



Phyphty Years of Phun and Physics in Kellogg

by Willy Phowler

The man who began it all — Charles Lauritsen — eyes the title of this condensed version of a recent talk given by his one-time graduate student, who is now the Institute Professor of Physics and who is also known as William A. Fowler.

This story is based on personal reminiscences. After all, I have been in Kellogg for 48 of its 50 years, so please forgive me if I appear on the scene from time to time. The hero of my story is Charles Christian Lauritsen (1892-1968), who was lured to Caltech by the siren call of Robert Andrews Millikan in 1926. Charlie worked with Millikan on the temperature independence of the cold-emission of electrons from metals and received his PhD in 1929.

Charlie, Ralph Bennett, and Benedict Cassen, with Richard Crane joining Charlie as a graduate student in 1931, then went on to develop a high-potential X-ray tube that could operate at the one million volts provided by the AC transformers in the old High Voltage Laboratory — now the Sloan Laboratory — at Caltech. The transformers had been installed by the Southern California Edison Company to test insulators, circuit breakers, and other equipment on long distance power lines such as the one from Hoover Dam to Los Angeles.

Springing from the successes in the High Voltage Lab, the Will Keith Kellogg Radiation Lab was built for the use of Charlie's high-potential tubes in cancer therapy and for study of the physics of high-energy X rays. One of the consequences of the latter activity was Charlie's demonstration with John Read that the energy dependence of the scattering of photons by the Compton effect was described accurately by the Klein-Nishina formula, which incorporated electron spin, rather than by the Dirac-Gordon formula, which did not.

Then came 1932 — the golden year of classical nuclear physics. Urey discovered deuterium, Chadwick discovered the neutron, Anderson discovered the positron, and Cockcroft and Walton, using an accelerated proton beam, succeeded in

disintegrating nuclei below the Coulomb barrier as predicted by George Gamow in 1928. With one million volts of alternating potential available, Charlie and Dick Crane began — and completed before the end of 1932 — an ion beam tube in the High Voltage Lab. They used two large porcelain insulator bushings left over from the power-line-testing days.

Another of Charlie's great inventions — the Lauritsen electroscope — consisted of a fine quartz-fiber about five millimeters long with a one millimeter cross hair on the end that could be viewed against a marked scale with a low-power eyepiece. We all carried a fountain-pen version for measuring personal exposure to radiation. In its most frequently used form, it was mounted inside a thin aluminum cylindrical chamber two inches in diameter and three inches long, and it was charged electrostatically via friction by turning a small knob on the side a few times.

The electroscopes were calibrated using standard gamma-ray sources, even then available from the National Bureau of Standards. When measurements started with the million-volt ion tube, these electroscopes were all that was available for detection in the laboratory, and they were superb for running excitation curves against beam energy. With a thin lead wall either inside or outside the aluminum, they were excellent detectors of the Compton electrons and pairs produced by the several-million-volt gamma rays produced in nuclear disintegrations. Lined with a thin layer of paraffin, they were an ideal neutron detector. Crane, Lauritsen, and A. Soltan (a Rockefeller Fellow from Warsaw) put helium into their ion source, bombarded a beryllium target, and were the first to produce neutrons in an accelerator using the same reaction that Chadwick had used. Thanks to the fact that Soltan was a French-speaking (and writing) Polish count, they got quick publication for their first paper in *Comptes Rendus*, thus managing to beat their competitors, Ernie Lawrence and company at Berkeley and Tuve and Hafstad at, of all places, the Department of Terrestrial Magnetism in Washington.

I joined the team as Charlie Lauritsen's graduate student in 1933 at the same time as Lewis

Delsasso. To all of us he was “Del,” and it is my recollection that he was the one who pinned “Willy” on me. He was an “older man,” as graduate students go, and was already married with a family of three. Charlie put the two of us to work building a cloud chamber, actuated by a syphon bellows, and supplied with a uniform magnetic field by a pair of Helmholtz coils. We both did our theses with this somewhat Rube Goldberg-like device that had cam-operated switches for turning the illuminating light on and off, expanding the chamber, opening and closing the camera shutter, and recompressing the chamber — all on a 30-second cycle. We eventually took 100,000 pictures with that darn thing and reprojected and studied every one of them. The early result was a series of about ten papers by Crane, Delsasso, Fowler, and Lauritsen that cluttered up the *Physical Review* from 1933 to 1935.

