KENT M. TERWILLIGER; GRADUATE SCHOOL AT BERKELEY AND EARLY YEARS AT MICHIGAN, 1949 - 1959

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It is a great privilege to be the first to honor the memory of a very dear colleague, Kent Terwilliger, at this symposium. From the time we entered graduate school at Berkeley and throughout the 1950's, Kent and I worked very closely together, first as graduate students, and subsequently as junior faculty at Michigan. It is this decade that I shall recall, starting with our graduate studies at Berkeley, our thesis research under Carl Helmholz, our coming to Michigan, our early research activities at Michigan with the racetrack electron synchrotron and 40-inch cyclotron, the beginnings of MURA with study groups at Brookhaven, Madison, and Ann Arbor, and highlights of our Michigan Model FFAG accelerator. It may be noted that our work was so closely collaborative over this period that what follows is in a sense autobiographical as well.

Graduate Studies at Berkeley

Kent and I arrived at Berkeley to enter the graduate program in Physics in September 1949. Kent had had a superb undergraduate education at Cal Tech; his electromagnetism course, for example, had been taught by W. R. Smythe from a text normally regarded as a pretty tough graduate text. (I had stayed at Northwestern for a year beyond my B.S. to earn a M.S., but my academic background was not significantly ahead of Kent's.) Kent and I were in most of our classes together and were in the same office of teaching assistants (we were both T.A.'s in the elementary labs). We both lived in International House, right on the edge of campus, and often studied together evenings. Other I-House physics graduate students at that time with whom we became good friends included Bill Jarmie (now at Los Alamos), Byron Youtz (now at Evergreen College, Washington), Bob Eisberg (Santa Barbara), Gerry Igo (UCLA), Gerry Fischer (SLAC), and Bob Kenney (LBL) among others.

Our courses included quantum mechanics from Bob Serber, E&M from Panofsky, Nuclear Physics from Ed McMillan, and Statistical Mechanics from Geoffrey Chew. In the latter case, we learned that a thermodynamics course was a prerequisite, but both Kent and I thought we had had a pretty good thermodynamics background as undergrads. We were told to see Owen Chamberlain, who was offering the required thermo. We expected a big hassle to get his permission to skip his course; to our pleasant surprise, he was very supportive, and urged us to go straight into statistical mechanics. Both Chamberlain and Chew had just arrived on the faculty from Chicago; Chew announced that this was the first course he had ever taught. It was an excellent course, and we worked very hard at it (the text was Tolman's book).

Student days, in spite of lots of very hard work, are very mellow times in one's life. We enjoyed picnics in Marin County at Point Reyes and over-night camping trips to the Lick Observatory, to Yosemite and to Lake Pillsbury; Sunday evening dinners in San Francisco, and good bull sessions with friends. Some of these good times are recorded in the accompanying photographs. During our second year, Kent began seeing a lot of another I-House grad student, a chemist named Doris Heisig. That summer Kent and Doris drove Ruth and me (I had married a year earlier and brought Ruth to Berkeley) to the State Fair in Sacremento; Doris kept gesticulating with her left hand in the oddest way, until we at last noticed the engagement ring.

Thesis Research on Photonuclear Reactions

Toward the end of our first year we thought that we should consider a thesis advisor and thesis research. For one thing, we were weary of teaching the elementary labs, and many of the faculty had connections with the Radiation Laboratory, so that their students had research appointments there. This also paid a little better than the T.A. We discussed in I-House who might be a good thesis advisor to work for. I recall that we eliminated a couple of faculty because their students spent most of their time working with nuclear emulsions, staring through microscopes. This didn't sound too interesting; Kent and I both aspired to more active experimental work. We finally decided to approach Carl Helmholz, who had students working with the newly-completed 330 MeV electron synchrotron. Carl was very receptive to our interests; he gave us some reprints to study and suggested that not much was known about how gamma rays interacted with nuclei above about 20 MeV, although above 150 MeV or so the recently discovered π-mesons were photo-produced, according to recent work.

That summer and fall we started work at the Rad Lab, after getting our Q (security) clearances, etc. What was known at that time, chiefly from Kerst's work at Illinois, was that there was a "giant resonance" of photon absorbtion in nuclei with a peak between about 18 MeV (lighter nuclei) and 12 MeV (heavier nuclei). Then Kerst had made measurements at the new 320 MeV Betatron at Illinois which suggested that there might be an increase in neutron yield and hence photon absorbtion above the giant resonance. It would be interesting to map out the photon absorbtion between the giant resonance and the highest energy of the synchrotron. Was there any absorption between the giant resonance and the meson threshold? How did the cross section rise above the meson threshold? How did the cross section vary with atomic mass number? The experiment we did had to solve two problems: to unfold the continuous Bremsstrahlung

Kent Terwilliger: Student Days at Berkeley

Picnic at Mt. Hamilton, 1950. (Clockwise Around Table) Bill Jarmie, Libby, Byron Youtz, Doris Heisig, Kent Terwilliger, Mary Ishi, Ruth and Larry Jones.

Badger Pass, Yosemite, 1952. (Left to Right) Doris and Kent Terwilliger, Lila and Bob Eisberg, Ruth and Larry Jones spectrum in order to get an energy dependence, and to find a suitable monitor of the gamma absorption. The first we solved by operating the synchrotron at a series of discrete electron energies, and using the calculated gamma spectra at each energy. Using the normalized flux at each energy, the Bremsstrahlung curves could be subtracted one from the other and the "difference" gammas (mostly from the energy band between the two electron energies) were ascribed as responsible for any differential effects. This was called the "Photon Difference Method". We monitored the absolute flux at about 16 MeV by the yield of the ^{63}Cu (γ,n) reaction, which resulted in Cu^{62}, a positron emitter with a 10 minute half-life which was easily counted with a Gieger counter. This reaction was dominated by the measured Giant Resonance, with no evidence of appreciable yield from higher-energy gammas. In order to determine the absorption of gammas by the target nuclei, we counted the photo-neutrons produced using a boron trifluoride counter in paraffin. This counter had a reasonably flat energy response to neutrons up to about 5 MeV. Nuclear statistical models gave the average number of neutrons produced vs energy, so that our neutron counts and photon-difference gamma fluxes could be interpreted in terms of a gamma absorption cross section vs gamma ray energy, after all the appropriate normalizations.

