FELIX BLOCH
Reminiscences of A Graduate Student

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My first meeting with Professor Bloch was shortly after V-J Day on a hot afternoon in the fall of 1945. We met in his sparse, high ceiling, cool office in the old physics corner at Stanford University where he described with the elegant use of chalk and blackboard a new and exciting but simple concept – Nuclear Induction. The blackboard and his unconscious cross temple stroking of his forelock were to remain an important part of communications on this concept which won him, along with Edward Purcell, a Nobel Prize in 1952, and won me a Ph.D. in physics in 1949.

After an hour or two of discussion about the project, we agreed that I should come to Stanford as a Ph.D. candidate, working with Bloch and Hansen on the yet untested but carefully researched idea of Nuclear Induction. Stanford was a little more informal in those days. I became the first postwar Physics graduate student without filling out forms or submitting transcripts. This unstructured environment was the mark of a Country and University in transition, and was most conducive to rapid progress.

My introduction to Felix Bloch came through Dr. Daniel Alpert, my group leader at Westinghouse where I had worked on passive microwave devices prior to being assigned to the Manhattan Project at Berkeley. Alpert's major professor was Prof. William Hansen and he had worked with Bloch as well. He thought that Bloch and I might form a symbiotic relationship – my experimental skills and Bloch's theoretical concepts.

The Physics Department basement – or for the theoretical students, the Physics attic – was to become an exciting environment, due largely to the backlog of ideas and the quality of the staff – F. Bloch, D. Webster and P. Kirkpatrick, followed soon by W. Hansen, and later E. Ginzton and L. Schiff. The graduate students were more mature than usual and technically skilled, especially in electronics. Conversely, some of the students had forgotten what they had learned of classical and quantum physics.

In the fall of 1945 the Stanford Physics Department was a mess. Much of the basement had been devoted to X-ray research and was reminiscent of a rabbit warren with hutches constructed of chicken-wire and lead, interconnected by a random network of forgotten wires. Local Indian relics were stored in one of the enclosures. One wag wondered if the bones were the remains of an unlucky graduate student. None of this disorder seemed to trouble Bloch. We evoked squatters rights, and I chose the old upstairs klystron laboratory. (I still wonder whether the correlation between creativity and surroundings is negative or positive.)

Ours was a cross-cultural relationship, which melded under the urgency of demonstrating Nuclear Induction ahead of omnipresent competitors. My culture, which was primarily experimental, was derived from Westinghouse Research where I worked under E. U. Condon, and was modified slightly at Ernest Lawrence's laboratory in Berkeley. Felix, as I addressed him, came from a theoretical background with the basic culture of Bern and Zurich.

Never did our Western familiarities and informalities appear to bother Felix or his wife, Lore. It was not until much later that I learned how long it takes the Swiss to be on a first-name basis. On the other hand, he retained a few residual European customs, which we accepted but with small internal grumblings. One of these was the extended midday dinner. We soon learned that this was not idle time and that conversations which were interrupted at the end of the morning would be continued late in the afternoon, sometimes past our dinner time. One notable example of this creative time was Bloch's invention of the spinning sample for high resolution NMR which came as he was stirring his midday tea.

Although there was a drive for progress, this was implicit and formal meetings were never held between Bill Hansen, Felix and myself. A division of labor was tacitly understood and the work went on independently. Bill Hansen worked on the crosscoil assembly, which was used for decoupling the strong transmitter field from the receiver); Felix modified and calibrated an ancient, small lecture electromagnet, with special transformer pole pieces and a field sweep; and I built the rf circuitry and visual display.

Progress during the first three months seemed slow. There was no infrastructure comparable to that at Westinghouse or Berkeley. However, the two skilled machinists, Bert and John, were most helpful in building complex parts or in teaching us how to bend metal or grind tool bits. Teaching responsibilities also interfered with research. Felix was sensitive to the material needs of graduate students, and so I was paid as a teaching assistant for a freshman physics course. Since I had been away from freshman physics so long, preparation time was longer than if I had been teaching, e.g., Laplace transforms, but Felix never complained.

Nevertheless, the apparatus did come together and, as with all systems, some back-tracking was required. Hansen's original kinematic design, which used an ingenious design of flexible wires, was abandoned in favor of his newly invented paddle for flux steering.

By the Christmas holidays everything seemed to be in order. Barbara and I decided to drive to Oregon to visit my parents, whom I hadn't seen during the war years. Felix expressed mild disappointment but not anger, at the delay. During my six-year stint at Stanford I cannot recall that he ever spoke harshly or was not considerate of his graduate students.

This is not to say that Felix avoided controversy or did not speak on issues that he felt important. I felt squeamish about his participation in the early political infighting which surfaced at selected weekly seminars. Later on he expressed strong opposition to the organization of Physics at Stanford, a problem which continued for a long time.