Del was an exquisite technician and superb experimentalist; his contributions at Aberdeen Proving Grounds and White Sands during and after World War II capped an all-too-short career. But he had trouble with the theoretical courses all the graduate students had to pass in those days. He had already flunked Fritz Zwicky’s course in Analytical Mechanics several times, but he took the course once again when I took it for the first time. Fritz believed in the Socratic method of teaching. He lectured infrequently but most often sat in the back of the class and sent one of us to the board to answer questions that literally came out of the blue. We were allowed to take the textbook with us, and we could search through it for help. The textbook was Webster’s *Dynamics*, and Del’s copy was well worn and tattered. When Del was asked in turn to go to the board, he took this old book with him, and would try to leaf through it with pages falling on the floor. We all cringed. Del never answered a question all term, but, lo and behold, at the end of the term he passed. Those of us who knew of his great ability in the lab were pleased and decided to call on Zwicky to find out what principles of teaching we could glean for our future use. We put the question to Fritz. He glared at us, waved his arms, and replied, “Vell, Gottdammit — I had to pass him; his book wore out.”

My thesis, published with Del and Charlie, was an experimental one but concluded with a fairly significant theoretical conclusion. There is a story behind that. We measured the curvature in the magnetic field and thus the momentum and energy of the positrons from ^{11}C , ^{13}N , ^{15}O , and ^{17}F . Franz Kurie was doing the same thing at Berkeley, and he developed the famous Kurie plots. We noticed that the energy end-points of these

radioactive nuclei progressively increased. When we called this to the attention of Robert Oppenheimer and Robert Serber, they pointed out that this progression was the effect of Coulomb energy increase along the series and that our results showed the existence of mirror nuclei and the charge symmetry or equality of proton-proton and neutron-neutron forces. That went into my thesis, thanks to the two Roberts.

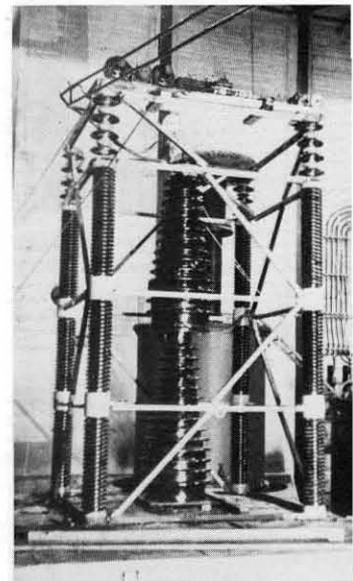
Robert Oppenheimer — “Oppy” — was the great man in theoretical nuclear physics in those days. He had a joint appointment at Berkeley and Caltech and came to Pasadena every year for the spring quarter. No other theorist at Caltech except Richard Tolman gave a hoot about what we were doing. Richard was a benign observer of what went on in Charlie’s lab, and he and Charlie and Oppy were close friends. Many times they sat after lunch in some old weatherbeaten wicker chairs in the sun outside the High Voltage Lab discussing the great happenings of the day in physics.

Charlie had three other graduate students in those early days — Wilson Brubaker, Walter Jordan, and Louis Ridenour. Before the start of World War II he graduated nine more — Kamal Djanab, William Stephens, Thomas Lauritsen, William McLean, Frank Oppenheimer, Robert Becker, John Streib, Charles Sheppard, and Everett Tomlinson.

Charlie did more than guide our graduate careers. He taught us how to use a lathe, how to bring the mercury back down in the stem of a Macleod gauge by gently tapping without breaking it, how to outgas the vacuum tube after repairing a leak by painting it with shellac, and a million and one other practical things in the nuclear lab of those days. But he taught me much more than that. When I complained that the hours were too long, day after day, he said “Stop complaining; what if you had to work for a living?” When I thought some of the problems we worked on were just too hard, he said, “If it were easy, someone else would already have done it.”

Actually doing experiments in nuclear physics in those halcyon days in the mid-thirties was much easier than it is now. We didn’t have to do much planning or thinking about the future. When one experiment was done, we picked a new target, decided what to bombard it with, measured the activity produced as a function of peak bombarding energy with the electroscope, or ran the cloud chamber for a few days or a few weeks, measured the tracks, and wrote a paper. It all changed after the war.

