Berkeley Days

How Great Events Shaped Our Careers

For Elihu Abrahams at his 80^{th} birthday celebration

Elihu received his undergraduate degree at the University of California, Berkeley, in 1947 and stayed there for his graduate studies in Physics. I started graduate studies in physics at Berkeley a year later in fall 1948, meeting Elihu soon after in shared courses.

It was then three years after the end of the Second World War, and physics had become a popular subject. The dramatically successful roles physicists had played during the war in the development of the atomic bomb and radar, for example, had brought physics into the public consciousness, imbuing it with intellectual excitement and a kind of glamour and bringing an awareness that in doing physics one could have, however remotely, an impact on society. There was a new interest in supporting basic research by both government and industry; there was a sense that jobs were available. The "GI Bill", a federal law which subsidized the college education of returning veterans made it possible for many to prepare for graduate study who might otherwise never have gone to college. For Berkeley, the result was a student body of over 400 in physics. Nevertheless, the classes, though large, were not so large as to seem impersonal. The students got to know each other, and so it was with Elihu and me.

A typical pattern for graduate studies there and then was to take courses, pass the preliminary exams (fortunately reduced to one in number in our time), pass a caricature of a foreign language exam, and then start one's Ph D research. Elihu and I reached that stage at the same time, the end of the spring semester of 1950.

However, while we were enjoying the intellectual delights of physics and rising to its challenges, and also enjoying the Northern California climate and the beauty of Berkeley and of the San Francisco Bay area, the world was moving on. Like World War II and the great expansion of the interest in and support of physics after its end, the intensification of the Cold War with the Soviet Union at the end of the 1940's was to have a major effect on its practitioners, particularly so for the cohort of students at Berkeley to which Elihu and I belonged.

Within the United States, the anxiety and uncertainty generated by the Cold War resulted in over reactions by governments at the state and national level. In California, the state legislature moved to impose a loyalty oath on all state employees in the spring of 1950. With Berkeley being a campus of the University of California, all faculty members, post docs, graduate assistants, and staff members were required to sign it. There was an immediate strong protest by the faculty at Berkeley, and the faculty senate moved to resist its imposition. Negotiations between the faculty senate and the legislature proceeded during the spring semester, leading to an agreement exempting the Berkeley faculty and staff from taking the loyalty oath.

Thus, when my wife and I, newly married, departed Berkeley in June, all seemed back to normal. When we returned at the beginning of September, I went immediately to campus to meet with the professor whom I hoped would accept me as a Ph D thesis student. As I entered campus, the first person I met was Elihu. After a brief greeting, Elihu informed me that the agreement between the legislature and the faculty senate had broken down, that the loyalty oath had been imposed on the university, that all the theoretical physicists in the department either had resigned in protest or had declared their intention to do so, and that there was no one to work for. I rushed to see my faculty advisor, Francis Jenkins, gravely uncertain about my future.

I learned from him that he had been given the task of rebuilding the theoretical group within the department. To start doing so, he had attended the annual summer school on theoretical physics that George Uehlenbeck organized at the University of Michigan. There Jenkins met a young solid state theorist named Charles Kittel, who impressed him and who, chafing under the unpleasantness of Shockley's leadership of the theoretical group at Bell Labs, was open to leaving it. Jenkins arranged an offer of an Associate Professorship for Kittel, who was quite uncertain about moving out to Berkeley. Jenkins convinced him to come for the fall semester on a trial basis.

Jenkins encouraged me to talk to Kittel about his research interests and gave me reprints of Kittel's recent papers to read. I had had no intention of working in solid state theory. I had planned to become a particle theorist. My only contact with solid state physics had been an uninspiring reading course for which a professor of electrical engineering named Silver had gathered a group of students to work through Fred Seitz's "Modern Theory of Solids" with him. [It was some time before I realized how prescient Silver had been.] Moreover, Kittel had committed himself only to the fall semester. Despite the uncertainty, there was nothing else to do. Kittel was the only game in town, so to speak. So, I went to speak to Kittel. He gave me a problem to do, and I became his student, at least while he was there.

