Accelerator Memoirs

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Introduction

I have used eight electrostatic accelerators, have modified and maintained four, and have taken lead responsibility for three. The pioneers in electrostatic machines include Van de Graaff (open air), Ray Herb (pressure insulated), the Notre Dame group with Collins, Waldman, and Coombs (contemporary with and inspired by Van's machine) and Heydenberg-Tuve have worked at three of these four pioneering laboratories and have been a collaborator with leaders of three of the groups. The Wisconsin machines and one of the Notre Dame machines did much of the definitive work in the Bomb project and thus played a major part in shortening the war and saving some millions of lives.

It was fascinating to learn in 1993 that Bill Giegold (my second cousin's husband) had been at Met. Lab. ("Manhattan Project") at the time they were sending samples to Notre Dame for photodisintegration, where Walt Miller was one of the machine operators. Small world! It is also of interest to me that I was associated with Van de Graaff, Buechner, Trump, and Sperduto, four of the first five directors of what became High Voltage Engineering Corp. I also became a customer of both HVEC (N. D. EN) and Ray Herb's company, NEC (SNICS source). HVEC and NEC are essentially the two companies in the world that have made and sold lines of electrostatic machines.

Vacuum Insulated Accelerators; 1946

My first experience with accelerators was the initial project given me by Ray Herb when starting graduate work. This was the design of a vacuum insulated machine. I didn't know at the time that he had tried this for his own graduate thesis work but had in the end, and somewhat out of desperation, pressurized the tank. He produced the prototype of all the successful electrostatic machines. He told us the story of holding a meter stick against the end of the tank (originally designed for vacuum) to judge its distortion as it was pumped up, so he could guess whether it would safely hold the pressure. He had a formula for comparing strength needed for vacuum to the allowable positive pressure. Anyway our assignment was to build a model in a vacuum tank to hopefully hold hundreds of kV. Johnny Faulkner and I got an old steel tank and scrounged a vacuum system (about which I then knew almost nothing) and got vacuums in the below Torr region. I dreamed up something like a Wimshurst machine to charge a terminal. Ray insisted that I learn machining techniques, so I learned to run a lathe by making the vacuum feed-through for the drive shaft. The device charged all right but we got arcing at 10 to 20 kV. We then took the tank to the Ohio Chemical Co.(which was making medical equipment) and got them to nickel plate the interior, with the hope that the smoother, cleaner surface would help. The plated tank out-gassed even worse. I think later we discarded the induction charging system and used a big power supply. We never got more than 20 to 40 kV. Ray decided to abandon the project. Later it was clear he realized the problems were basic to vacuum systems and so he developed the Getter-Ion pump and did all the work on surfaces and brazing techniques that are incorporated in NEC accelerators. Ray founded NEC after retiring at the University of Wisconsin. The company promptly cornered the world market for electrostatic machines. But I needed a thesis! Ray essentially fired me when I suggested that redoing proton-proton scattering did not seem to be a very pioneering project for a thesis. He sent me to Hugh Richards. I did some experiments with Richards' group on measuring (p,n) threshold energies. I was sent to an APS meeting at Chicago to give my first ten minute paper. The topic was the 7Li(p,n) threshold energy. At this time and especially after the spherical analyzer was built and coupled to the Wisconsin accelerator, I learned to run that machine, maintain it, learned the tricks of high voltage in pressure and operation of an ion source.

Wisconsin "Long Tank"; 1948-1951

This machine got its name as a code word during the time it was at Los Alamos doing physics leading to the Bomb. The earlier Wisconsin machine that went with it was called the "Short Tank". The machine was sneaked out of Sterling Hall, through a window opening, in the dead of night and moved to a flat car on the railway. About this time I was a junior Physics Major working as an assistant to H. B. Wahlen in his lab. I once asked what the big empty (but obviously once- used) room at the other end of the hall was for. I was told very peremptorily to shut up and not ask questions! Wahlen had also asked me to sign the Espionage act before starting to work for him. I couldn't see why measuring optical emissivity of a metal was vital to national security but later realized that the metal was uranium which "H.B." had borrowed from "Metallurgical Laboratory" at Chicago! It was Top-Secret that vast quantities of Uranium were being amassed! After the war Prof. Williams of Minnesota gave a colloquium in the Department in which he stated that seven of the ten crucial measurements needed to determine the feasibility of a bomb were done at Los Alamos with the Wisconsin accelerators. He also said that one of the ten was done almost single-handedly by H. T. Richards (soon to be my major professor).

Anecdote: One day while Johnny and I were working on our "vacuum insulated generator"(in a small lab next to the once mysterious "empty room"), a crew of workmen began bringing crates into the room. Two of the crates were long and narrow. I objected strenuously to one of the "workmen" that this was our research space and we didn't want it cluttered up. The guy looked very young and most unintimidating. I was politely informed that I was speaking to Professor Richards and that he and the others were bringing the "Long Tank" back home. Hugh was in charge of returning this accelerator to Wisconsin and he was starting his career as a Wisconsin faculty member. He had made an agreement that he would do no teaching until his research was established. The crates contained the accelerating tube that was to provide the beam for my thesis. Obviously neither of us knew that Hugh was speaking to one of the first of his graduate students to *get his degree. (I am listed number six in Hugh's memoirs but I don't remember the first two, were they war time types? At a reunion Hugh claimed I was his first). I thought I was second, between Bashkin and Fay Ajzenberg. Hugh had 50-some in all! Small world; Only recently and after hearing Hugh and others reminisce at the APS Division meeting in Sante Fe did I realize that when I was sitting in Ira Davis Physics class at Wisconsin High School and being intrigued by the rumor of nuclear fission and the "ability to drive an ocean liner across the Atlantic with a kilogram of Uranium", a block away Ray Herb was building the prototype of the accelerators which would do the bulk of the necessary nuclear physics. Two of the accelerators would be vital to the Bomb effort. Then after the talks at the 1992 Notre Dame reunion and after reading Bill Giegold's history, I finally realized that a third, vital electrostatic accelerator, was at Notre Dame the "Long Tank" made the beam for my thesis and the tank of the N.D. machine housed the accelerator that was used in a large part of my whole research career!*