Charlie helped us learn about X-ray and nuclear physics as well as about plumbing. He held

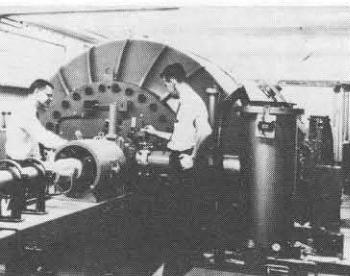


An ion beam tube (above) was built in the High Voltage Lab by C. C. Lauritsen and Dick Crane in 1932. What is now called the “experimental area” was located in the concrete block house beneath the tube. Count Soltan (below) is in this area peering at a Lauritsen electroscope next to the target, which is behind lead shielding.





Tommy Lauritsen teaching a class sometime in the 1960s.



Tommy Lauritsen with Bob Christy and Kellogg's first tandem accelerator.

a weekly seminar every Friday night. Dick Crane remembers the graduate students plowing through Siegbahn's book on X-ray spectroscopy. When I came in 1933, we started on Rutherford, Chadwick, and Ellis, and each graduate student led the weekly discussion, chapter by chapter. This was interspersed frequently by journal club reviews of exciting new papers as they were published. But there was time left over for relaxation. After the seminars, we repaired to Charlie's home, where he served drinks and Sigrid, his wife and a practicing doctor of radiology in her own right, served refreshments. Young Tommy Lauritsen played the piano, Charlie played the fiddle, and we sang sentimental songs.

I'll never forget when I first came to realize what it really meant to work in Kellogg, which was named for the Corn Flakes King. Millikan had inaugurated dinners for The Associates at the Athenaeum, and faculty and even postdoctoral fellows were invited. On one occasion I was placed next to a little old lady from Pasadena. (Little old ladies in tennis shoes was the usual description, but Millikan invited only those whose tennis shoes were studded with diamonds.) In good time she turned to me and asked, "Young man, what do you do at Caltech?" "Madam, we bombard lithium with protons and produce alpha-particles in nuclear disintegrations." The look on her face told me that she may have heard of lithium, had probably never heard of protons, and certainly had never heard of alpha-particles. I tried again with, "Madam, what we are doing is popularly known as atom busting." "And where do you do this, young man?" "In the Kellogg Laboratory," I replied. "Oh," she said, "now I know what you do — you puff rice."

Tommy Lauritsen became a graduate student under his father in Kellogg in 1936 and received his PhD in 1939. We came to realize in 1936 that the days of an alternating potential tube were numbered, so we had to do some planning and building for the future. Thus Tommy's doctoral work was mainly the construction in collaboration with his father and me of a 1.5 MV electrostatic accelerator housed in a pressure vessel. The new accelerator was put into use early in 1938. It operated continuously, except during World War II, until 1980.

With this new accelerator we were able to produce high-resolution, precise excitation curves for a number of targets primarily bombarded by protons. Charlie and I were the only faculty members before the war, joined later by Tommy. However, when I say "we" from then to the start of World War II, I refer to the graduate students and visiting associates, the earliest being Hans Staub and

Tom Bonner. That was it. Before the war, fewer than 20 people were involved in research in nuclear physics in Kellogg. Kellogg was one of the first team operations in physics, but the team was small, especially compared to Berkeley.

Early in 1939 Hans Bethe described the operation of the CN-cycle in heavier main-sequence stars in a letter and a full paper in the *Physical Review*. For those of us in Kellogg this was a dramatic event in our lives. What we were doing in the lab had something to do with the stars. There we were in one of the great astronomical centers of the world with the 100" telescope in operation on Mt. Wilson and the 200" Palomar mirror already under way. There was no way to go but up — up to the stars. There was a long delay, however. World War II came along. By August 1941 we were developing and producing rocket ordnance, primarily for the U.S. Navy, and creating an organization of several thousand people, most of whom were transferred at the end of the war to the Naval Ordnance Test Station (now the Naval Weapons Center) at China Lake.

The war finally came to a nuclear end in August of 1945. Charlie spent some time in Washington assisting Admiral Robert Conrad and others in establishing the Office of Research and Inventions, which soon became the Office of Naval Research. He decided under some counter-pressure not to involve Kellogg or Caltech in further work in rocket ordnance, and by mid-1946 we had transferred all war work to NOTS, China Lake. We were virtually alone in Kellogg until a swarm of new graduate students returned in the fall of 1946.