Setting up the experiment was a physical chore. Not only were lots of lead bricks needed for gamma shielding, but we had to stack up hundreds of pounds of paraffin-filled boxes for neutron shielding. Then after a run taking neutron counts in the synchrotron beam, we had to turn off the machine, dash in to get our copper foil, dash out with it, and then count the copper radioactivity for about 10 minutes. As it turned out, it was not really practical to run the Berkeley synchrotron below 80 MeV; the beam wasn't good, and it seemed to every one a waste, given its capability at four-times the energy. But the University of California Hospital and Medical School in San Francisco had just received a 70 MeV electron synchrotron from G.E. for radiation therapy, and a student of Don Kerst at Illinois, Gail Adams, had come to operate it. As it wasn't ready for use in therapy yet, we negotiated to use it for this physics study. So we ran the photonuclear yield data from 13.5 to 70 MeV in San Francisco and the 80 to 320 MeV data at the Berkeley Radiation Laboratory. Although we both worked on the experiments at both machines, we divided the data from these two machines into our two theses; Kent wrote up the lower-energy data and I the higher-energy work. The decision as who would do which was quite random; I don't recall how we finally did decide.

Of course all of our data was from scaler readings hand-written into data books. And the calculations (the photon difference method required point-by-point subtraction of these Bremsstrahlung curves) were all done on Frieden or Marchant electro-mechanical calculating machines. The data we finally got were very well received. We gave our first papers at a December 1950 meeting of the

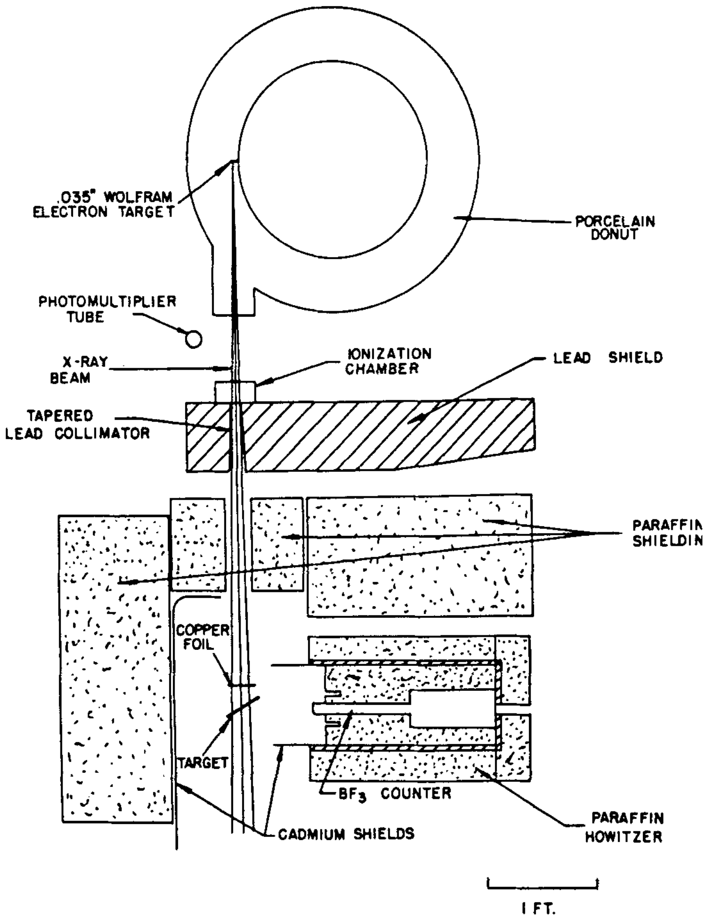
American Physical Society in Los Angeles (and stayed to watch Michigan beat California in a Rose Bowl game). In April, 1952 we went to the Washington Physical Society Meeting where we presented a pair of 10-minute papers on our data on photon absorbtion from 13.5 to 320 MeV on about 10 different nuclei.

The abstract of Kent's thesis is included here as an appendix, as are the references to the Physical Review papers reporting his and my theses. Our theses appeared as UCRL (University of California Radiation Laboratory) reports; Kent's was UCRL 1917. Earlier, preliminary data were presented in UCRL 1083. Graduate students should take note that Kent left Berkeley with his Ph.D. just three years after he arrived with his B.S. from Cal Tech. His thesis is a very literate, scholarly, and complete piece of work, although compact; the total thesis is only 68 pages, of which the text is just 39 pages. The layout of his Thesis experiment is reproduced here from Figure 1 of this Thesis.

Being in the Berkeley physics department and at the Radiation Lab was a very rich experience in those days. The weekly Research Progress Meetings at the Lab nicely supplemented the weekly physics department colloquia in keeping us abreast of current physics. On campus there was also a regular weekly evening journal club, well attended by the faculty and many graduate students. I recall one such evening when the chairman, R.T.Birge, noted to the group (we were meeting in a modest-sized class room) that there were several Nobel Laureates present. Indeed, Lawrence, Giauque, McMillan, Seaborg, and some other big names from physics and chemistry were there; I believe that the speaker was Willis Lamb, talking about the Lamb Shift. And this was of course before Lamb, Alvarez, Chamberlain, or Segre had their Nobel prizes.

At the Lab each graduate student was assigned a task beyond his thesis research to pay his way. We became regular members of the synchrotron operating crew, running operating shifts, and learning the complete care and feeding of this "big" accelerator. This was a wonderful introduction to the intimate details of accelerator engineering and particle beam behavior. Maintaining a good beam meant continuously riding a battery of controls; injection timing, injection high voltage, rf tune, etc. plus monitoring other parameters. Operation was from a single console and was a one-man job. Among the experiments for which we ran the machine was the first definitive neutral pion photoproduction experiment of Steinberger, Stellar (another graduate student) and Panofsky. Carl Helmholz was a very patient and sensitive advisor, although he did not spend a great deal of time with us. Often we would have a chat with him following the weekly scheduling meetings Friday afternoon at the synchrotron, from which he would retreat backing out the door. He pretty much gave us free rein to set our own course in our research. He became very popular with graduate students, and we pursuaded him to meet an evening a week with us (a group of his students which grew to about a dozen by the time we completed our theses) for general

Schematic plan view of the experimental apparatus for Kent Terwilliger's Ph.D. thesis experiment on photoneutron production from the x-ray beam of the 70 MeV Synchrotron at the University of California Medical Center; 1951–1952.



rapping about physics; what was current, what was interesting, etc.