Back to the Long Tank! The control console was at the base of the tank with no shielding. The voltages and currents in the terminal were read by standing up and peering through a window in the base of the tank to look at meters in the terminal. The unshielded beam line was about four feet to your left as you sat at the console. Later when we got the spherical analyzer running we put our data-taking table just beyond the 900 electrostatic analyzer, essentially on the accelerating tube axis. I think it was known that x-ray and gamma-radiation were detectable in the accelerator room and perhaps even in the Physics library, two floors directly above the tank, but we didn't really think about it until we noticed our scintillator detector on the spherical analyzer (home made by dusting ZnS on a patch of glue on a phototube), (at least we were ahead of Rutherford in not having to use eyeballs to detect the flashes) kept counting when the beam was stopped ahead of the target. Counting stopped, however, when the terminal voltage was removed from the accelerator. We picked up the detector (I can't remember what the phototube circuit was) and started down the hall away from the accelerator lab. When we got to the main Department shop at the far end of the building and the thing was still counting (yes, correlated with voltage on the accelerator) we knew we had a problem. This was my first awareness of the problems of neutron shielding. Shortly after this discovery, some concrete blocks began to appear. It was also pointed out that the Department coffee hour was held at the high voltage end of the tank and some of the theorists (e.g. Sachs) liked to lean against the end of the tank in line with the tube while sipping coffee. We didn't know much about tube loading and secondary electrons coming back up to the terminal. I don't believe, however, that anyone was in any way harmed. Maybe a little radiation has kept Ray going strong into his late eighties!

The tank was pressurized with air. Compressed air from the University mains was used. Normal pressure was below our desired operating pressure (lOOp.s.i.) but by calling the maintenance office ahead of time one could get them to increase main pressure for the short period needed to fill the tank. We were aware of the danger of fire in a tank pressurized with air. A CO2 system was permanently connected to the tank and the operators (us) were warned to be alert for any pressure rise in the tank and to look through the window at any sign of trouble.

Anecdotes: The tank was entered by removing the end plate at the high voltage end. Access to the terminal and source was easy but, as the column was tapered, the only way *one could get to the low energy end was to squirm flat on your stomach or back, with your head turned sideways, in the small gap between column hoops and tank. Doctrine called for mopping the tank out with alcohol moistened rags as a last step before closing. A final wipe with "bare skin" was deemed the very best technique. When Fay joined Richard's group she was eager to help and to be considered one of the "gang", certainly not a "Lady". We suggested she might do the honors of the final swabbing and wiping. We did reject the "strip to your waist" routine! She gamely went into the base end of the tank and started work. Fortunately we sensed she was having trouble and someone realized she had gotten a bottle of methyl alcohol. A rescue team immediately dragged her out and fortunately there seemed to be no lasting ill effects. OSHA would have had a field day! Lots of beryllium around, hydrogen gas used for leak sniffing (I had a whole cover over the Spherical Analyzer tank explode right next to my left ear and McGruer fairly regularly blew up the large tank he used for hydrogen brazing). Adair would demonstrate that a radium-beryllium source wouldn't hurt you by putting it in his mouth (after the German machine gun what were a few neutrons?). I had a vacuum degreaser that consisted of a large tank of boiling carbon-tetrachloride. One day I bent over to retrieve something I had dropped to the bottom and got a good whiff of vapor. Things went black pretty fast and I staggered back. Fortunately the next breath was pure air and I revived. I have read that "Carbontet" would be a good anesthetic except that the difference between unconsciousness and death is about one breath!*

MIT Open-air Van De Graaff; 1951-1953

I was only an observer for the last days of operation of this famous machine as a working accelerator, but then watched its tests as a museum piece and its disassembly for moving. It was in a huge sheet metal dome attached to the High Voltage Laboratory building at M.I.T... There was a feeble attempt at drying the air in the dome. There were two huge spheres set on 40ft. columns. The two spheres were originally charged with opposite polarity and were to have an accelerating tube running between them. When the machine was moved from foggy Cape Cod to the MIT campus, the two spheres were joined together. The charging belt was in one column and the accelerating tube in the other. Ion source controls were in the terminal and a huge ladder was placed against the sphere (40' off the ground), to allow one to climb up and into the terminal. One day Harold Enge was up in the terminal and Hugh Watson (graduate student and my house-mate) came in, thought no one was there, moved the ladder, and charged Harold up to about 2 million volts. Harold knew his physics and was careful not to stick his head out of the terminal. He claimed he felt nothing! He was concerned about getting radiated however, so he pulled a wire off the ion source, killing it. Soon Hugh came up to see what had happened to the beam and found a somewhat annoyed but unhurt Harold in the terminal.

