev came to power in the Soviet Union in 1985, and in 1986 he called Sakharov in Gorky to tell him that he was permitted to return to Moscow. I was, however, still unable to get a visa until 1989. I therefore did not see Sakharov again until December 1988, when he visited the United States for the first time. There was an emotional reception for him at the NYAS—with tears of joy flowing down many cheeks. I had flown back from Paris (where I was on sabbatical) that day to welcome him to the NYAS.

Sakharov, looking tired but in good health, spoke the truth as he saw it; there were very many human rights, political, and economic problems in the Soviet Union, including dissidents in labor camps, yet the West should support Gorbachev, as his policies were reducing the risk of nuclear war—always the first consideration for Sakharov.

The last time I saw Sakharov was in September 1989, and we were again in the kitchen of his apartment on Chkalova Street. Ann and I arrived at their apartment about three in the afternoon, after having had a big lunch at the home of Vitaly Ginzburg, and found that Elena and Andrei had waited to share their lunch with us. Andrei and Elena were very upset about the Azerbaijani blockade of Karabakh then in effect, which was causing great suffering. I left the apartment with the feeling once again of having been in the presence of moral greatness—a feeling and memory that will always stay with me.

I continued to be active in the NYAS, as chair of the human rights committee for many years and later as a member of that committee. That committee was later abolished by the NYAS when the committee insisted on writing letters of protest to the Chinese government about their jailings of human rights activists while the NYAS, under new management, was doing business with the Chinese.

My human rights activities were then, and are still now, focused on work with CCS, of which I am a cochair. After the dismantling of the Soviet Union, CCS has continued to monitor and protest the violation of human rights of individual scientists, scholars, medical doctors, and students around the world. We do this by writing letters of protest to governments and institutions (see below). Unfortunately, the number of such cases has not decreased after the demise of the Soviet Union. A look at the website of CCS (**https://concernedscientists.org/**), which I strongly urge you to do, shows cases from Azerbaijan, Bangladesh, China, Iran, Israel, Turkey, Russia, the United States, and Venezuela. The need for scientists to get involved in human rights is as great now as ever.

I end my notes with a talk I gave, long distance, on March 3, 2024, at the March meeting of the American Physical Society (APS) in Minneapolis. The leaders of the conference, Smitha Vishveshwara and Paul Chaikin, organized a Nobel Symposium honoring recent recipients of the Nobel Prize in Physics, as well as Narges Mohammadi. She, like Sakharov, is the recipient of the Nobel Peace Prize—hers in 2023, for her work on behalf of human rights in Iran. She is currently in prison in Iran. I was invited to present a five-minute video talk, described below.

AMERICAN PHYSICAL SOCIETY 2024 MARCH MEETING: HUMAN RIGHTS

I will start my very brief presentation with a quote from Einstein:

> The existence and validity of human rights are not written in the stars. The ideals concerning the conduct of men toward each other and the desirable structure of the community have been conceived and taught by enlightened individuals in the course of history. Those ideals and convictions which resulted from historical experience, from the craving for beauty and harmony, have been readily accepted in

> theory by man – and at all times, have been trampled upon by the same people under the pressure of their animal instincts. A large part of history is therefore replete with the struggle for those human rights, an eternal struggle in which a final victory can never be won. But to tire in that struggle would mean the ruin of society. (9, p. 35)

The times we live in are certainly no exception to Einstein's statement about "at all times [human rights] have been trampled upon." The suffering of so many around the world is horrifying and heart breaking. At the same time, the fight for human rights continues, waged by courageous people like Narges Mohammadi (10), who is carrying out the struggle, as she writes, "from behind the high cold walls of a prison."

Another courageous fighter for human rights whom I greatly admire was the famous physicist Andrei Sakharov, who, like Mohammadi, also received the Nobel Peace Prize, in 1975, and who was then sent into exile by the Soviet authorities. Sakharov's memory is honored by the APS with the Sakharov Prize.

I quote from a letter Sakharov wrote to US President Jimmy Carter in 1977 (11):

> It's very important to defend those who suffer because of their nonviolent struggle, for openness, for justice, for destroyed rights of other people. Our and your duty is to fight for them. I think that a great deal depends on this struggle—trust between the people, trust in high promises and the final result—international security.

The important point in both my quotations is a message to those of us who are fortunate not to be in prison or in exile, which is that we are all required to take part in the struggle for human rights here and abroad. Even when it seems, and it often does, that there is very little we can do about the terrible things going on in the world, we can and should show our concern for the victims of human rights violations. One way of doing this, which I hope you will do, is by supporting activities of organizations dedicated to helping our colleagues in distress. Surprisingly, the letter writing by these organizations often have positive effects on how the prisoners are treated, and they are essential for keeping up the spirits of those imprisoned or otherwise mistreated for their advocacy of human rights. Let me mention just a few such organizations with which I am familiar:

- The Committee of Concerned Scientists;
- Scholars at Risk; and
- The Committee for the International Freedom of Scientists of the APS.

Of course, this is not to the exclusion of organizations like the International Rescue Committee and Doctors Without Borders (**https://www.doctorswithoutborders.org/**), who are not focused on scientists or scholars.

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