Sudip Chakravarty

If anyone had asked me if I realized when I was young that I would become a physicist, I would have had to emphatically say no. Rather, I had ambitions to become a Cricket Star, a writer, a poet, an artist, but most likely a musician. These are not, however, the professions my family (except my mother) approved of, especially my father who dreamed of me becoming a lawyer, nay a barrister, a graduate from Oxford. He himself became a judge of the Dhaka High Court, and was about to become a judge of the Supreme Court, as the first military coup led by General Ayub Khan in Pakistan took place. We fled to India instantly, never to return to Dhaka. A part of my childhood was wiped out forever. Yes, I did build crystal radio sets and had fun with Chemistry sets, but to become a professor of physics---never in my wildest dreams.

My flute teacher once remarked to me that in his profession very few are self-taught. I found this rather odd at the time, as I thought of myself as being self-taught in physics. But upon further reflection I realized that the role of teachers in setting a style and craving for research when one is young is often remarkable. Thus, I cannot ignore the countless hours Professor Kamal Dutta spent with me at the Coffee House while I was at the Delhi University where I completed both my bachelors and masters degrees. In turn, I spend countless hours with my own students at the UCLA's Coffee House, Kerckhoff. Professor Dutta, or Kamalda, as I used to call him affectionately, instilled a sense of beauty in physics, discussed the powerful notion of symmetries, the mysteries of quantum mechanics, and introduced me to some classic papers and books, such as Dirac's book on quantum mechanics.

I was fortunate to be a postdoc with Walter Kohn during the first year of ITP Santa Barbara. Walter is a remarkable mentor in every respect. I learned much from him about precision of thought, although we never seriously collaborated. He even encouraged me to take up flute after seeing that I could obtain a reasonable tone at first try with his flute. Playing flute still is one of my favorite past-times, as the other musical instrument that I played for years, sitar (mostly self-taught once again), is painfully becoming harder and harder.

It was not until I was appointed an assistant professor at Stony Brook that something special began to happen. I stumbled upon the idea of macroscopic quantum tunneling in superconducting devices, then introduced to condensed matter physics by Tony Leggett. I was able to quickly put together a paper (the fastest I have written) showing that with increased dissipation the device may undergo a transition from a quantum mechanically coherent to a classical state, oddly the state that carried supercurrent. This was a symmetry broken state not realized by Leggett and Caldeira. Shortly thereafter I was to share an office with Tony at Cornell (en route to Illinois) during a sabbatical spree and was thoroughly encouraged by Vinay Ambegaokar to pursue my direction of research. Tony and I collaborated on the dynamics of the two-state system coupled to a heat bath, which was published jointly in the Physical Review Letters, and which later blossomed into a major review article in Reviews of Modern Physics to which many contributed. I

pursued the idea of dissipative quantum phase transition quite actively and applied it to Josephson junction arrays, which to this day is a subject of active interest.

The days at Stony Brook were colorful. I acquired a good friend and colleague in Steve Kivelson. I had a number of spectacular students: Doug Stein, Lit-Deh Chang, Rajiv Singh, Peter Kopietz, Peter Scharf, and Shubha Tewari. Rajiv and I were brave enough to tackle successfully the difficult problem of high order series expansion of the Ising spin glass completely from scratch, in spite of being thoroughly discouraged by a leading figure in statistical mechanics, who remarked that we were sure to fail without the database of King's college, London. There were also some near misses that would have never occurred had I had more confidence. I stumbled upon the idea of quantum Monte Carlo method of Handscomb, but became fearful of the uncharted numerical territories (cryptically but correctly alluded to by Richard Feynman at a meeting of the EST Foundation in San Francisco that I attended). Generally, my ideas on the subject were considered to be complex and unwieldy, as compared to the world line method of Hirsch and Scalapino. The exception was a lovely hand written letter by Handscomb, which still belongs to my collection of memorabilia. He was amazed that someone would attack the same method he had invented after so many years, and which he considered to be "simply a flash in the pan". At present Handscomb's method (or the stochastic Monte Carlo) is the method of choice, thanks to my former student Olav Syljuasen and Anders Sandvik. Another remarkable disappointment came from the (1/N)-expansion for the Kondo problem, which I developed. The paper was rejected from the Physical Review Letters (what a surprise!), never to see the day of light beyond the Bulletin of the American Physical Society. Under pressure from a distinguished physicist visiting Princeton from India, I basically gave up. The problem involved a subtle restoration of a broken symmetry that was well ahead of its time. The neatly type written paper characteristic of those days still survives in my file folder. The subject was subsequently beautifully developed by Nick Read and Dennis Newns. Dennis was gracious to send me a lovely note later. The moral of the story is that we often succumb to peer pressure and the establishment, especially when we are young. It is only because my portfolio was diverse that I survived.