The target room was beneath ground. As ground was a foot or two above the level of the Charles River and the river was a block of swampy ground away, continuous pumping was required to keep the target room dry. When power failed for some period of time the water level rose just over the target chamber. I was told that the building was a "temporary" World War building that had been used for torpedo development. I believe that during the Second War, Van, Spud, Bill and others had made x-rays for the purpose of looking inside weapons. A railroad track ran a few feet outside the window of my office and so each morning a deep layer of soot had to be cleaned off all horizontal surfaces. The new “Building 58” was going to be a real pleasure! The final data for Hugh Watson’s thesis was to be the last running of the accelerator. Hugh, Professor Curt Powel and I were sharing an apartment so I got daily reports on the progress. Some time after the accelerator was shut down we were told that Van was making arrangements with the Boston Museum of Science to move the machine to their building on an island in the middle of the Charles river and set it up as a display of high voltage. Van arranged a test demonstration for Museum staff and a picked audience of children and mothers. He set up bleachers in the dome with a rather open wire mesh screen in front. I was in the audience. Van ran the machine up to sparking voltage and got horrendous sparks to the dome. He had a small sphere hung by a conducting rope from a terminal. This was made to swing as a pendulum. He swung it toward the mesh screen so the sparks (several feet long) hit the screen right in front of the audience. The kids squealed with joy and the mothers screamed in terror! The museum people I decided it was an excellent demonstration. Van had a model farm set up on the floor, complete with barn, haystack, house, lightning-rod, etc. He charged up the terminal, swung the sphere over the haystack, got a big spark and the haystack went up in flame! I assume he had drenched the haystack in gasoline. The house with its lightning rod was unharmed. I have slides of the later disassembly of the machine as it was moved to the museum.

ONRIMIT 12 MV; 1951-1956

About the time I got to MIT the control of the High Voltage Laboratory (Van de Graaff, then Bill Buechner) was given to Bill along with the new, not yet operating, “ONR-MIT 12MV accelerator”. Apparently Trump, in the Engineering Department, who had been responsible for building the machine was not happy about this and had instructed his people not to give any assistance in assembly or operation. There had been a chance meeting in an elevator when Bill B. was showing me around. When Trump was told that I came from Wisconsin he reacted as though he thought I was a spy sent by Ray Herb (the enemy). When I first saw the accelerator there was an empty tank, except for the base plate and drive motor, and a collection of column sections, terminal, intermediate shell, etc. stacked on the “bridge” above the tank. In early June, Bill B. announced that as usual he was leaving for his three months summer vacation and hoped that I would see about getting the accelerator assembled. Also get the beam analyzing magnet finished, get an ion source and try for a beam! I didn’t even have any drawings of the beast! Fortunately Charlie Goldie took mercy on me and said, although Trump had forbidden him to enter the accelerator building, he hadn’t said Charlie couldn’t talk to me in his office. So I got verbal instructions on how to put the thing together. Of course there were other people around, especially Sperduto (“Spud”) but as I remember Charlie Bockelman didn’t come until we had at least started assembly. The accelerator was contained in a vertical tank, enclosed in a circular tower. Some distance from this tower there was a second, rectangular tower. At ground level these were connected by the building containing target room, control room, compressor room, a large experimental area and one rather spacious office. Apparently only one faculty member was expected to work in the building and the staff was thought not to need space. Bill made this his office. One end of the experimental area was walled off and we put three or four desks in there. Across the top of the circular and square towers there was a connecting bridge. The bridge had an overhead traveling crane which could lift things out of the accelerator tank and place them on the bridge floor. An elevator took one from ground level to bridge level. There was an elevator platform suspended from cross beams which could be placed on the top of the open tank (the lid having been moved to the bridge floor by the crane). People stood on the platform and lowered it to the portion of the accelerator column being worked on. The platform was a close fit to the inside of the tank and around the column.

When we started taking data we turned part of the control room into analysis space and Bill moved all the plate readers (all young women) to desks in the compressor room. The readers had many pungent comments about working in the room when the compressor was running! Spud was designated straw boss of the plate readers but I pretty much managed the work distribution and counting oversight. The charging belts were terrible. Later when the generating voltmeter was working and we had a beam, I estimated voltage swings to be as much as 1 MV per revolution. Facilities for vulcanizing a belt of this length, all at one time, didn't exist and electrical resistance apparently varied from section to section. We installed a sprayer to spray antifreeze on the belt while running and gave it a shot every now and then. Sometimes this helped. The beam, of course, went through the exit slit of the analyzing magnet only a fraction of the time of each belt revolution. Especially in the wee small-hours of the day, when you were exhausted, the violent swing of every meter on the panel, in time with the belt rotation, was, to say the least, somewhat hypnotizing. Nevertheless in a year or so when I got the beast up to 8.5 MV for a few minutes I was under the impression that this was the highest D.C. voltage yet attained anywhere. As a strange coincidence my mother had come to Cambridge for a visit and she came into the lab with me that evening. We did come to expect to be able to take data at 8 MV for some period each month. Why the cycle? The vacuum pumps were mercury diffusion and after about a month of operation most of the mercury had condensed in the huge cold traps above the pumps. Thus there was not enough fluid left in the pump. We had to warm the traps, and then bang on them to get the mercury back down in the pumps. Start up voltage after this was more like 5MV and it took weeks of running to work back up. About the time everything "conditioned" it was time for another cycle. We never got anywhere near the "12MV" in the accelerator's official name. I got the idea of using a pressure triode in place of plain corona needles for voltage stabilization. Harold Enge whipped up one of his circuits which he designed by mounting a bunch of vacuum tubes on a chassis, along with a few transformers and "pots". He then started to run wires and to test until the thing worked. He explained that this was why there were always more tubes on the chassis than were actually used. "I don't want to have to drill more mounting holes after the circuit wiring has begun but the design is being changed" The pressure triode worked with a much smaller voltage swing on the grid than would be needed on bare needles and Harold's small box was all we needed for corona control. We ended up with the highest voltage, high resolution beam from an electrostatic machine, in the world.