The old Berkeley 330 MeV Electron Synchrotron is now in the Smithsonian Museum in Washington (the entire magnet core is not there; but the outer laminations and all the vacuum donut, rf, and injector are there).

Coming to Michigan

In the spring of 1952 Kent and I started to think about jobs. The Livermore Laboratory was just starting up and was recruiting staff, and Teller had arrived in Berkeley. However, we both felt that a university faculty appointment would be the most desirable. We had heard that Columbia was paying $3600 per (academic) year; not great, but it looked OK after living on the $110 per month of the teaching assistantship (a little more at the Rad Lab). We wrote to several places, and lined up an itinerary of places to visit on our Washington trip. We visited the GE labs in Schenectady, Westinghouse in Pittsburgh, the GE lab in Cincinnati (where they were working on nuclear powered aircraft), and a couple of universities. At the APS meeting we were talking to the department chairman from the University of Florida when Helmholz came rushing up and told us he wanted to introduce us to Dick Crane from Michigan; Michigan was looking for new faculty. Dick persuaded us to juggle our travel plans to visit Ann Arbor on the way back to California. In Ann Arbor we visited with several faculty. I remember in particular George Uhlenbeck with his long fingers and cigar. Dick invited us out to his home on Ferdon Street, and we were both favorably impressed by the faculty and the campus.

Back in Berkeley we each received several job offers; some places offered one of us a job but not the other, and some, including Michigan, had two positions and would hire us both. We had found working together very productive, and really prefered to stay together if possible. Bill Nierenberg had recently come to Berkeley after having spent a couple of years on the Michigan faculty, so we went to talk to him about Michigan. I vividly recall visiting him in his lab; he was diddling with some molecular beam apparatus as he talked in his typical booming voice: "Why, if you have an opportunity to go to Michigan, are you considering any place else!" In his bombastic way, he gave us to understand that we would be absolute fools not to accept the Michigan offer. And so we did. In August, 1952 we drove cross-country to Ann Arbor to start our Michigan careers as instructors at a salary of $4000 per (academic) year.

Michigan, 1952-1955

At Michigan, Crane's synchrotron was running at 70 MeV, awaiting power equipment to permit it to go to higher energy. Bob Pidd was the younger faculty member involved. It was in the second basement of Randall in a two-story bay (extending through what is now the instrument shop). Next to it, in the 1st basement, was the 40-inch cyclotron, which Bill Parkinson and Paul Hough were running. Pidd's and Parkinson's idea was that one of us would work on the cyclotron and the other on the synchrotron. Kent and I, however, chose to continue to work collaboratively on both. On the synchrotron, we worked with Rod Hines, a student of Dick Crane's, on the electro-disintegration of copper, using an internal target. This hadn't been studied before, and nicely complemented our photon work. This research as well as the program of elastic electron scattering made good use of the accessible straight sections in the machine, at that time a unique feature of the Michigan synchrotron. Besides, there was not a very good external beam area. In fact, the x-ray beam came straight toward the control room (although there was, of course, an appropriate beam stop). The machine took pretty much the whole room, and didn't leave much room for experiments.

Hough and Parkinson planned to develop a high-resolution beam at the cyclotron in order to do precision nuclear spectroscopy. the idea was to make an extracted beam spectrometer using an old "C"-magnet, and then to use a spare quadrant of synchrotron magnets as a reaction products analyzer. Kent and I were to learn about beam optics and to design pole tips for the C-magnet to bring the beam to a focus and to have satisfactory dispersion. This we did, using an $n=1/2$ double-focusing pole design,and the system worked pretty well. At the synchrotron, we also got involved with the machine side of things, and it was there that Kent conceived and developed the "rf knockout" method of determining the betatron tune of the machine. The idea was that a transverse rf field that gave a kick to the beam in phase with the beat frequency between the orbital and betatron frequencies of the beam would build up oscillation amplitudes until the beam blew up. It could be applied horizontally or vertically; the method worked very well, and was applied subsequently throughout our MURA activities.

For our first ten years at Michigan Kent and I shared an office, 1075 Randall (at the north end of the first floor hallway) and our telephones were two extensions of one line. We taught mostly elementary recitation sections the first few years, plus elementary labs our first year. A memorable aspect of life in Michigan then was the weekly Thursday noon lunch at the Union. There we usually had a large round table, and Uhlenbeck, Otto LaPorte, Ken Case, Don Glaser, as well as Pidd, Hough, and Kent and I were usually there. The conversations were very stimulating, and we learned considerable physics from these informal gatherings.

Kent and Doris had a second floor apartment in a house on 8th Street on Ann Arbor's west side during the first years in Ann Arbor. We became friends with the Krimms and the Cases, who had also only recently joined the faculty and who were also starting their families. We enjoyed picnics together at Delhi Park on the Huron River and elsewhere, and Ruth and I also often got together with Kent and Doris for bridge. As our families grew, we decided to look into leaving our small apartments and buying homes; after some looking we both decided that we might be better off building. The Dennisons and The Cranes had both recently built homes designed by Bob Metcalf, a young architect at the University, and we were impressed by the intelligent, contemporary design of their houses. We approached Metcalf, and he was interested in the challenge of designing low-cost houses (all that was realistic on our instructor salaries). In about 1955 we both built houses on Ann Arbor's North Side, about a block apart, and the Krimms built a new home nearby. Our children went to nearby Northside School and were good friends. I have included two photos of Kent and Doris from those early Michigan years.