At Stony Brook my daughter Leila was born, a source of great joy in spite of the hardship of being an assistant professor in the midst of a large faculty consisting of very few condensed matter physicists. At Stony Brook the Late Albert Schmid and Ute showed up for a long visit, lived in our small rented house (which we had to vacate every summer for the owners, who would arrive for summer visits from New York) and took many long hikes with Leila on his back along the shores of Long Island Sound. A deep friendship developed. Albert and I were bent on rewriting the complex digrammatics of the weak localization theory invented by the Russians in terms of simple Feynman path integral, which was published in Physics Reports, but was hardly a review of any kind. The other paper we wrote together in that two-month period is I think a cute forgotten paper on the path integral solution of an open transmission line in the context of the pioneering macroscopic quantum tunneling measurements of Richard Webb at IBM. In a different guise the method showed up later in a more successful paper.

Another sabbatical sent me to Harvard where I was struck by an inspiring seminar by Bob Birgeneau where he discussed his recent experiments in quantum magnetism in the context of high temperature superconductors. He has been, and still continues to be, a great source of inspiration to me. Along with Bert Halperin and David Nelson, I developed a theory of quantum magnetism and explained these experiments, to a degree of success that is embarrassing; this is perhaps the only theory in the context of high temperature superconductors that has succeeded. Bob (now the Chancellor at UC Berkeley) remains a great friend, sending me recently a note on the occasion of my 60th birthday saying, "…we made history together". Indeed, our theory appears to be completely intact and the notion of quantum criticality introduced there has developed into a mature subject. It was at that time that Ray Orbach, then Provost of UCLA, invited me for a possible job, almost out of the blue. My wife Nancy and my daughter Leila and I arrived at UCLA on a gorgeous Fall day with its rainbow colored flower beds all over the campus. I instantly fell in love with it. It was hard for us to leave Stony Brook, where my daughter was born, and I was just beginning to put my roots down. But the attraction was too great, and Ray was remarkably persuasive. I was indeed sad because the encouragement of Professor C. N. Yang to the untenured life of an assistant professor was critical to my Stony Brook days. Ray and I wrote a paper on the nuclear magnetic resonance of the antiferromagnetic phase of the high temperature superconductors that was subsequently verified in detail by Slichter and his coworkers at Illinois, a source of great pride to both of us.

At UCLA a new life began, both professionally and otherwise. I worked with Phil Anderson intensely on the mystery of high temperature superconductivity, a long distance collaboration that was remarkably successful. Phil and I continue to be great friends notwithstanding many ups and downs, but never a dull moment physics-wise. Steve Kivelson and I pursued a radical idea of superconductivity in doped C60, which finally succumbed under social pressures, but which we both still feel to be essentially correct. During this period at UCLA, I enjoyed my contact with my student Guillermo Castilla (who also taught me to play tennis), with whom I performed one of the most difficult calculations I have ever laid my hands on, namely the structure of the renormalization group of infinite number of high gradient operators of the non-linear sigma model. Throughout my career I have interacted with many brilliant postdocs and students, all listed on my Web page.

At UCLA I ferociously recruited Chetan Nayak, almost straight out of graduate school. I have learned immensely from his insights and am proud to have collaborated with him on so many papers. Chetan and Bob Laughlin were instrumental in helping me develop the idea of a novel competing ordered state, the d-density wave that I am pursuing vigorously to this day even after a decade and, once again, under tremendous social pressures. I was sad to see Chetan leave UCLA, as I was to see Steve Kivelson depart for Stanford. The theory of d-density wave is thriving again with a new burst of remarkable quantum oscillation experiments of Louis Taillefer, Cyril Proust and their collaborators. Whether it will provide a clue to the complexity of high temperature superconductors remains to be seen. It is quite remarkable that the observed broken symmetries in condensed matter are so few, given the multitude of possibilities and their consequences.

I would be remiss not to mention many physicists I have met during my twenty plus summers at the Aspen Center for Physics, in particular Elihu Abrahams. I also miss very much my dear friend and collaborator Dick Norton at UCLA, who recently passed away. My intellectual development would have been impossible without the help of my brother during my growing years, the Late Sukhamoy Chakravarty, a brilliant economist, and a person of great intellectual integrity. I owe greatly to my mother Bindu Basini Devi, who instilled a sense of love for life, and to my wife Nancy and my daughter Leila.