The original design of the accelerator called for disassembly of the entire column, section by section in order to change a belt. Resistors were mounted between the belt runs and had to be removed section by section. After doing this a few times Charlie Bockelman and I were mighty sick of it. I measured the space between belt runs and the space between my breastbone and spine and the width of my head and concluded that I could just fit between the belt runs with my head turned sideways. We rigged a platform suspended from the crane for me to stand on. With my arms above my head I could remove a resistor and juggle it into a plastic tube which led to the bottom of the tank. Someone would break its fall and remove it from the tube. The crane operator would periodically raise me as I worked up the column from bottom to top, removing resistors. The new belt was then installed and I then got on the platform at the top, caught a resistor dropped down the tube from the top, and installed it. The scheme worked and cut the belt change time from maybe a week to a couple of days. I was mighty careful about who ran the crane controls while I was in the column. I had been told too many stories by Bill, Spud, and "Shorty" Vaudo (ex Ball-turret gunner on a B-17) about the horseplay that was common in "Van's" lab.

Anecdote: I realized the new belts had a strong phenol odor but after a few minutes inside them I got used to it. When I went home that night I knew that my clothes had the odor but didn't realize how bad it was, (even then my sense of smell was not too good) until the next morning when the entire apartment reeked! My house-mates were incensed. Next time I took a change of clothes to come home in. Imagine what OSHA would say about this now!

The machine sparked regularly at almost any voltage above about 5 MV. It was spark it, or don't take data. I was used to the Wisconsin machine sparking fairly often and was not too concerned. After a couple of years we opened the accelerating tubes and found that the electrodes were covered with glass chips, mostly at the top and decreasing at lower levels. The bottom of the tube had a thick accumulation of glass that had fallen down. From the side one could see through the glass sections and observe large chunks, some as big as the end of your thumb, missing from the inside of the insulators. "12MV" accelerator! Sure! 8.5 was a stretch. The construction of the column was faulty. Flat aluminum planes were used with rolled bars ringing them. The planes were not flat and the ringed edges were not true, so the gaps in the column varied a great deal. The terminal was not flared at the top of the column and thus there was a field concentration at the junction with the column which caused copious sparking and tube damage in the top few sections. The sparking raised hob with circuits in the terminal. I got so desperate about blown components that I built a large rectangular box out of eighth in. copper plates, hard soldered together, and put all the source electronics inside. This brute-force approach worked and the source was reliable.

The accelerator did do a lot of pioneering physics and hopefully taught the HVEC people a few things that led eventually to the beautiful EN Tandems (Yes they did accept Ray's scheme of horizontal machines but the basic column and belt structure is similar to the ONR/MIT machine. The RE ion source didn't work worth a damn. I have forgotten details but I decided the geometry of the exit aperture and focus apertures didn't make sense. I knew nothing about these sources but tried some fairly simple modifications and got something that worked better. One day I came into the lab and found that my drawing of the modified source had disappeared from my desk. Someone (plate reader?) said she had seen a stranger remove it. Later the new source sold to MIT by HVEC seemed to be identical to my drawing! Much later when I knew I was to convert the Notre Dame machine to positive operation I arranged with HVEC to sell me a source. They were pleased to do so until they found I wanted it delivered to Notre Dame. Then they refused to honor the order on grounds N.D. had stolen the 4 MV accelerator design from them! (see section on N.D. 4 MV machine) The bad feelings arose again years latter when we ordered the Tandem. By then Bill Rodney was willing to swing the power of U.S. Government to our side but the HVEC brass always seemed to try to give the impression that our machine didn't exist. Charlie Goldie, now with the company, was again most helpful.

Anecdote: One night the phone at home rang in the wee-small hours. I was told that there was a tremendous roaring noise in the accelerator building and I was the only one on the emergency list that could be reached. Would I rush in and tell Security what to do? When I got there it didn't take long to see that tank pressure was falling and to find that the sound was coming from somewhere up along the side of the tank. It was a broken feed-through bushing on the corona control. I started the compressor and began transferring gas to storage but we lost a fair fraction of the tank gas. Years later (May '67) a similar episode happened at N.D. except it was vandals dumping a Ge-detector so liquid-nitrogen made vapor which scared Security. Also the hallway was strewn with computer cards. Why me?

This accelerator had a differential pumping tube. These things were the latest fashion. The idea was to provide more pumping speed at the terminal. We became aware that after running for some days with beam, most of the loading was in the differential tube, not the beam tube. We discovered that leaking a bit of gas into the bottom of the differential tube so that its pressure rose to a few times iO~ mmHg, improved performance and raised terminal voltage. Practice became to set the appropriate belt charge and then increase the leak until the voltage went up from 5 MV or so to the desired value. We usually ran at between 7 and 7.5 MV, as I remember. For Q-value measurements the input energy was not critical so long as it was high enough to excite the reaction. Later at N.D. Walt used to bleed hydrogen into the tube while running negative and again set voltage by adjusting leak rate. I

To observe the beam, Trump's people had put a large quartz plate (six inches square) on a rotating rod, into the tube extension just below where it came through the ceiling of the target room. To observe beam one ran the voltage down, went into the target room, climbed the ladder and rotated the quartz into place. Then secured the target room, and got beam. You then observed the quartz through a small telescope in the target room that looked through the thick water shield window (and a glass window in the tube extension). One day while trying for a beam I became aware of unusual light in the control room. I looked through the shield window and saw the target room brightly illuminated. As the lights were interlocked to the door and the belt charge, I was puzzled. Only momentarily, until the white hot quartz plate shattered and the light went out. I suppose I had put a microamp or so of 7 MeV protons on the plate and blown it apart! Soon we had entrance slits to the beam analyzer and could read beam position at the console. The telescope could also be sighted on a mirror under a window in the base of the tank, so that meters i~n the terminal could be read. This was an advance over the Wisconsin machine in having shielding between the operator and the accelerator while looking into the terminal. I