Felix was a superb theoretician with considerable physical insight. I quickly appreciated his skills without knowing of his previous work; e.g., Bloch walls in ferromagnetics. Prior to my coming to Stanford, he had fully developed his phenomenological equations of NMR on purely theoretical grounds. The equations were never modified, only interpreted. Felix preferred to think about Nuclear Induction in classical terms, which he justified as being the expectation values of the quantum mechanical model.

Felix was a master of the art of keeping important terms and dropping the insignificant ones. It is hard to know how, but he managed to impart this instinct to his students. Perhaps the absence of computers or even hand-cranked Monroe calculators brought out this ability.

He welcomed the interplay between his theory and the experiment. This was well exemplified by the first observations of the Nuclear Induction phenomenon. His original belief was that the static dipolar interaction between protons in our water sample would give a long thermal relaxation time T_1, and a very short transverse T_2.

This understanding dictated the details of the experiment; namely, that the radiofrequency (rf) fields should be very strong, the order of a few Gauss, and that the sample must be pre-polarized in a strong magnetic field for many hours. For the soaking, Felix assembled a small outrigger pole piece in the fringing field of the cyclotron magnet, where the sample was left for more than 24 hours.

Initial observations at the proper magnetic field were disappointing. Almost in desperation we raised the field well above Bloch's calibrated value and turned off the magnet power supply, which added a linear sweep on top of the small sine wave sweep. I happened to see a signal which entered from the right and disappeared to the left, much as a radar signal on an A-scope. Having once seen the signal we very quickly improved it, noting that relaxation times were moderate and that our original instrument design was excessive.

After our success, we left the dark, cold and clammy Cyclotron laboratory for the Blochs' pleasant house, where we shared our euphoria and celebrated Lore's birthday or near birthday with a glass of wine. Felix had expressed his hope to bring Lore a Nuclear Induction signal as a birthday present.

Felix immediately reviewed his equations and explained what we had seen. The transverse relaxation times had been much longer than expected and the longitudinal T_1 much less than expected, due to the rapid tumbling of the water molecules. Felix explained as a matter of fact that the physics of liquids was very difficult compared to solids or gases. The shape which we saw was explained by the Bloch equations and was labeled "rapid passage," as it was neither a dispersion curve, which we had expected, nor a simple absorption curve.

This explanation of the experimental data by the Bloch equations gave me great respect for them, and for their creator. (It is now too late, but I wish I had understood from Felix how he built such a simple, but valid model. Did he introduce the relaxation times as exponentials because of analytical simplicity or did he recognize the similarity to stochastic collisions in optical spectroscopy?)

Jargon was readily accepted by Felix. He invented the term "rabbit ears" for one particularly stubborn problem and was quite comfortable with terms borrowed from radar, an acceptance lacking by purists in the German language.

Felix was always generous with sharing

credit with coworkers and never wished to be an author as the Director of the laboratory. The first experimental paper on Nuclear Induction was authored by F. Bloch, W. Hansen, and M. Packard; however, the accompanying theoretical paper bore only his name. The first paper on chemical shifts was written by W. Proctor and F. Yu. The paper on chemical shifts in protons was by J. Arnold, S. Dharmatti and M. Packard. With Bloch, there never was a problem about the order of the authors, they were just listed alphabetically. Felix was equally generous in acknowledging me and other colleagues at talks which he gave many years after the original work.

We had many social discussions, particularly as our wives often sat together wih the Bloch children while we spent the evening in the laboratory. Felix was well read, broadly educated in a classical European manner, and was an excellent piano player which he shared with us occasionally.

Felix did not appear to be the athletic type, however he did from time to time ride his bicycle from their simple house on Emerson street in Palo Alto to the Physics Department, and did take occasional outings to the beach at Carmel. (We still have a lovely bowl that he and Lore brought to us.) Nevertheless, he described his earlier experiences in rock climbing in Switzerland. One of his very difficult climbs ended with a fall and a broken leg. While skiing together at Los Alamos, where we went to measure tritium, I was amazed to see him making a graceful Telemark, a turn which was new to me, a self taught skier from the Northwest.

Felix Bloch's major scientific interest and thrust was in measuring nuclear magnetic moments of the isotopes as a basis for understanding nuclear forces, which he considered to be the "central problem" of physics. Prior to the war he had measured the neutron moment in terms of the cyclotron resonance with Prof. Louis Alvarez at UC, Berkeley.

During a concert at Cambridge where he worked in Prof. Frederick Terman's laboratory, he considered the way to measure magnetic resonance in matter of normal density. This would make it possible to measure magnet moments in terms of the proton moment without the uncertainty of field calibration. At the time, a magnetic field could not be calibrated to better than 1/10th of a percent. His ideas were sharpened at Cambridge. The experimental techniques, concepts of signal-to-noise, and other practical matters were developed in conversations with Bill Hansen. In the tradition of most scientists, these matters were discussed only with collaborators and not potential competitors.

Even though much of the early gestation of Nuclear Induction occurred while he was at Harvard and very close to Edward Purcell, they had no interaction. Both groups performed the experiment independently and did not learn of the other's work until the arrival of the Physical Review.

