John Hubbard 1931-1980

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University College, London
June 18, 2013

Although John Hubbard was three years older than me, we both were students at the same time. John was both an undergraduate and PhD student at Imperial College, London, while I was an undergraduate at Cambridge, and moved to Cornell to study for a PhD under Hans Bethe. In September 1958 I started in a postdoctoral position at what was soon to be called Lawrence Berkeley Laboratory (LBL), and was then known as the Radiation Laboratory. At around the same time, John Hubbard arrived on a leave of absence from Harwell, at the Physics Department of the University of California, Berkeley, which is on the flat land just below the steep hill on which LBL is perched, but I do not think that he spent much of his time in the Physics Department.

There was a bus running regularly between the campus and LBL, but I did not establish useful contacts with the UCB Physics Department until I started working on superconductivity theory a few months later. I approached Charles Kittel, who was not much interested in my work, although ten years later he did publish a paper covering much the same ground as one of the papers I wanted to talk to him about. He introduced me to the immensely helpful Michael Tinkham, and to a group of postdocs, which did not include John Hubbard, but did include the outstanding Pierre-Gilles de Gennes, who was also a post-doc in the Physics Department.

We did, eventually, make some contact with our neighbors, two of whom also worked on theoretical physics. We usually headed off on foot in the morning and did not linger to socialize. One evening there was a party at an apartment next door to us, and we realized that the hosts were a couple we had known at Cornell. Hugh De Witt had been a Physics graduate student a year senior to me, whose first job was at the Lawrence Livermore Laboratory (LLL) which Edward Teller ran. There was a shift towards controlled nuclear fusion at LLL during our year in Berkeley, which was signaled by a meeting outside the Livermore security fence, which I went to, where the enthusiasts proclaimed that the goal of controlled fusion would be attained within thirty or forty years, with perhaps another twenty years before it became economically self-supporting. The predicted time scale has varied in the past fifty years, but it has not shortened. Hugh shifted his work in that direction, and began to publish in the open literature. Later he became an outspoken opponent of nuclear weapons, but remained at LLL.

The other pair of neighbors we got to know were Berni and Esther Alder. They were somewhat older than us, and they have remained our friends since then. Berni was a theoretical chemist, who worked some days in Livermore, and some days at the UC Berkeley Chemistry Department. Berni's influence was important for me, as his simulation of two-dimensional melting planted the idea in my mind that a two-dimensional solid might have a melting transition, despite the arguments to the contrary by Landau and Peierls in the thirties. Esther had been an Instructor in French at UC, and produced a son, Ken, that spring, who became a novelist and a historian of science (Ken Alder, *The Measure of All Things*, describes the attempt to measure the earth's circumference in order to calibrate the meter, near the beginning of the revolutionary wars).

I think it was De Witt who first arranged for John and I to meet in Livermore with other interested people to discuss many-body problems. I cannot remember the details of our discussions, and I do not think I took notes. Although I had been working on nuclear matter when I was working with Bethe, and Hubbard's work was more directed towards the electron gas and to metals and molecules, we understood each other's language, and we drew the same sort of diagrams, probably because both of us had learned to organize perturbation series for such many-body systems from Jeffrey Goldstone. Jeffrey was a second year graduate student working with Bethe during my first graduate year at Cambridge. When I went off to Cornell with Bethe in the autumn of 1956, Jeffrey moved to Copenhagen for his final year as a PhD student. We met up again in the summer of 1957 when we both attended a nuclear physics conference in Pittsburgh.

In Berkeley I was excited to discover that the Cooper instability in superconductivity could be found by summing Goldstone 'ladder diagrams' while respecting particle–hole symmetry.

John and I each acknowledged Jeffrey Goldstone's important influence on our work, but I am not sure that either of us had much influence on the other over the next four years or so. My present knowledge of Hubbard's work is primarily based on rereading his publications. Probably I saw John again on a visit to Harwell during this period, but I cannot remember a specific occasion on which I saw him there.

Looking back on his earliest publications and on the number of citations they have obtained, it is clear that they have been influential, even if I only gave them a glancing reference in my 1961 many-body theory book. One reason for this is that although I was interested in real nuclear physics and in BCS superconductors, I paid little attention to real metals, until Mott interested me in them a few years later.

The first three papers by Hubbard that I could trace were published in 1954 and 1955 in Proc. Phys. Soc., and were essentially adaptations of work on a homogeneous electron gas by Bohm and Pines and others, with modifications that to take account of the confinement of the valence electrons within identical periodic cells. The first of these papers attracted only 10 citations in the literature, the second was cited 50 times, and the third, entitled 'The Dielectric Theory of Electronic Interactions in Solids', was cited 99 times. It is not obvious that Hubbard had much intellectual support at this time. The only acknowledgements are to his advisor, Dr. S. Raimes, and to the University of London for a Postgraduate Studentship. Under the circumstances, the attention he attracted was a tribute to his ability and to what he had to offer to physics.

There was a second important group of three papers published by Hubbard in 1957-8 on the description of collective motion in terms of many-body perturbation theory. These were published in Proc. Royal Soc. The first of these had 478 citations, the second 186, and the third 93. The first of these papers thanks John Bell for much helpful advice and criticism, and the list of references includes many papers which I cited in my PhD dissertation or in my book prepared two or three years later. It was shortly after this that we got to know one another in Berkeley.

An outlier from this group of papers was a paper on Calculation of Partition Functions published in *Phys. Rev. Letters* in 1959 which was cited 756 times. This showed that the same linked cluster expansion used for the ground state energy could be used for the free energy at nonzero tempaeratures. This insight was not unique to John, and indeed Takeo Matsubara from Kyoto published a similar result in 1955, to which I made a small correction in 1957. This result became very widely known after the publication of the famous book by Abrikosov, Gorkov and Dzyaloshinskii.

I was at Cambridge between 1961 and 1965, and during that period Mott tried to involve more physicists in the study of metal–insulator transitions. I particularly remember a high-powered informal international conference that he organized on the topic. One talk I particularly remember was by Walter Kohn, which characterized a low-temperature insulator as a material in which the electrons at the Fermi energy have wavefunctions that fall off exponentially with distance from the point of their maximum value.

Some time in 1964, Mott drew special attention to the Hubbard Model and got John to talk about it. What I particularly remember is that Mott also introduced me to Junjiro Kanamori, who had done work similar to Hubbard's on the magnetic properties of transition metals. Mott took us to a restaurant close to the Cavendish, which was still in its original site on Free School Lane, and left the three of us to discuss these matters over lunch. All I can remember after so many years is that we had an interesting discussion, and that I made some resolutions to follow up on these ideas. I am sure that none of my work led to a publication.

I did not stay in Cambridge, but moved back to Birmingham in 1965. In Birmingham Joe Vinen was the physicist who interested me most, and superfluidity and superconductivity seemed the most promising directions for me to follow. I did not make any substantial contributions in this area until I started collaborating with Mike Kosterlitz in 1971.

In the mean time I did begin to get seriously involved with the effects of disorder in solids. On a personal visit to Bristol in 1969 I had lunch with John Ziman, and he told me that his fresh insight into Anderson's 1958 theory of localization by disorder had been rubbished by Mott and Anderson at an Amorphous Semiconductors Conference in Cambridge. After a few month's thought on these claims and counterclaims I concluded that Anderson and Mott were right, and Ziman was wrong. Five years later the Nobel Committee came to the same conclusion.