Notre Dame 4 MV; 1956-1989

When I visited N. D., Bernie and Walt explained that the tank of the new accelerator (the third N. D. accelerator) had been used for the earlier accelerator (the one that did the “Metallurgical Lab.” work), in what was then the Science building and is now The Huddle. The accelerator was new, the design being identical to the Brookhaven Cosmotron injector. This was a Ray Herb design. One summer, while I was at Wisconsin, Oscar Sala was visiting from Brazil and he and Ray were working on drawings for a new machine. I think this was essentially the design; (A horizontal Herb type, not a vertical Van de Graaff type). HVEC got the drawings, I think, from Brookhaven and made the Cosmotron Injector and the Duke machine from them. N.D. drawings have seen say “Property U.S. Gov.” (Can’t find these drawings now, only some equipment drawings saying HVEC). Whether Bernie got them from Brookhaven, Ray or elsewhere I never learned. HVEC of course built the Brookhaven machine and also sold copies to Duke U and Strasbourg. Bro. Cosmos, Walt, Bernie and Oscar built the N. D. Machine. I was told they got all of $35K from the Defense Department as a contribution toward the machine. {Some story about “Engine Charlie” (Secretary of Defense) not thinking accelerators were good for General Motors and hence not good for the U.S. }

I was allowed to watch as the brand new machine was turned on and the voltage run up, ostensibly for its first trial run (undoubtedly somewhat staged). I was much too naive to know about tricks of “site visits” . I was most impressed that this fine looking accelerator had been built completely with N.D. facilities and resources. It did seem like an awful come-down to think of experiments at 4 MeV after getting used to, say 8 MeV with the, at that time, worlds best combination of high resolution at high voltage with a high resolution spectrograph. The accelerator vault was empty beyond the short stub of beam line sticking out of the base of the tank. When I asked what they intended to do with the space, Bernie and Walt said they hoped I would have some ideas and suggestions. At Case I was shown a completely empty vault and asked for suggestions about putting a machine in it. Here there was already an ion beam! I immediately began thinking about a copy of the Browne-Buechner Spectrograph.

The machine had been built to run negative. Could polarity be reversed? The column had corona gaps rather than resistors for voltage grading. They would have to be reversed in direction each time polarity was changed. The belt charging power supplies would have to be reversed. The almost empty, nice large terminal would have to acquire a positive ion source. A beam analyzer ith associated beam lines and pumping would have to be built. A copy of the MIT B.B. spectrograph would have to be built (I didn’t want to make the publication-stream break any longer by spending time redesigning). All work, but doable and straight forward. Terminal voltage stabilizing was done with a tank liner rather than corona loading. My impression is that this was a Waldman-Miller idea. I also have the impression that the use of a generating voltmeter to measure terminal voltage was a Notre Dame development.

After some haggling, HVEC sent me an RE ion source bottle. Oscar made an oscillator and I scrounged an extractor power supply and set the source up on the target-making vacuum system. As I recall there was an output of some l00μA. At the first opportunity we opened the tank and reversed all the corona needle gaps in the column, also the charging, and then demonstrated positive voltage. I don't remember but I think it went easily to 4 MV. Then at the next opportunity I got electronics in the terminal, removed the electron gun and put the RF source on. I probably got some l0 pA of protons out of the tube extension. We never did really solve the problem of transmission in this accelerating tube. I also worried that there was no ground plane in the tank and that things like the drive motor were not shielded from sparks. One day I got some copper screen wire, scrounged four aluminum bars, a drill and tap, and some bailing wire and made a "temporary "ground plane. It was still there when the machine was dismantled in 1993!

This accelerator had an improvement in the means used to observe terminal voltages and currents. Unlike the "Long Tank" method of eyeball to tank base or the MIT/ONR eyeball to telescope, it had TV-camera to tank base window. One had the luxury of sitting at the control panel and watching the meters in the terminal! They were rather hard to read accurately and latter Jim Kaiser made a masterful design for, and built, a telemetry system that digitized signals from the terminal on a light beam, detected the beam at the base of the tank and gave digital readout on the console. To humor my conservatism he also had (apparently) analogue meters on the panel although they relied on the digitized light signal. Such luxury! This system was Jim's Masters Thesis. Walt was very insistent that I not burn up his quartz viewer in the tube extension. I remembered MIT and always tried hard not to, but once, some years latter, we did forget to swing it out of the way and hit it with a positive beam. I don't remember any disaster but there were some hard words. I went about designing a slit to form an object for the analyzing magnet, then being built. It had a massive copper bar that could be moved up and down to put one of three? slits with gold jaws for 0.5, 1.0 or 1.5? mm opening into the beam. Always henceforth this was referred to as the "copper bar" and the measure of performance of the ion source was "copper bar current". Of course there was an "out" position for electron running. It did not take too many polarity changes to make all of us decide we wanted a resistor string instead of corona needles for grading. This was a cooperative project of Walt, Bud? and me.

Running schedules followed a pattern of equal time for each group, with about two or three days as a typical run. With Paul Chagnon now on board this meant about ¼ time negative. The need to open the tank, let the tube up to air, remove the one source and install the other became onerous so we thought about a "source exchanger" which could be run with the tank at pressure and that would rotate one source out and the other in. Bud's Post-Doc (Lucas Schaller?) did the development of this concept. I did much of the terminal survey and dimensioning, and did the mechanical drive and vacuum seal which evolved through a number of models. In spite of accusations about "Rube Goldberg" it worked and saved us a lot of time and trouble. It would not rotate at full tank pressure, however, but the time to drop to say 60 lbs and then pump back to 100 lbs made negligible delays in running.

Early on, Walt and I felt we were not getting enough voltage stabilization from the liner. Walt pointed out that the liner was shorter than the terminal. I undertook to design a longer liner and found a local welding and machine shop that would roll sections of steel, bring them into the vault and weld them into a cylinder. We made Mycalex standoffs to bolt to the tank and liner. Workmanship was somewhat shoddy but the thing worked well and served as long as the accelerator was used. I think Walt raised the voltage of the driver at this time also. Paul designed and built a very fine control circuit that allowed varying and mixing inputs from slits and GV and varying control of liner voltage and up-charge. It had an automatic recovery-from-spark system. A true “Messy” (Paul Chagnon’s trademark) triumph.

