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Lars Onsager Was My Thesis Director

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1. INTRODUCTION

He was just so generous with ideas. His conversation was always full of original thoughts, and "ownership" never came up. Of course, many of those thoughts I was not ready to absorb, to understand. I think he liked it that I didn't pretend to.

It was in the spring of my first year as a graduate student at Yale that I started to "look around" for an advisor. I knew that I wanted to do theory; that still gave me a choice of at least four faculty members. I was in the Physics Department and Onsager was in Chemistry, but he had directed a physics thesis before. (It was Bruria Kaufman's, on the phase transition in the two-dimensional Ising lattice!)

I just walked into his office and stated my business. No, he did not give me an appointment. I sat in his little office for a good two hours, listening to what interested him that morning. At one point he asked me what problems I had solved. Sheepishly I admitted that I usually was able to do the homework in my Theoretical Physics course, and that was about it. "But I'm only 21," I added. "When I was 21, I already had a good one under my belt," said Onsager. I later found out it was the "Onsager-limiting-law" correction to the Debye–Hückel theory of electrolytes! I left with two reprints in hand, "Reciprocal relations in irreversible processes," *Physical Review* 1931, almost two decades old at the time.

Some time later we met at an evening lecture, and I stopped to say hello. There was no small-talk. He had a problem for me. "Calculate the efficiency of irreversible processes." I mentioned thermoelectricity and he nodded. "What does efficiency mean?" I don't remember how I got that out; I was not proud of my question. The answer was typically Onsager.

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"It is the rate of free-energy production divided by the dissipation function." I had to memorize it in order to digest it.

Somehow I managed to find a paper by Tolman and Fine in which they solved that problem. Onsager was not pleased. "First, you solve the problem; then you read Tolman and Fine." That was his style.

In the fall I registered for his course. The catalogue called it Statistical Mechanics. The students called it Norwegian I. There was no textbook. He lectured without notes. He did not collect homework, nor was there an examination. Of course we all took notes, it was a small class, and we talked to each other after class. After he did Fermi–Dirac statistics and started to do Bose–Einstein he commented that there weren't any other kinds of quantum statistics because the Good Lord did not know how to make any other kinds of particles. I especially remember when he had just done the ideal gas of diatomic molecules. During the next class he handed me the chalk and asked me to do the gas of polyatomic molecules at the blackboard.

Petrified, I started to write down a partition function. He was very supportive and essentially led me through a derivation. I had passed. Incidentally, after Lars Onsager's death I had the privilege of being invited by Mrs. Onsager to help organize his papers to make them accessible to scholars through the Yale Library. In 27 boxes of files there was not a single file folder of course notes.

Yale was full of Onsager stories, as you might expect. There was the one about the unexpected guests at his house, apocryphal, of course. Mrs. Onsager sent him out to buy ice cream. When he had not returned after quite a long time, she asked a colleague to check in his office. There he was, working at his desk, the ice cream melting on a chair. It makes a good story, and portrays him as somewhat oddball and scatterbrained and perhaps overly dedicated to his work. My private theory is that cultivating such a public image saved Onsager a lot of time, and he knew it.

2. RECIPROCAL RELATIONS

My second "practice problem" was to calculate a dimensionless quality factor for an isotope-separation device. It was another application of the reciprocal relations. I got a very small estimate. Onsager did not even check my calculation. It was at this point that he suggested a thesis problem. In the second of those 1931 papers there was an "It can also be shown... ." Then came a formula, and it took me better than a month to discover that it was off by a factor 2. That was the jumping-off point for my thesis, which was essentially to explore the statistical interpretation of the dissipation function: Calculate the probability of a fluctuation "path" in terms of the kinetic coefficients describing the linear irreversible processes that take place not too far from equilibrium. The sophisticated reader will note that the Feynman path-integral formulation of quantum mechanics (published in 1948) was not all that well known by 1950.