As the only game in town, Kittel attracted six other desperate students: Elihu, Al Overhauser, Fred Keffer, Harvey Kaplan, Yako Yafet, Jack Tessman, and John Weymouth. He gave each of them a problem in a quite different area of solid state physics. I had no idea how impressive a feat that was until I started my own research group. He started a solid state theory seminar which was quite well attended despite there being no other solid state physicists in the department, perhaps because of the novelty of the subject for Berkeley. We seven were there of course, but many faculty members attended and showed interest. Kittel talked himself and assigned seminar topics to us, which was a bit scary because we had to explain to the faculty from other subdisciplines matters we barely understood ourselves.

In his assignment of research topics to us and in his organization of the seminar, Kittel displayed a grasp of the entire subject of solid state physics as it was known then. Elihu worked on lattice relaxation in ferromagnets. Overhauser worked on electron spin relaxation in metals, work which evolved into an early study of electron correlation. Kaplan worked on the exchange integral in ferromagnetic iron. Tessman worked on ionic polarizabilities. I worked on size effects in metals. I forget what Yafet worked on, something in magnetism, and what Weymouth worked on. [Perhaps Elihu remembers.] What we discovered only later was that Kittel was already working on his famous 1953 textbook "Introduction to Solid State Physics", consolidating his vast knowledge of the subject, so detailed that it extended to page numbers of key references.

Our circumstances were clearly much better than they were before Kittel emerged as our possible rescuer from the devastation which followed the imposition of the loyalty oath, but uncertainty remained. Kittel left at the end of the fall semester, returning to the Bell Labs, and we did not know whether he would return. Nevertheless, we seven remained together as a group, continuing the seminar informally, working away individually on the research problems he had assigned us, and hoping for his return.

While back at the Bell Labs, Kittel continued his search for an academic position. Clarence Zener had left the University of Chicago to become director of the Westinghouse Research Labs, and Chicago was looking for a senior solid state theorist.

Kittel was offered a full professorship there, and Chicago accepted his condition that he bring there his seven students from Berkeley, unbeknownst to us. He informed Berkeley of the Chicago full professorship. Berkeley matched the Chicago offer. Kittel accepted, returned to Berkeley in the summer of 1951, and solid state physics was born at Berkeley.

Upon his return, Kittel resumed teaching and supervising his group of graduate students. We were now six, as Overhauser, brighter than the rest of us and newly married, had already finished his thesis, wanting to get on with his life. In addition to these pedagogical responsibilities, Kittel started building up solid state physics at Berkeley, evidently with the strong support of the department. He was instrumental in attracting Walter Knight to Berkeley, he of the Knight shift, and he convinced Art Kip to leave MIT and join him at Berkeley. That these were the first experimental solid state experimentalists hired reflected Kittel's conviction that nuclear magnetic resonance (Knight) and electron spin resonance (Kip) were among the most fertile techniques then available. Within one year of his second coming, Kittel had firmly established the nucleus of what was rapidly to become one of the great academic centers of solid state research.

By the end of the academic year '51/'52, we had all finished our Ph. D. research. Elihu had continued working on ferromagnetic relaxation, his thesis topic, and I migrated from size effects to ferro-and antiferroelectricity to nuclear quadrupole spectra in solids, my thesis topic. Now put yourselves in Kittel's position. Over a period of a year or so, you have to find jobs for seven students. That would be very difficult now. Fortunately for us, times were different then. Physics departments were growing, and many of them wanted to create solid state physics groups, very few of which existed then. As a consequence, we faced a far more favorable relation between supply and demand in our field than has existed for recent decades. On a larger stage, the slowing of the exponential growth of science, which had persisted for three centuries, was still a decade or so in the future. To us, it seemed as though Kittel simply decided on which available job he should allocate to which student. He had already sent Overhauser to work as a post doc with Jimmy Koehler at Urbana. He kept Elihu on as a post doc to work with him for another year at Berkeley. He arranged an Assistant Professorship in the physics department at Pitt for Fred Keffer, where Fred spent his career. Yako Yafet got a post doc at Urbana. Harvey Kaplan got a post doc in Slater's group at MIT. Jack Tessman had a faculty position at Tufts. John Weymouth went to Nebraska. I went to Chicago with an instructorship in the physics department and the institute for the study of metals. To emphasize how different times were then, I had an offer from Chicago three weeks after Kittel told me that he'd "put me in Chicago". I'd written only two papers, and there was no interview. All this took place in June for a job to start in September.