Anecdote: The liner sections were brought out about a week or ten days before Christmas. I had specified that the job be done by Christmas and was pressing the company. They said “no problem” they would work some overtime. The welding seemed to be going very slowly and the quality was getting very bad. I complained and the boss came out, but not much changed. Then I realized that the welder and helper had decided to celebrate the holidays a bit early with a bottle in the vault. As I remember, the liner was finished by New Years with some compromise on how many welds were redone and how much grinding was done. The bottle was finished before Christmas! The grinding dust in the vault was awful.

Other improvements included a new gas handling and tank evacuating system for which Chuck Kelsey did most of the work. A beautiful liquid nitrogen trap that would hold for a week was designed and built by Irv Michael (Post Doe.), using the skills and tradition he had gotten at Wisconsin. Both these systems served until the end of the machine use. We installed valves and reservoirs in the terminal for the He4 and He3 beams. He3 beams were still novel and we got some good papers out of them. It was doctrine that maximum negative voltage was about 1 MV lower than maximum positive voltage. I recall that we once took data at 5 MV positive and Walt occasionally got 4 MeV electrons. 4.5 MV positive was rather routine. Altogether it was a fine accelerator!

The grant for the new Tandem accelerator and building was made on December 17, 1965. After this I was more and more involved with building-design and contracting , actual building construction, accelerator specifications, purchasing, testing, shipping and installing, and all the other myriad details of the $2M project. Less of my attention could go to research with the 4MV machine. Andre accused me of being able to follow six independent projects at one time and answer questions on the run (literally). One of my students, however, accused me of neglecting him in favor of the new building. Having John Goss and later Pete Jolivette made it all possible. A large contribution was made by graduate students and undergraduate Physics and Engineering students. We had money for them at that time and they learned a lot and we gained a lot. It was fun at reunion to have people say, “I built that” When the Tandem came on-line, interest in the 4 MV waned. With the advent of negative heavy-ion sources a new possibility for its use arose. A talk was given at a meeting in Glasgow in 1954 by Bill Buechner. I don’t remember whether I met Roy Middleton there but he was very interested in the new spectrograph. Clearly he wanted one, or perhaps a more advanced one. It is not clear to me how the ideas grew but Roy built a Multigap (at Penn.) and Harold built a Multigap (at MIT). Roy then turned to sources. Ed informed me that Roy was giving a talk at a SNEAP meeting - about negative heavy-ion sources. I decided to go with Ed to Brookhaven to hear the talk. Roy saw me and asked what I was doing at a meeting on equipment. He was amused when I said I had come to listen to him. Later Ed and I negotiated a contract with(?) for a source that could be run in a terminal inside a pressure vessel. We were the first or the second to do this. It was put in the 4 MV in the late seventies?

The accelerator reverted to negative operation and became an injector of heavy ions for the Tandem. I spent much of a summer revising the terminal so it could still be converted back to an RE source and positive polarity but Ed didn't like my workmanship and thought there wasn't enough room so he tore out all the positive polarity stuff. We never missed it. Using the machine as a negative heavy-ion injector to the Tandem, along with J.J.Kolata joining the faculty, not only helped in keeping the lab alive through the era of many closures but moved us nearer the forefront. The design and construction of the injector beam line (optics by W.C. M., the rest by E.D.B.) was a major feat of skill, ingenuity and hard work. When Garvey was NSF Program Officer he saw the beam line and simply couldn't believe that we had done it with no additional funds and with our budget which was the lowest of the Tandem labs.

One outstanding event with the 4 MV machine was the fire that occurred 4/22/67. As I was driving back to the campus one evening I saw the fire truck pull out of the fire station and turn south on old Juniper Road. Of course I wondered where the fire was. When the truck made the turn toward Nieuwland my interest rose considerably. When the firemen dashed in the side door nearest the control room, horrible thoughts arose. Sure enough they went into the control room! As I dashed in I met Paul in a very distraught mood. He said the accelerator was on fire and feared we had lost it. It was standard procedure, after any maintenance, to run a charging test in air before the tank was closed. The charged terminal attracted dust from the room, which was undesirable. Thus the tank had been rolled almost closed as a dust shield and then the belt was started and the charging check begun. This time something had arced in the terminal and started a fire in insulation (and capacitor oil). With the tank closed the firemen could not get to the fire. A truly Bernie act by Grant Garritson (our Navy Officer-Grad student), who dashed into the smoke filled vault and behind the tank to turn on the tank mover, saved the machine. As soon as the tank rolled back a foot or so the firemen could get extinquishers in the opening and this very quickly quenched the fire. The column was still in place and at first seemed undamaged. When we removed the terminal shell and dug through the mess in the terminal, however, we found that hot gasses had gone down the insides of the hollow Textolite insulators that support the column; especially the two upper ones. The insides were charred. Tests showed that they would not hold voltage and things looked bleak. We made a "reamer" consisting of a disk covered with sandpaper, just the diameter of the inside of the Textolite, and mounted on a long rod. The rod was spun with an electric drill. With several days effort the insides of the textolites were abraided to remove the char. The machine did go back to over 4 MV but my impression is that it was never quite as good as before. Of course it did noble service running negative at 2 to 3 MV, as an injector for the Tandem.

