

Autobiographical Sketch of Hans C. Andersen

I was born in Brooklyn, New York, on September 25, 1941. Both my parents were born in Norway in the previous century. My father, also named Hans, was a marine engineer. My mother, Thora, was a homemaker and worked as a housekeeper. Each had emigrated to the U.S. in the 1920s because of the hard economic conditions in Norway, and each of them suffered through the Depression in the U.S. Each was married, had one child, and then lost the spouse to illness. In this country, they were both part of a close-knit Norwegian immigrant community in Brooklyn and in northern New Jersey. They met each other and married. I suspect that I was somewhat of a surprise, given the age of my parents.

My earliest memories are of growing up in a duplex in Brooklyn, with my parents and my brother John Larsen, who is my mother's son. Several of my uncles and other relatives from Norway, who worked in commercial marine activities, were outside of Norway when Norway was invaded by Germany in World War II. They joined the Allies, bringing their vessels with them, and when they were in the New York area they stayed with us in our little house in Brooklyn. My father's daughter, Alfild, lived in Norway, and I first met her in 1968 on my first trip to Norway.

My parents had little formal education in Norway, probably not even the equivalent of a sixth-grade education in the U.S., but my father had been to a school that trained him for his marine engineer's license. He loved the mathematics that he did know, and I enjoyed the math puzzles he would give me. I could solve some of them, but others were too difficult until I learned algebra in junior high school. My parents believed that an education was essential for their children, and from my earliest days they made it clear to me that I was going to college. As my father would say, "If you go to college, then you won't have to work for a living!" He was right, given the definition of work that my parents knew. When I started publishing scientific papers, my father liked to carry around reprints of my articles in *The Journal of Mathematical Physics* in order to impress his friends, even though neither he nor his friends could possibly understand them.

I attended public schools in Brooklyn and then Stuyvesant High School, a public high school in Manhattan that focused on science and math. A National Merit Scholarship made it possible for me to attend MIT as an undergraduate, starting in 1958. There I received what I recognized at the time to be superb training in math and science and an outstanding introduction to the humanities and to economics. I remember taking a fascinating course in group theory from Al Cotton, at a time when he was writing his book on the subject. The course met at 8 a.m., but I had no trouble getting out of bed to attend class. As a senior, I did undergraduate thesis work in the MIT Spectroscopy Lab in the group of Richard Lord. I learned a great deal about spectroscopy, but I also learned that experimental work was not for me. (Actually, I had several much earlier indications of this, especially in organic chemistry laboratory courses.) During the summer after my senior year, I worked at the Proctor and Gamble Research Labs in Cincinnati with Tom Flautt. This gave me my first experience with the theory of high-resolution NMR and with computer programming. At this point I was sure that I wanted to be a theoretician.

I decided to do my graduate work at MIT, against the advice of half the faculty I talked with about this decision. I did my Ph.D. research with Irwin Oppenheim, the only faculty member who ever gave me a C grade in an undergraduate course. As a senior, I had taken his introductory graduate-level statistical thermodynamics course, where I was first introduced to Irwin's rigorous approach to theoretical work. I clearly didn't "get it" at first, but I was certainly attracted to it. (Fortunately I subsequently got an A in his advanced course.) I owe a great deal to Irwin for what he taught me about science (and about food and wine) and for the training he provided. He is a wise and generous teacher, and his way of thinking about statistical mechanics and about science is a permanent part of my understanding. During the summer of 1963, I had the opportunity to work with Kurt Shuler at the National Bureau of Standards in Washington, D.C. My desk was in the statistical physics section, where I also had a chance to meet Mel Klein and the large number of permanent staff and postdocs in that section. Bob Zwanzig would often visit the section. This seemed to me to be the hub of the statistical mechanics universe, and being there was a great experience for me as a young graduate student.