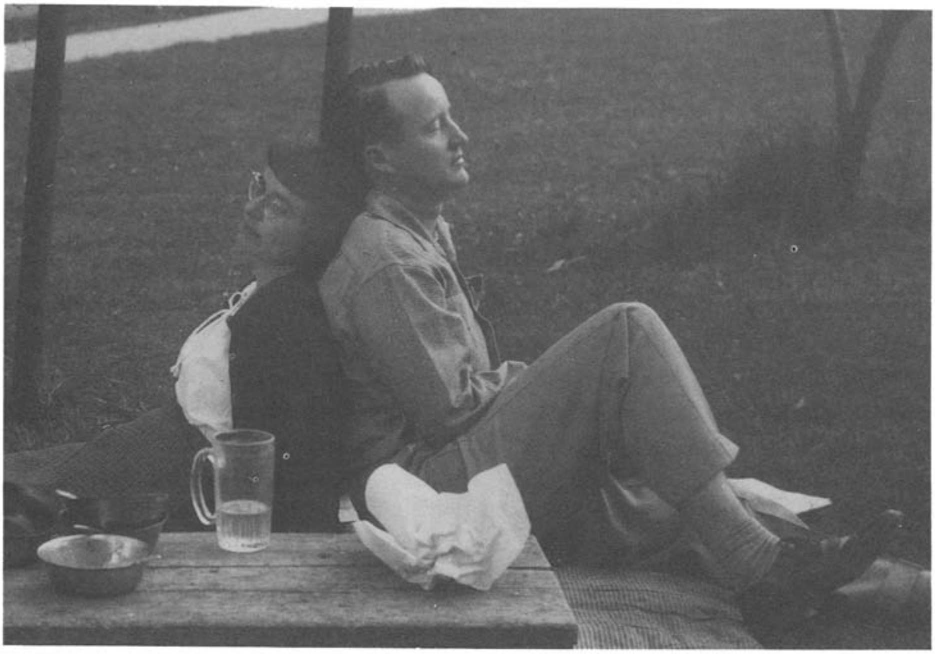
MURA AND THE ADVENT OF STRONG FOCUSING

At the end of 1952 we learned that the idea of strong focusing (or alternating gradient) had been developed at Brookhaven, and we were eager to learn about it. Crane and Pidd were quite excited, and with Kent and I, there were several lively discussions on the subject. It turned out that Dennison had worked out the solutions to the Hill Equation for the Michigan Synchrotron which had straight sections, and the same mathematics was readily applicable to calculating alternating gradient structures. Dennison had also discussed the Matthieu Equation in classical mechanics, and had used as an illustration the motion of an inverted rigid pendulum: if a pendulum is attached to a point which oscillates vetically, and the pendulum is up-side-down (mass at the top), the pendulum can execute slow oscillations about its vertical equilibrium position, i.e. this vertical position which is unstable without the vertical oscillations becomes a stable position in the presence of the vertical oscillations. All of this was exactly what was involved in strong focusing.

During the spring of 1953, Kerst, Crane and others had been discussing developing something analogous to Brookhaven in the Midwest. This jelled in the summer of 1953 when Kerst led a group of us younger physicists to Brookhaven for a three-week study session. Crane suggested that Kent and I represent Michigan there. Kent and Doris drove me there (as Ruth was expecting our second child soon and didn't make the trip). We visited Niagara Falls and the Cornell Synchrotron on the way East. At Brookhaven Ken Green, Hartland Snyder, Ernest Courant, Stan Livingston, Milt White, and others gave us wonderful tutorials. Ken Green gave us an exhaustive education on the Cosmotron, which had only recently been completed and brought into operation by him. We all stayed at an aging lodge in Bellport, so our discussions continued late into the evenings.

We were only back at Michigan a week or two before convening again in Madison on the University campus to discuss among ourselves what we had learned and to see what innovations we might come up with. Bob Hofstadter

Kent Terwilliger: Early Years in Ann Arbor



Doris and Kent backyard picnic in Ann Arbor, ca 1953.

Kent and Doris at Niagara Falls enroute to Brookhaven, 1953.

and Ernie Courant were among the non-Midwesterners who visited the group in Madison at that time.

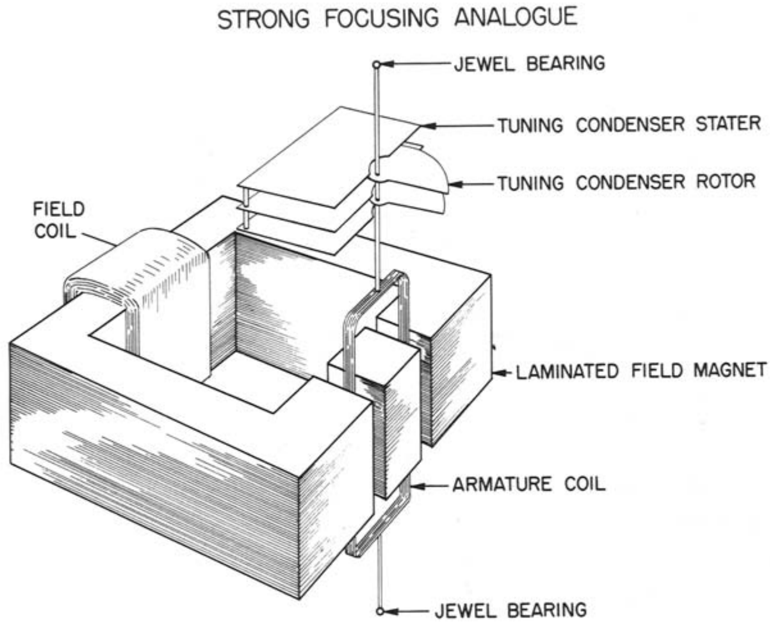
Back in Ann Arbor, Kent and I put together an electrical-mechanical analogue device to allow us to study (play with) strong focusing, perhaps inspired by Dennison's inverted pendulum. We built a gadget which was basically a galvanometer, with a current through a field coil which could be driven by a square wave source (using two Eimac 304TH tubes) and an armature coil driven by a circuit such that its current was proportional to the angular displacement. With the field current one way, the armature was stable and experienced a restoring torque proportional to its angular displacement. And of course with the current the other way it was unstable about the same position. By varying the current, the frequency, and the relative times and amplitudes of the two opposite currents (focusing and defocusing), the parameter space of the Hill Equation solutions could be mapped out. The coil angle vs. time was presented on an oscilloscope which we photographed. This proved to be a nice educational toy. Of course, for large angles, the restoring force was not linear, so we were able to look at some non-linear effects. We readily found "lock-in" conditions, which corresponded to what came to be called "Craneac Motion" (as Dick Crane had conceived of it before we had seen it), whereby the oscillations were stable about a resonance at a large amplitude, well into the non-linear regime. A sketch and a block diagram of this analogue are reporduced here.