Wisconsin EN Tandem; 1959-1960

The second summer that I went to Madison to teach summer school the brand new EN Tandem was being installed. This was an interesting case of bringing coals to Newcastle, or HVEC products to Wisconsin. Of course the inspiration for Ray's development of negative ion sources was to exploit the tandem principle. Development of negative ion sources and the idea of the tandem principle had matured. (In his memoirs H.T. R. takes credit for He- and for SNICS). Actually the first EN went to Chalk River but Wisconsin had the first one in the U.S.. I am afraid I wasted many hours watching the tank being moved in and the installation begun. Perhaps I had some vague dream about a similar scene at Notre Dame someday? I did think it would be nice to use this shiny new machine with its 10MeV proton beam. The next year when I was again invited to come to teach in the summer, I bargained that I would come if I could have some EN beam time. Barschall agreed. We had gotten wind of silicon particle detectors by then and heard that Argonne had made some experimental ones. Friend Borchers [Borchers had built the NMRs for the N.D. beam analyzer and spectrograph while an undergraduate, had gotten a Ph.D. at Wisconsin and become Associate Provost at Wisconsin. When not made Provost he went to Livermore] had connections at Argonne. He got me a sample detector. Wally Schier made a mount and a target and brought them to Madison and we measured a reaction cross section. I think it was about the first charged particle measurement with a Silicon detector to be published. I

Argonne Tandems; 1963-1973

Argonne Lab. wanted a spectrograph and I was asked by John Schiffer and Lin Lee to design a 75cm. version of the Browne-Buechner Broad-range. I managed to have the target chamber essentially identical to the N.D. chamber and, most important, so that the lids with the target mounts were interchangeable. Thus targets made at N.D. could be immediately inserted at Argonne. Bollinger was head of Physics and was very receptive to our using, first the EN, and later the EN (HVEC called the Argonne EN an “upgrade” rather than a new machine so it wouldn’t count as a new Tandem). It went through production with the N. D. machine, however. Another big factor was that John Erskine (my first N.D. Ph.D.) was working at Argonne. The policy there was that physicists couldn’t touch the accelerator controls, only technical staff could. John, however, had remarkable rapport with the staff and so when we were totally frustrated with low, or no, beam I would get him to come down to the control room. While we talked to the operators, John would quietly twitch the knobs and get the beam for us. When Lin went to Stony Brook he took some of the best staff with him and my student, Gary Marolt, had a dreadful time over the period of a year without getting any useable beam from the EN. Fortunately by then we had our own beautiful EN and the world expert Ed Berners. Walt, Paul and Bud were also some of the best accelerator people anywhere. John Erskine built an automatic plate scanner at Argonne but he was the only one who could make it work and then only with some batches of Kodak plates. He also pioneered an angle-sensitive resistive-wire detector for the spectrograph. I copied some of his ideas in our new darkroom when we got the 100cm Spectrograph. We tried to adapt his detector to our spectrograph but couldn’t solve the space problem in our focal plane enclosure. I have never liked the “user mode” of data taking and am very grateful that I have always had in-house accelerators.

FN Tandem; 1960-

We had been very productive at Notre Dame with the 4 to 5 MV accelerator while the rest of the world was writing proposals and getting big bucks for new, higher energy machines. In fact Bill Wright of ONR remarked to me and Walt, “You guys thought the boat left every day and there was no hurry in asking for a new machine. Now it may be getting late.” In fact a year or so latter Hesburgh, then on the NSF board, stated that the board had decided not to fund any more Tandems. Fortunately they did fund another, his! (but he learned of it only after the fact). But much happened between these two statements; my first idea (before the first “Tandem” was built), after hearing Ray talk about the “Swindletron” (two accelerations for the price of one), was,. “let’s mount a vertical machine on the roof over the terminal end of the 4 MV, bring the beam down through the roof and inject it into the 4 MV”. There was enough empty tank to install another column and tube from the terminal to the terminal-end of the tank. I wanted to make a tandem out of the 4 MV machine and inject it with a negative beam from a vertical machine. I called HVEC to explore buying a vertical negative machine. I was told that I had it backwards. HVEC was now building “Tandems” and they were horizontal (I believe Ray was a consultant) (friend or foe?). He did get the first U.S. Tandem. As written above it wasn’t too long before I used it. By about 1960 Walt and I were trying to talk to ONR about buying us a new accelerator. We emphasized that the N.D. lab probably had as much collective experience with electrostatic machines as any lab. in the world, but we had not asked for a new machine before because we had a very active program with the present accelerator. We were encouraged to submit a proposal. At this time the nuclear group was divided between the accelerator types (Walt, Bud, Paul, me) and the radioactivity people (Mihelich and Funk). We wrote a proposal for the accelerator with the four accelerator people on it and sent it to ONR and NSF. I remember talking to the NSF Program officer at the Padua conference and having no luck in communicating with him. I felt that he really didn’t know what the situation was. In a year or so I was desperately trying to learn how to run down Bill Rodney (whom I did not yet know) at the Washington meeting. When I succeeded I found I could communicate quite well with Bill, although definite answers were seldom forthcoming. Little did I suspect that I would spend a year and a half working with him at NSF, at his request!

