

LARS ONSAGER

November 27, 1903–October 5, 1976

In Memoriam

This volume grew out of a conference held at the Norwegian Institute of Technology, University of Trondheim, in Trondheim, Norway, June 2–4, 1993. The aim of this conference was to commemorate the 90th birthday of Lars Onsager, who had been a student at the Institute. The conference was (excellently) organized by Per C. Hemmer, together with Helge Holden, Erik Lund, Signe Kjelstrup Ratkje, Harald A. Oye, and Geir Walso. A list of lectures follows this preface.

The meeting, which was very interesting scientifically, also served as a nostalgic reunion for many of us who knew and/or were influenced by Onsager. The comment or lament “if only he could have been here” was heard everywhere. This volume is a small token of our high esteem, affection and appreciation.

Onsager was one of the giants of twentieth-century science—indeed of all times. A description of some of his contributions is contained in the articles by Longuet-Higgins and Fisher and by Philip Lyons which are reprinted in this volume. There are also many personal reminiscences in the articles to which I would like to add some of my own.

I first saw Lars Onsager in the spring of 1953. I was a first-year graduate student at Syracuse University and four of us, all students of Peter Bergmann, drove in my very old car to Pittsburgh where John Richardson had organized a conference on nonequilibrium phenomena.

I can still vaguely remember the meeting room—a standard, shabby lecture hall with a capacity of about 200, which was about one-quarter full. During the first morning session, speakers kept on referring to “Onsager’s relations.” To me, still quite blurry from having driven most of the previous night, this sounded like relatives of the famous Onsager about whom I had already heard in a physics colloquium given by Marc Kac about the mean spherical Ising model. (I still remember Marc Kac, in his inimitable style, describing various unsuccessful attempts to solve the Ising model and then

with a dramatic gesture proclaiming “finally Onsager threw all his heavy machinery at the problem and it simply collapsed under the weight.”) In my then-young mind I thought of Onsager as one of the “old giants,” expecting him to be ancient, and now I was hearing for the first time about his relatives. Great was my surprise then when in the course of the discussion the chairman turned to the perpetually grinning, and not so old looking, gentleman sitting in front of me with the remark “maybe Professor Onsager would like to comment.”

I saw Onsager again a year later at a conference in Syracuse organized by Bergmann. I believe that this time I actually met him and exchanged a few words with him. I then started reading his papers with Machlup. In the winter of 1955 I wanted to apply for an NSF postdoctoral fellowship—expecting to get my degree in 1956—so I wrote to Onsager (not without some trepidation) to ask if he would consent to be my mentor. To my pleasant surprise he answered positively by return mail. When I received the fellowship the next spring I went up for a visit, wanting to get prepared for work in nonequilibrium statistical mechanics, the subject of my thesis, along the lines of Onsager–Machlup. When I arrived at his office, he greeted me with his characteristic grin and started telling me about his work on liquid helium in which he was then engaged in collaboration with Oliver Penrose. He then took me to visit the Penroses in their basement apartment, described by Joan Penrose in an article in this volume, and I had what I believe was my first taste of sherry.

All and all, Onsager must have spent almost ten hours with me that day. He explained to me how he solved the two-dimensional Ising model, and how he was trying to solve the three-dimensional one. He also told me how he was scooped by Brillouin on Brillouin zones. This pattern of interaction continued. He seemed to be the least busy of any scientist I ever knew. During the following year, when I was a postdoc with him, I would drive up to Yale once a week and sit for many hours in his famous overcrowded office with never an interruption by phone or otherwise. He would sit in his chair with his long feet on top of the table, which was crowded with all kinds of journals and unanswered mail, and discuss liquid helium, electrolytes, and a range of other topics about which I knew almost nothing. He particularly enjoyed doing complicated integrals on his yellow pad, including integrals of cubes of Bessel functions. I was awestruck by his erudition at that time and still am. I did find out however how he managed to have all this time for thinking about science. It came from having managed to make himself so useless in administrative and committee jobs that he was totally free from such duties.

Let me take this opportunity to relate some Onsager anecdotes which have stuck with me over the years: The first occurred during lunch in his

house where I had driven up with Oliver Penrose who was passing through New York at that time. I believe it was the summer of 1969. I told Onsager of my work with Elliott Lieb on Coulomb systems which required us to find an efficient way of packing spheres to fill a three-dimensional volume. We did it by using spheres of many different sizes. He understood the problem immediately and grinned, "Oh, yes, when I was a student in engineering they taught us that graded sand (containing grains of different sizes) makes the best concrete."

He also mentioned during that lunch that he had just come back from a conference in Boston where they were discussing the functioning of the human brain. I asked him then what he thought of studies of the brain via electroencephalography about which I had read recently. With the usual grin the response was "trying to obtain information about how the brain works from such studies is like trying to find out how the phone system works by monitoring the fluctuations of the power voltage in the telephone wires."

Finally, I remember seeing him in some crowded lobby of a hotel where there was some conference going on. I told him of my work with Oliver Penrose on metastability and asked him the sixty-four-dollar question: Was metastability an equilibrium phenomenon or a purely kinetic one? After some pause he grinned and replied, "There was a factory in Canada. It manufactured glycerine. Everything was going very well until one winter the pipes froze" ...long pause... "they moved the factory." It took me some time to get his point that metastability was purely kinetic and that once the pipes froze in that factory they kept freezing every winter as the crystal seeds remained there.

Knowing Lars and working with him was certainly a great scientific experience as well as a memorable personal one. Like many others, I was touched by his simplicity, humanity and generosity. Even if we cannot aspire to his originality and brilliance, we can learn much from his conduct and life.

Joel L. Lebowitz