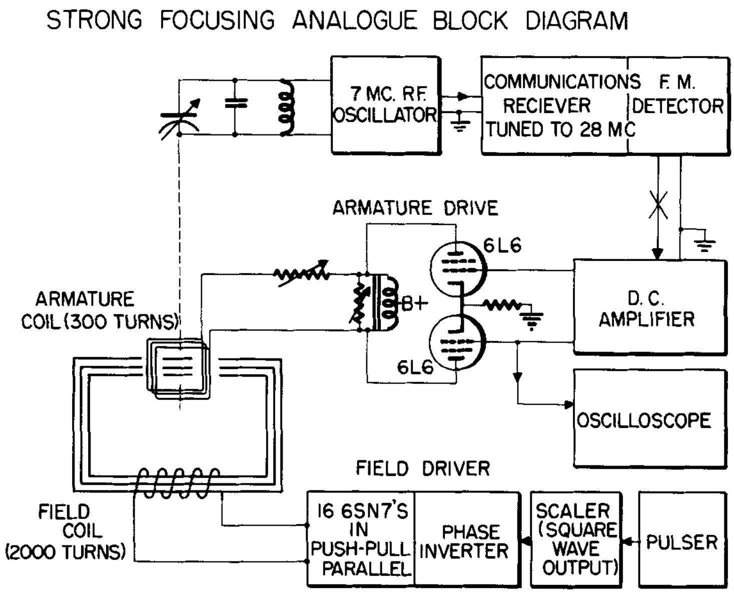
The senior physicists and officers of our midwestern schools had formed a consortium, the Midwestern Universities Research Association (MURA). During the academic years 1953-54, 54-55, and 55-56, we held MURA meetings every month or two, almost always on weekends (late Friday through Sunday to avoid conflicts with teaching obligations), rotating among the different MURA universities as sites. These meetings served to keep us in communication and to exchange and develop new concepts. Kerst was the organizer and director of these meetings. He was always stimulating and encouraging. Working closely with Kerst was in fact like another doctoral thesis experience; as a mentor he had patience and tolerance of our sophomoric concepts, and he was a superb teacher.

In the summer of 1954 we again met at Madison, this time for a longer period. It was towards the end of this workshop that Keith Symon conceived of the FFAG (Fixed Field, Alternating Gradient) principle, although there was little time to pursue it before we broke up. The following summer (1955) we met in Ann Arbor, with Dave Judd from Berkeley, Otto Frisch from Cambridge, Courant from Brookhaven, and Tihiro Ohkawa from Japan (among others) joining us. It was during August that year that Kerst conceived of the colliding beam idea, while at Los Alamos. In September Kent and I were in our office when he phoned us to tell us all about this wonderful idea; we must have talked for over

Michigan Strong-Focusing Electron-Mechanical Galvanometer Analogue; 1953

Experimental configuration sketch (top). System block diagram (bottom). The 6SN7's of the field driver were replaced by a pair of 304TH tubes.





an hour, with Kent and I on the two phone extensions.

In the fall of 1954 we formed the Michigan Working Group. Kerst suggested that we needed more continuity of effort, as there were so many concepts being developed and ideas were developing so fast. Kent and I as well as Dick Crane were in Ann Arbor, and Keith Symon was nearby at Wayne State, so that Michigan was a reasonable site for this activity. Almost every week during that autumn Kerst and Jackson Laslett would come to Ann Arbor (via overnight train from Urbana and from Ames) for the latter half of the week, and Symon would drive in from his home northeast of Ann Arbor. These were quite lively, creative times. Kerst was a very stimulating person to work with; he spun out new ideas and analyzed situations with a facility and clarity I have rarely seen before or since. He was particularly adept at understanding magnetostatics; he had an intuitive feel for magnetomotive force planes and magnetic fields which was quite remarkable. Kent was a wonderful foil in these discussions. When presented with a wild idea he would often give it a little thought and then present a very neatly reasoned argument why it wouldn't work. Kerst used to say, after a brain-storming session between a couple of us: "let's see if we can sell this to Kent; if he buys it, it must be O.K." Photographs of the Michigan Working Group and of some of the attendees at the 1955 Michigan summer study are included here.

The Michigan Model

Sometime during late 1954 we felt that our ideas on FFAG were well enough developed to incorporate them into a small model accelerator, and we proceeded to design the machine we believed we understood the best; the so-called Mark Ib wherein the magnetic field alternated from magnet to magnet in sign (but of the same magnitude), but the bending magnets were longer for the positive curvature than for the negative curvature. We chose to use electrons, and to inject them with hardware and techniques Kerst had developed for his betatrons at Illinois. The injection was at 30 KeV and the top energy was to be about 500 KeV; the vacuum tank spanned from an inner radius of 31 cm. to an outer radius of 55 cm. The magnet lattice contained 8 sectors (16 magnets). We worked out the design with Kerst, Frank Cole, and Bob Haxby in meetings at Illinois during that winter and early spring. Cole, working at Illinois, cranked out the detailed design parameters on his Frieden calculator, and Haxby at Purdue undertook to build the 16 magnets there with the assistance of Ed Rowe, a young Purdue physicist. At Michigan, Kent and I designed and built everything else: the vacuum system, injector system, magnet power supplies, mounting, detectors, etc. We decided to use betatron acceleration, and proceeded to build a betatron core which was in fact just a laminated iron picture frame-shaped rectangle. We drove it with a 500 Hz rotary converter (which came out of an X-ray lab in Randall) driven in turn by a motor-generator (for the required 220 VDC). The