During the last year of my graduate work and my two years of postdoctoral work, I was a Junior Fellow in the Society of Fellows at Harvard University. I lived in Leverett House, did research in the chemistry department, and sat in on courses that interested me in everything from molecular biology to social psychology to biblical history. At this time I started collaborating with David Chandler. We had met at MIT where I was a teaching assistant in one of his courses and where he had done undergraduate research with Irwin.

In the summer of 1966, a friend of one of the graduate tutors in Leverett House arranged a blind date for me with June Jenny, a graduate student in the Harvard School of Education, who was about to leave Cambridge for a Ph.D. program in biophysics in Colorado. After a six-month transcontinental courtship that was choreographed in conjunction with a series of job interviews for academic positions, we were engaged. We held the wedding the following June.

Academic job hunting in the mid 1960s was a very different matter from what it has subsequently become. It was much less formal, and much less was demanded of the candidate, usually just a seminar and a day or two of conversations. In some cases, it was not clear to me that I was being considered for a faculty position until after I had received a written or verbal job offer. Moreover, universities were expanding, and jobs were plentiful. I had several offers from excellent institutions and decided to come to the Stanford University Chemistry Department in 1968, where I have been ever since.

June completed a Ph.D. program in the genetics department in the Medical School at Stanford, and after a short postdoctoral period at Stanford she took a position at IBM Corp. as one of the first biologists to work for that company. She had a sequence of increasingly challenging scientific and managerial positions and is currently on the corporate staff. We have two sons—Hans, who graduated from Stanford and is now working for Microsoft in Redmond, Washington, and Albert, who is a sophomore in computer science at Stanford.

My collaboration with David Chandler continued after I came to Stanford as an assistant professor and David went to the University of California, San Diego, to postdoc with Kurt Shuler, who had recently moved to the chemistry department there. It was at UCSD that David met John Weeks, also a Shuler postdoc, and the Weeks–Chandler–Andersen collaboration was formed. We were young, energetic, and ambitious, with differing skills and temperaments, but we worked together well. I look back on that collaboration fondly as some of the best research I have ever done, as well as the most enjoyable. (Kurt was remarkably tolerant and supportive of two of his postdocs spending so much of their time collaborating with one of his former summer students.)

The work with David and John on the equilibrium structure and thermodynamics of simple liquids involved me in a subject that has been a continuing theme of my own research. It introduced me to a style of theoretical research that was primarily analytic in nature, that used computers to evaluate quantities that were too complicated to evaluate by analytic methods, and that tested theoretical approximations by comparison with computer simulations. In those days, relatively few people were doing computer simulation studies on liquids, and we had to rely on simulation data published by others. But during the 1970s, as computers became more powerful and less expensive, the technology needed for computer simulation became more readily available. Starting in about 1980, I became interested in both doing molecular dynamics simulations myself as well as developing new simulation methods, and this became the second major theme of my research. I acquired a minicom-

puter system and a fast floating point array processor. With the inspiration and assistance of Kent Wilson at UCSD and with some very able postdocs, Jeffrey Fox, William Swope, Tariq Andrea, and Kyoko Watanabe, I ventured into computer simulation studies of supercooled liquids and of water and aqueous solutions. This work continued in the 1990s with collaborations with William Swope (who had moved to IBM Corp.) on very large scale simulations of two-dimensional melting and of homogeneous nucleation and with Walter Kob on very long time simulation studies of supercooled liquids and tests of mode coupling theory. A third major theme of my research is the development of diagrammatic methods for calculating time-dependent correlation functions and transport coefficients of equilibrium fluids. This work was begun in the mid 1990s and continues today. In addition to these major themes, there have been many variations, since the interests of my students and the suggestions of collaborators have carried my research into such fields as bilayer phase transitions, photodetachment spectroscopy, and energy transport in random materials, as well as many others.

I have probably worked harder than my father ever imagined—not working with my hands in manual labor but working with ideas and people and institutions. While ideas, people, and institutions can sometimes be difficult, troublesome, and even intractable, they can also be inspiring, cooperative, and supportive, and on balance I am very fortunate to have a career that is as interesting and rewarding as it is.

Hans C. Andersen