Others will ask, what is the connection between the reciprocal relations and fluctuation theory? The answer is that Onsager had used fluctuation theory to prove the general reciprocal relations theorem, of which the Kelvin relation between the Seebeck e.m.f. and the Peltier heat is one specific example. He invoked the assumption that the regression of fluctuations (the "expected" path leading from a nonequilibrium state back to equilibrium) is exactly what the linear relations connecting "forces" and "fluxes" predict. That was the kernel of the "fluctuation-dissipation theorem." Incidentally, how do you define a measure in the space of possible fluctuation paths? Fortunately, I already knew, or I might not have understood Onsager's short answer: "Use slalom flags." And now I must mention the role of good luck. One of my friends was a graduate student in mathematics; he told me that Prof. Kakutani was about to offer a short course in stochastic processes. Spontaneous fluctuations.... Of course, I went.

3. CAMBRIDGE, ENGLAND

In the spring of 1951 Lars Onsager told me that he would be a Fulbright Fellow the next fall at the University of Cambridge, and would be in England for a whole year. I had just been granted a 1-year Atomic Energy Commission fellowship to study with him at Yale. What to do? I wrote to the AEC. They wrote back "Fine," and sent me a check for the boat trip to England. Onsager arranged to get me a desk in the basement of the Old Cavendish Laboratory, and I became a "research student" at Cambridge, attached to the Royal Society Mond Laboratory, the traditional home of low-temperature physics.

Not many days after I arrived, Mrs. Onsager invited me to dinner. It was the beginning of a wonderful year. As before, Onsager made it easy for me to see him. I was usually embarrassed about my slow progress, and he agreed that it was slow. Cambridge had its stochastic processes mathematician, H. E. Daniels, whose lectures I attended and who gave me considerable help in his office. Spring in Cambridge was something very special, with daffodils in the "backs" of the colleges. One weekend I hit on the proof of my main theorem. I wrote it up, and phoned Onsager: I wanted to show him a draft of a thesis. He invited me for Sunday breakfast.

His family was apparently off on a trip. It was 9 a.m., and he plunged right into reading. There were smiles, and occasional compliments about well-turned sentences. For me, it was terribly exciting. He had obviously forgotten that he had invited me for breakfast, but what did that matter? At noon he made coffee, and the conversation changed from Gaussian Markov processes to the taxonomy of his garden flowers. When he threw the coffee grounds out of the window, they landed in the flowers and he just muttered, "Good organic matter." I knew I had a Ph.D.

A few weeks later he gave an evening lecture in one of the Cambridge colleges; the title suggested it might be about my thesis problem. That night I learned why his lectures were usually so hard to follow. He just assumed that his listener was as smart as he! The logical leaps were enormous. But the chain of reasoning was all there, and the formulation was elegant. I believe that was the only Onsager lecture I completely understood.

4. THE NOBEL

It was another 16 years before Onsager got the Nobel Prize. Much of his work during those years was about ionic conduction in ice. Was that what the prize was being awarded for? On the other hand, he was the first to use the concept of negative absolute temperatures, in the 1949 paper called, "Statistical Hydrodynamics." Many little vortices roll up into few big vortices—only at negative temperatures, of course. But that paper was little noticed, rarely cited. It contains a footnote about vortices in a superfluid, asserting that the quantum of circulation is h/m! Onsager's solution of the two-dimensional Ising problem was a mathematical milestone, and the first (1942–1949) analytical treatment of the statistical mechanics of a phase transition without a "mean-field" approximation. Was it any of those? Incidentally, was the Prize for physics or for chemistry?

The Nobel Prize was for chemistry, and the citation was the 1931 "Reciprocal Relations." He entitled his Nobel address, "The motion of ions," and told the story of the 1923 "limiting law." To arrive at it he had needed the principle of least dissipation ("derived by Helmholtz"), which he realized was equivalent to the symmetry contained in the reciprocal relations. The story continues with his explanation of the Wien effect (1934) and the ice research of the 1960s. The biological implications are only hinted at. But it is clear that he saw the wide sweep of his work and appreciated its importance.

He was a giant.

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