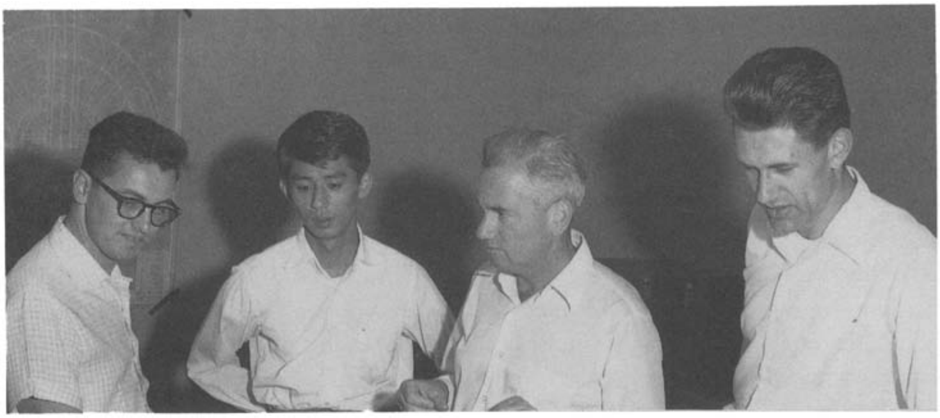
Kent Terwilliger: Ann Arbor Meetings of MURA Physicists

Michigan "Working Group", Autumn 1954. Jackson Laslett, Dick Crane, Donald Kerst, Kent Terwilliger, Keith Symon, Larry Jones.

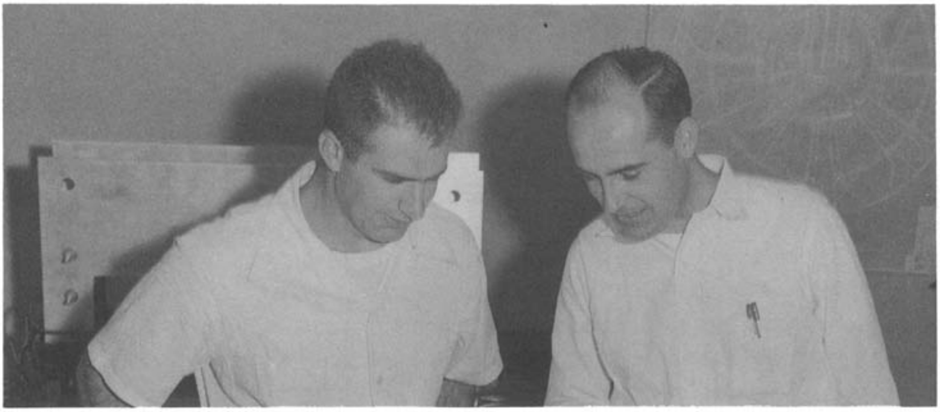
MURA Summer Study, Ann Arbor 1955. Ernest Courant, Tihiro Ohkawa, David Judd, Nils Vogt-Nielsen, Kent Terwilliger, Felix Adler, Otto Frisch.

Kent Terwilliger: MURA Summer Study in Ann Arbor

Kent Terwilliger, Larry Jones, Donald Kerst



Ernest Courant, Tihiro Ohkawa, Otto Frisch, David Judd



Andrew Sessler and Keith Symon

vacuum system was machined out of aluminum by a Detroit tool and die outfit, and was heliarc welded together. Access flanges were then machined into the sides, causing many vacuum leaks (our hard-earned lesson was that you never machine into a weld where you expect to hold a vacuum). The betatron core looped through the support table and vacuum tank, hence these had to be made in two halves insulated from each other so that they would not short out the betatron accelerating voltage.

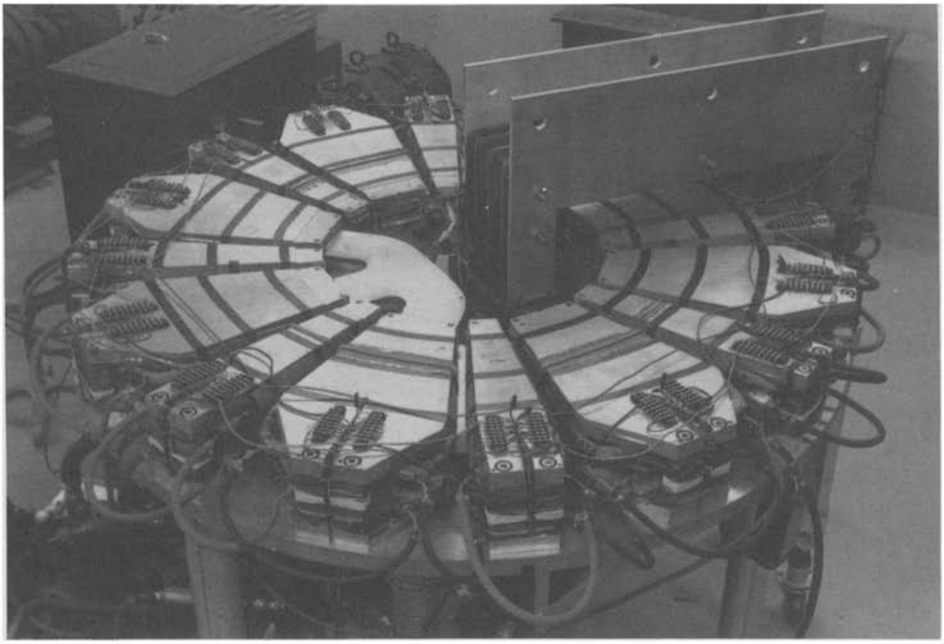
The magnets were delivered from Purdue starting in the spring of 1955 and continuing into the following winter. Even with five magnets we were able to study injection orbits and the injection tune, which we did during the summer of 1955. Finally, in March, 1956 everything was delivered and installed, and we proceeded to inject and accelerate beam. Our detector at that time was a simple Geiger counter radiation survey meter which we connected to our oscilloscope. One of my greatest thrills as a physicist was seeing that first accelerated beam. During the subsequent months we studied orbits and characteristics of our Michigan Model (as it came to be called) in considerable detail using, among other things, Kent's "rf knockout" method of determining betatron frequencies. We also learned that we could "hear" many of the relevant frequencies of the circulating electrons by connecting an rf probe in the vacuum tank to a communications receiver.