Charlie made some deliberate decisions. First, Kellogg was to remain part of the Division of Physics, Mathematics and Astronomy and not become a separate lab with a director and any administrative bureaucracy. All permanent Kellogg staff were to be faculty members with tenure.

Second, we resolved to stay in low-energy, light-element classical nuclear physics using electrostatic accelerators equipped with high-resolution electrostatic and magnetic analyzers, with double focusing magnetic spectrometers, and provided with electronic detectors, photo-multipliers, and all the other marvelous gadgets that came out of war research. Eventually an 0.7 MV high-current accelerator and a 3 MV accelerator were built in-house, and a 6.5 MV EN-tandem accelerator was provided by the ONR. We resolved to concentrate on those nuclear reactions thought to occur in stars, not only in the CN-cycle but also in the proton-proton chain of Bethe and Critchfield. In this we were greatly encouraged by Ira Bowen, who had been our colleague

before the war, directed all our high-speed rocket photography during the war, and became director of Mt. Wilson soon after the war.

Third, with Oppy's departure to the Institute of Advanced Study, we realized that we urgently needed a theoretical nuclear physicist in Kellogg. Oppy recommended Robert Christy, and Bob came to Kellogg in 1946. He brought a keen appreciation of the relation between experimental and theoretical physics and contributed in a most significant way to the development of postwar Kellogg.

Finally, we needed some money, and so on June 19, 1946, we submitted a three-page proposal to ORI (even before it became ONR) for \$94 thousand to fund research in nuclear physics and astrophysics for one year for four faculty members, one senior research fellow, eight to ten graduate students (we hoped), and one engineering draftsman, one electronic technician, one instrument maker, one machinist, and one secretary. Small in some ways, but lots of people to be supported on 94 kilobucks.

That proposal concluded with an interesting commentary on the perceived problems of those times. The final paragraph was entitled "Publication of Results and Nature of Research." Here it is in full:

It is essential that all results of these studies be in the "unclassified" category and that publication in scientific journals and announcements to scientific societies be permitted at all times. It is also essential that it be clearly understood that these studies are primarily fundamental in nature and that the possibility of applications is of secondary importance. Finally a clause must be inserted in any contractual agreement guaranteeing the Institute the right to purchase from the Government all equipment provided under the contract.

Kellogg entered a new era. The faculty grew to include Ward Whaling, Charles Barnes, Ralph Kavanagh, Tom Tombrello, Steve Koonin, Peter Haff, and Robert McKeown. They and their students and postdoctoral fellows deserve all the cre-

dit for Kellogg today. Closely associated with Kellogg have been Jim Mayer and Jim Mercereau, and still closely associated are Don Burnett, Kip Thorne, and Jerry Wasserburg, all of Caltech. It has been hard to match their accomplishments.

There is no room to discuss what has transpired since 1946, but it was all given in a wonderful talk presented by Fay Ajzenberg-Selove at the Kellogg 50th birthday celebration. Her talk and one given by Dick Crane are available in the Caltech Archives, and I am indebted to both of them for some of the things I have been able to put down here. Let me give you, for example, a few facts from Fay's talk about Kellogg activities. She pointed out that prior to 1942, 16 PhDs graduated from Kellogg, and since 1947, 147 individuals have received Kellogg PhDs from Caltech. Before 1950 a total of 8 postdoctoral fellows or visiting faculty were given appointments at Kellogg for varying lengths of time. Since 1950, almost 400 such appointments have been made.

Fay checked up on our publication record since 1946 too and came up with a total of slightly more than 2000 papers, including all publications whether published in scientific journals, in books, in conference proceedings, or in popular magazines. And she included publications written by members of the extended Kellogg groups while on leave at other institutions and the publications written by visitors, research fellows, and visiting associates while they were at Caltech.

That's just about the end of the line. We continued to work hard, sometimes under difficult and complicated circumstances, but we continued to have fun trying and sometimes succeeding. We have high hopes for the future in using our newly dedicated high-current, high-resolution 3-MV tandem accelerator. It was lowered into place in September and, because of its paint job, was promptly christened the Yellow Submarine.

Good laboratory facilities lead to good physics. Charlie Lauritsen believed that all his life. The Kellogg gang has grown considerably since Charlie's time; it still has fun doing good physics. □



A recent picture of most of the Kellogg gang.