Bloch was always very quick to grasp new concepts and to synthesize them into his own understanding. I had used a symmetry criteria at Westinghouse to measure precision frequencies of cavities and sought to apply this to measuring the magnet moments. Felix thought about this briefly and agreed that that was indeed a valid criteria. He understood and taught us the subtleties of precision and accuracy during many discussions at the blackboard.

In view of Bloch's ability to handle new concepts, it was somewhat surprizing to me that there was not instant recognition on the part of either Bloch or Purcell that the two groups were viewing the same phenomenon, even though the apparatus and the theoretical description were much different. Bloch had approached Nuclear Induction on the basis of classical phenomenological equations, while Purcell's group followed a spectroscopic model. The Stanford group initially observed dispersion, using a strong radiofrequency field, while the Harvard group's apparatus was phased to observe absorption in the presence of very weak fields.

It is surprising to me how long early impressions persist. Many years passed before most physicists and chemists accepted nature's view of the commonality of the two approaches. Nevertheless, I was recently asked by a foreign scholar whether Varian Associates' instruments used the Purcell or the Bloch technique.

Bloch coined the term Nuclear Induction, which gradually became displaced by NMR. I felt like a traitor when at Varian we adopted NMR. Confusion still exists in the nomenclature of electron magnetic resonance, chemists seem to like ESR while physicists tend to use the older EPR.

The very first NMR experiments were inexpensive and were, I believe, financed by the Physics Department. I purchased the electronics, which consisted of a Dumont oscilloscope (just like the one I had used at Westinghouse); a few vacuum tubes, some discreet components, a chassis or two, and panels. After the initial success, additional funds were provided by the Office of Naval Research (ONR) to extend the technique to the measurement of magnetic moments of other isotopes. Happily, the details of this support were invisible to me. I have no recollection of being asked to prepare proposals, budgets, or write reports; Felix either handled this himself or left it up to Anna Laura Berg, our administrative, secretarial, 4 o'clock coffeemaker staff.

This support of basic research by the ONR returned a huge yield to the DOD and to society. No one at that time had any inkling that Nuclear Induction would lead to magnetometers used to hunt submarines or become a major tool for scientists. NMR has become the most important spectroscopic tool for structural and organic chemists; biologists use NMR to study the conformation of DNA; and physicians like the superior diagnostic images of NMR.

Felix was proud but not ostentatious about his role in NMR. During the last few years of his life, Felix was fond of telling that his finger was the first in vivo specimen. I suspect that not even history will ever clarify whether it was the Professor's or the graduate student's finger which was first placed in the Nuclear Induction probe.

Early ONR support enabled the laboratory to become more consolidated, adding additional people and new experimental apparatus. Teaching Assistants became Research Assistants. Under Prof. Hansen's guidance, a Bitter Magnet was designed and built with the expectation that a nearly automatic machine could be designed for searching out and cataloging all the isotopes that had spins. This turned out to be impractical, so magnetic moments were measured isotope by isotope by successive graduate students.

Bloch's foremost interest was in the physics of NMR and phenomena which its use could explain. While he did not enthusiastically support the chemist's interest, neither did he belittle it or interfere with research in his laboratory on chemically oriented problems. This was a lesson he taught me well: a great researcher has humility about his views of what the future may bring.

Felix was a dedicated teacher with exceptional abilities. His lectures were well prepared but not sterile. Even though he reviewed his lecture notes carefully before class, he sometimes needed to share with us his thought processes as he reconstructed the logic or worked through a difficult chain of the mathematics. I felt that this was an important part of his pedagogical style, and was much better than one visiting professor who presented beautiful stress free lectures, which were so easy to follow that little was remembered.

The classroom work was important, but I feel that our almost daily discussions at the laboratory blackboard were the hallmark of a marvelous teacher. He instigated occasional tutorial sessions to insure our progress towards the Department Oral examination which was the prelude to the final goal. I remember well his asking me to explain internal reflection of light and kindly helped me by elucidating how the light must be attenuated exponentially outside the surface.

His writing was lucid, but sometimes had a slight Swiss accent. This created some anxiety for me and for my wife who was the typist, because after he would return my thesis with revisions, I in turn would change it back. Happily for all of us, including the onlooking graduate students, the series finally converged and my thesis was signed on the day of the birth of our first child.

Dan Alpert had been right. Felix and I formed a symbiotic relationship, which was beneficial to both of us and to society. But sadly it did not last over the years, since Professor Bloch chose estrangement with many of his students; an estrangement which was never reconciled. The reason is unfathomable to me, but seemed linked to his consulting agreement with Varian Associates Inc.

Nobel Laureate Felix Bloch was Switzerland's greatest native physicist. He brought to Leland Stanford Jr. University not only fame but a classical approach to physics and the love of teaching. It was my privilege to have studied under him, and it is my hope that his style of teaching and his approach to physics will be carried on.