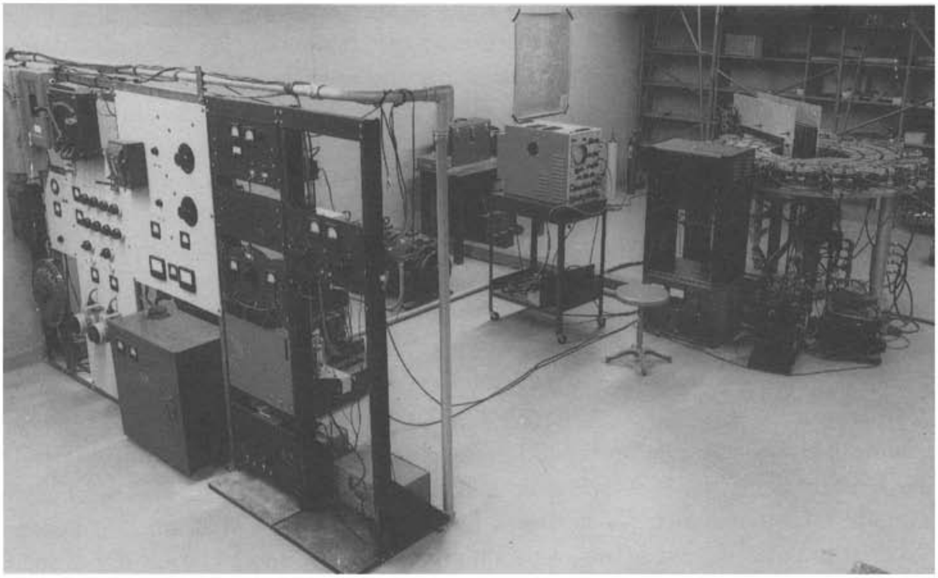
Due to space problems in Randall, we located our Model in the new Phoenix Laboratory on the (then) new North Campus of the University. The building, designed for Nuclear Engineering and for a reactor, was still not complete and the nuclear engineers were not to move in until later. We had a new, well-shielded room for our machine, designed for a ^{60}Co γ-radiation facility. Our summer workshop was held there also. It should be added that Rolph Scharenberg, Mel Stewart, and Dave Wilkinson all worked on the Michigan Model as graduate students, although all three went on to do thesis research elsewhere; I believe that at that time the Department did not think that machine physics was appropriate for a thesis. Charlie Pruett also joined us during that time as a postdoc, and stayed on as a MURA staff member.

In the summer of 1956 the results of the Michigan Model Studies were presented at an international conference on high energy physics in Geneva. I have included here two photographs of this model.

In the fall of 1956 Kerst felt that it was time we came together as a group in a single laboratory; the inefficiencies of long distance communication and commuting were no longer tolerable. Kent and I moved to Madison with our families and took a year leave of absence from teaching duties at Michigan. The Michigan Model was shipped off to Madison where it was reassembled and put into operation. There we proceeded to adapt it to rf operation (as a synchrotron) by applying rf across the insulating betatron gap of the vacuum tank. A self-excited

Michigan FFAG Mark Ib Electron Model, 1956





oscillator was built (using an 829B tetrode) where the the vacuum tank was the one-turn inductor and the capacitor was a string of barium titanate capacitors tuned by biasing them in parallel with a tailored bias voltage. Dick Crane had used such a frequency modulation scheme on the Michigan synchrotron. The scheme worked fine. We were able to accelerate the beam from just above injection (30 KeV) to full energy (500 KeV) with the rf. The rf system is shown schematically on the accompanying figure. The purpose of all of this was to study with real particles the rf phase space ideas of Symon and Sessler, and the principles of beam stacking. Although the vacuum was too poor to leave a stacked beam long enough to bring up another bunch from the injector, it was possible to accelerate a beam to just below full energy and then move the rf through it several times before betatron-accelerating it into a target where its time structure could be interpreted in terms of its radial (hence energy) distribution at the stack. We thus explicitly demonstrated phase displacement and other aspects of the Symon-Sessler rf theories. Kent's bibliography of publications is included as an appendix to this volume, and includes reports of this work. In addition, much of the MURA work was circulated as unpublished MURA reports; Kent was author or co-author of 21 of these.