That ends my story of the Berkeley days. Please realize that it is a story, not a piece of historical scholarship. I have relied almost entirely on my memory, with few attempts to confirm those memories with documentation. Many details may be wrong. I may have conflated times and events. However, I do believe that in the main, what I have written conveys the sense of what it was like for Elihu and myself at Berkeley.

Now let me continue with a few brief remarks about the intervening years. Regarding recognition received by that group of students, three of the seven, Elihu, Overhauser, and I, became members of the National Academy of Sciences. Overhauser was awarded the National Medal of Science. Regarding areas of research, all of us were heavily influenced in our choice of problems by Kittel's interests during our early post Berkeley years. One look at the title of Elihu's early papers confirms that in his case. Overhauser's National Medal of Science was awarded for his discovery of the Overhauser effect, which found very important application in MRI. Perhaps more significant for me was the influence of Kittel's style, which emphasized cutting rapidly to the core of a problem with very simple physical arguments. I see it also in Elihu as I listen to his comments and questions in seminars. Also, Kittel's effectiveness in building up solid state physics at Berkeley has to have been a model consciously or unconsciously for both Elihu and myself. Elihu has had remarkable success in building solid state theory here at Rutgers, as all of you know. I was given the responsibility for building up solid state physics at Chicago a year after I got there. Though it scared me half to death, aware of Kittel's activities at Berkeley, I naively thought it was something one does after one arrives at a place. For Kittel, it was bread cast upon the waters. Fred Reif and Leo Falicov went directly from Chicago to Berkeley, and Marvin Cohen went there after a year at Bell.

Elihu's influence on physics at Rutgers has been profound, but there is another channel through which Kittel and Berkeley have influenced this department. David Vanderbilt and Karin Rabe also descend from Kittel. Their line of ascent is John Joannopoulos to Marvin Cohen to Jim Phillips to me to Kittel. Less directly, Joe Sak and Premi Chandra were my post docs, and David Langreth was a post doc in my group, so they have a collateral line of ascent to Kittel and Berkeley.

Now for a brief note on terminology: You will have noted that throughout I have referred to as "solid state physics" what you all know as "condensed matter physics". Elihu is not a member of a "solid state theory" group. I did so in an attempt to be historically accurate. During the times I have been talking about, the field was called "solid state physics", and the relevant Division of the APS was the Division of Solid State Physics. Physicists doing solid state physics were nevertheless interested in superfluid helium, in some aspects of hydrodynamics, in neutron scattering studies of liquid structure and dynamics, and in understanding the electronic properties of liquid metals and semiconductors. The common theme was condensed matter physics. Accordingly, when I was chairman of the executive committee of the Division of Solid State Physics, I proposed to the committee that the name of the Division be changed to the Division of Condensed Matter Physics. The name change was approved by the Division membership at the 1970 APS March Meeting. That is in part why Elihu is a member of a "condensed matter theory" group.

Finally, let me comment on my subtitle: How Great Events Shaped Our Careers. The great events referred to were the end of World War II, the Cold War, the communist witch hunt during the early years of the Cold War, and the cessation of the exponential growth of science. Added to this were smaller matters like Bill Shockley's psychological problems and Clarence Zener's departure from Chicago. Were it not for the great events, Elihu and I and the other members of our cohort would not have become condensed matter theorists. Kittel would not have gone to Berkeley to build solid state physics there. The composition of the condensed matter theory group here would be very different. Indeed, the pattern of development of condensed matter physics in this country and to a lesser extent elsewhere would have been quite different. One example might suffice. Pseudopotential theory might have developed later and less effectively. Band gap engineering, central to the semiconductor industry and dependent in part on pseudopotential theory might have developed later, leading to a different history of the semiconductor industry.

Warmest congratulations Elihu! I am very sorry that I cannot be present to say that to your face, but I am very pleased that once again we are together in the same department.

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