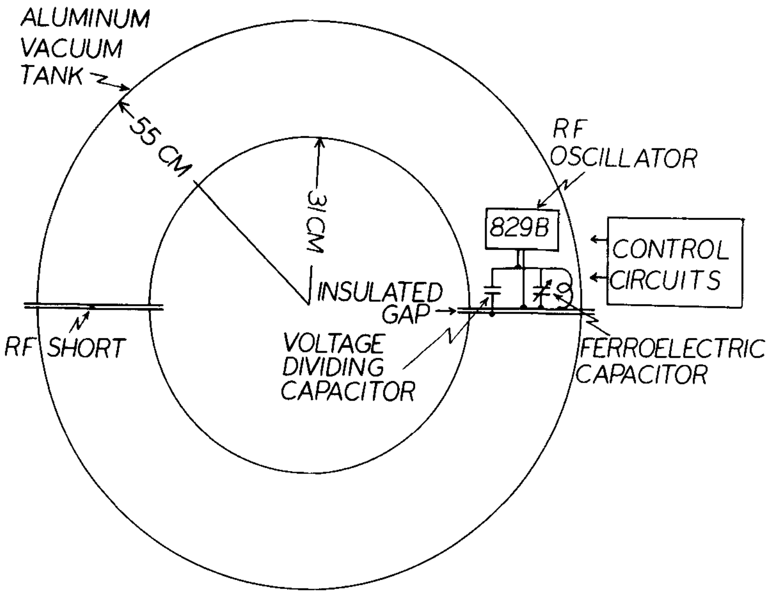
The End of Our Collaboration

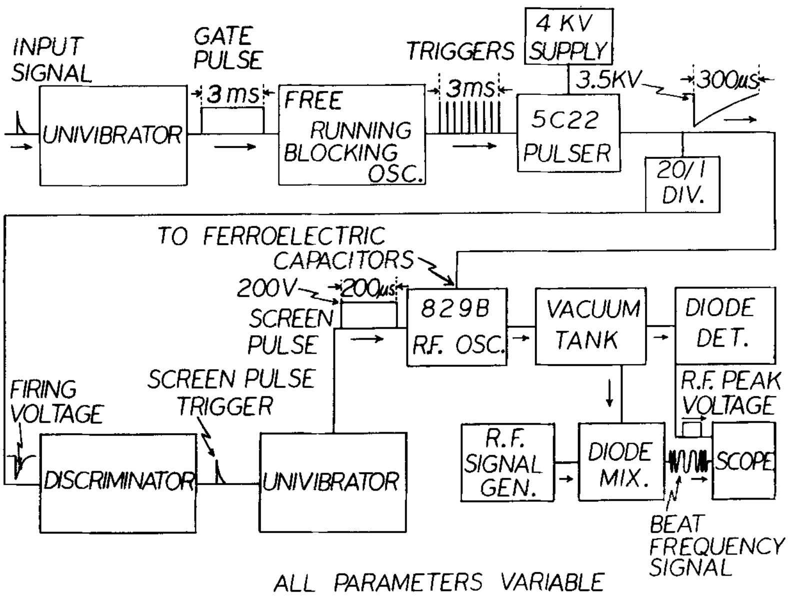
The MURA years were a continual roller coaster of emotional ups and downs. Of course we wanted to build a "Big Machine", and we kept grinding out proposals to do so. However we were often out of phase with prevailing physics sentiments; when we were hot on colliding beams, the conventional wisdom was that what we needed most was a high intensity K factory, for example. Worse, the Atomic Energy Commission (the Department of Energy's predecessor) was very cool toward establishing a new lab in the Midwest. It increasingly wanted any new facility to be put at the existing Argonne Laboratory. The University of Chicago operated Argonne and was not interested in a different university consortium taking it over, and MURA had its collective heart set on an AUI-type Brookhaven operation. The Associated Universities, Incorporated (AUI) is the consortium of Northeastern universities which operates the Brookhaven National Laboratory under contract from DoE. Funding had almost dried up for MURA during the fall of 1956 when Sputnik went up and temporarily saved the day.

In any case, in the spring of 1957 it appeared that the probablity of MURA becoming a full-scale new national laboratory soon was very dim. Besides, Kent and I really liked the university modality better than the more programmatic national lab environment. Another significant factor was that we had become involved in MURA so that we could be involved in building the most advanced machine so that, in turn, we could do the best elementary particle physics. As we had gotten deeper into accelerator physics we had gotten further from

Modification of the Michigan FFAG Electron Model to Synchrotron Operation in order to Demonstrate Beam Stacking; 1956

Schematic plan of method of applying r.f. (top); block diagram of electronics (bottom).





experimental physics, and it seemed that we had to make the conscious transition back. During the few years since our Ph.D.s, elementary particle physics had evolved as a distinct field (rather than merely high-energy nuclear physics), with the discovery of Strange Particles and the turn-on of the Cosmotron. Hence we returned to Michigan in the fall of 1957 with the intention of becoming active in experimental high-energy physics. We remained involved in MURA, commuting regularly back to Madison to confer on accelerator problems and to work on proposals.

One time that I recall in particular was a Saturday morning when we were waiting in the MURA office, killing time until we were to go to the airport. In considering colliding beam 2-way FFAG machines and storage rings, we wondered whether it might be possible to increase the spatial density of circulating beams while conserving phase space by violating "scaling" (a feature of most FFAG designs) and having equilibrium orbits corresponding to different energies intersect at those azimuths where beam-beam collisions occured. This would thus increase the luminosity (as it later came to be called) for a given beam current. We got quite excited about this and called Frank Cole who came down to discuss this with us before plane time. Subsequently, during his sabbatical at CERN, Kent applied this concept to the CERN ISR by designing special quadrupoles to be added to the lattice. These were built and came to be known as the "Terwilliger Quadrupoles".

Since our first professional work at Berkeley every piece of research we had done and every publication had been a joint effort, so that everything was Terwilliger-and-Jones, or Jones-and-Terwilliger. We really had no independent professional identity, to the extent that people even confused whose wife belonged to whom. We mutually decided that we should split up, and work separately on different projects. Thus I joined with Marty Perl to develop the Luminescent (or Scintillation) Chamber while Kent joined with Don Meyer in other work which led soon to their very successful spark chamber program.

Hence, save for the wind-down MURA activities, Kent and I were not collaboraters on subsequent physics. We and our families remained very good friends and close neighbors for many years. We frequently discussed our separate research activities and other physics matters with each other. Later, as department chairman, I was most fortunate to have Kent as Associate Chairman for Research and Facilities.

Graduate school and the several years following it are the formative period in a scientist's professional life. I was very fortunate indeed to share this period with Kent. In a professional sense, we grew up together, and his influence on me was profound. Although very modest and self-effacing, his standards of honesty and integrety were as high as those of any person I have known. It is difficult to imagine a more sensible, thoughtful, intelligent, and wonderful colleague. I treasure his memory with gratitude and deep affection.

Appendix A. Kent M. Terwilliger's Ph.D. Thesis Abstract August 1952 (printed as the University of California Radiation Laboratory Report UCRL-1917)

EXCITATION FUNCTIONS FOR PHOTONEUTRON PRODUCTION FROM 13.5 TO 70 MEV

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I. ABSTRACT

Total neutron yields from eleven elements were obtained as a function of maximum beam energy of the University of California Hospital 70-Mev Synchrotron. The maximum beam energy was varied from 13.5 to 70 Mev. The neutrons were detected at 90° to the beam axis with a BF_3 proportional counter in a long counter geometry. The beam was monitored by the positron activity of Cu^{62} produced by the reaction, Cu^{63} (γ, n) Cu^{62}.

Excitation functions for total neutron production were calculated from the total yields by the photon difference method.

The excitation functions show the same resonance-like behavior in the neighborhood of 20 Mev that has been observed in (γ, n) reactions. However, while the (γ, n) cross sections tend to drop to zero above the peak, the total neutron cross section curves have a flat tail to 65 Mev. With the use of calculated values of neutron multiplicity, the excitation function for gamma ray absorption in tantalum was determined. The integral of this cross section from zero to 65 Mev, $\int_0^{65} \sigma dE$, was found to be $4.87 \pm 20\%$ Mev-barns. The total integrated cross section for tantalum predicted from the Levinger and Bethe theory is 3.66 Mev-barns for an exchange force fraction of one-half and 4.70 for an exchange force fraction of unity. Thus, the experimental result is consistent with the theory.

With the use of estimated values of neutron multiplicity, the cross section for nuclear absorption of 45 Mev gamma rays was found to be
$\sigma = 0.128\ A \cdot 10^{-27}\ cm^2$.