Then President Kennedy was killed. We were told that President Johnson had ordered that all proposals not already acted on were to be declined and thus we would have to start over. Charlie Mullin, then “Chairman” (but acting more like “Head”) of the Department, insisted that the new proposals come from all six experimental nuclear types. His argument of strength in numbers made sense but it was not clear how John and Em were going to use an accelerator. In the event, they did a remarkable job of evolving their research program into “in-beam” experiments. There were heated discussions about whether we should continue to ask for an EN Tandem or go the IU path and ask for a big cyclotron. I remember Paul’s remark that “perhaps a strong D.C. signal will be most effective. It would indicate that we really knew what we needed.” Watching IUCF over the years I have felt that theirs is really a bigger operation than N.D. could have handled. It is interesting to recall how Dan Miller and I had worked in Ray’s lab at the same time. Later he had gotten me an offer from IU. Now we were competing for accelerators, and later he was to become director of IUCF. We did resubmit the FN proposal to NSF. Charlie insisted on writing up a separate proposal, under his name, for the building. I had designed the building, with lots of help from Walt and the others and with the rigid specification from the Dean that the roof be one inch or more below his window sill. But I guess Charlie wanted a piece of the action. Time passed. Bill Rodney was never “in” and never returned calls. There was a strange conversation with someone else at NSF in which I must have given the impression that I had gotten some encouragement from Bill. This “someone” got Bill on the phone and accused him of drinking too much and asked, “what had he told Browne!”. Hesburgh’s statement came in here somewhere. Finally, at a conference, I was talking to a DOE program person and remarked that Bill had suggested that the east and west target rooms were too narrow and that I should change the drawing. I said this seemed to be a total waste of time if we were not going to be funded. The DOE guy said he really didn’t think I would be wasting my time. This was the first positive encouragement I had gotten. One day Bill called and said the proposal was going to the NSF board and that that almost always meant approval! I learned later that the Board established a new policy of asking representatives of schools whose proposals were being acted on, to leave the room. Thus Hesburgh was “thrown out”. I understand he was hurt by this, although I do think it became standard policy. Thus I knew about approval before any N.D. administration people did and it was at least several days before anyone else really believed it. Bill Rodney did stipulate to Charlie that the building proposal be combined with the apparatus proposal and that the nuclear types were the Investigators with me as P1. Our chemists were gracious about having the target room wall right against their former windows. Bill called on December 13,1965 and said the Grant was being made for $2,187,000. The Grant Letter is dated December 17.

Bill came out and we met with Dean Rossini, Executive V.P. Fr. Joyce, V.P. for Business Affairs, Fr. Wilson and others. Rossini wanted to have all deliveries to Nieuwland come through the Chemistry stockroom. I insisted on a Physics loading dock where it now is, pointing out that hundreds of students moved through the halls every hour and we would have a lot of heavy equipment moving to and from the loading dock. Joyce settled the matter with a curt comment to me “you made your point”. He then said, “here are your architects” (we were stuck with Ellerbe), “start the building as soon as the drawings are finished and the contract let”. It has been said that the best form of government is a benevolent dictatorship. This is one instance where I appreciated Notre Dame being governed this way. Fr.Joyce then asked Bill Rodney, “how do we actually get the money? This is the largest research grant the University has ever had”. The question of cost overrun on the building was raised. Bill said, “NSF has granted this amount of money, you have contracted to build this building. Over runs are your problem.” Later I had to remind Fr. Wilson of this. He was miffed with the contractor (Hickey) and thought he let Ellerbe charge us for many of their own mistakes. Hickey was annoyed that I had followed the building so closely and had so often insisted on sticking to specifications. Except that the building was never officially finished (we simply moved in), no major problems remained. A book could be written about the errors of Ellerbe (three foot high walkways, leaking roofs, water dripping on the control console, impassable fire escapes, humidity control that never worked etc., etc. Walt did a great deal of the work, in checking design and drawings and in discussions with the architects. One example; he discovered that the specifications called for some 2000 ft3/min., more air going into the vault than coming out. Walt remarked that, “we really wanted to ventilate the vault, not blow it up like a balloon”. Walt and I went up to Minneapolis to the Ellerbe headquarters in the winter. Construction was started in August 1966 and the exterior finishing completed by May 1967. We began to move in in the summer, as the interior was finished. After the contractors left, Fr. Wilson told me he really wasn’t sure just what the final cost of the building had amounted to.

Anecdotes; During construction Walt and I had a run-in with the plumber union. We had to have some high pressure (2,000 psi) tubing running from the compressor room to the storage tanks in the new basement. We intended to imbed them in the concrete walls. They were not in the building drawings and when the union steward found out from the "Super" that we meant to put them in ourselves, he threatened to strike the whole project. He demanded that we hire specially qualified steam fitters for high pressure work. We finally mollified him by agreeing that the work would be done by Faculty members, not craftsmen, and after hours. So Walt and I did it ourselves (with some student help). We got the contractor to allow us to start on the wiring, wire trays in trenches and stands for magnets, while the finishing touches were being made on the building. One job was to place the large shield blocks which form the wall between the vault and the East target room. The "Super" had gotten a big fork lift. It was easy to get the lower row in but when they came to fitting the upper row in, the thickness of the forks on the lift was enough to prevent the block from clearing the top of the opening. The workmen sweated for hours without success and the "Super" was getting frantic. (Blocks and wall are three foot thick solid concrete). Finally one of our undergraduate helpers said to me that he had spent a summer driving a fork lift, stacking pallets in a Coca-Cola plant, and was sure he could do the job. Believe it or not, I got the Super to turn his back and move his crew out while the attempt was made. Success! The blocks are in place! This was not the only time that my spending hours a day on the site with the workmen, and befriending the "Super", paid off.

With the building in train we then had to deal with the specifications for the accelerator. We wanted a standard EN Tandem for positive operation but we also wanted the unique feature of being able to accelerate electrons. Even though many applications of electron machines had been made at M1T/HVEC, I got the impression that Walt knew a lot more about electron machines than HVEC did. The extra cost for the option of negative polarity, third tube, pumping station and electron gun, was $1 15K. They insisted that one could not use inclined-field tubes with electrons and we never could sway them. They tried to stabilize the negative terminal with a second corona needle system [at an extra cost of $3500]. Walt had to abandon this and use a modulated proton beam going up the low energy tandem tube. During positive operation there was often "loading" by the electron tube and we used the old trick of bleeding hydrogen into the electron tube. There was a lot of discussion about pressurizing the electron tube while running positive but HVEC claimed it would take a very long time to condition the electron tube after pumping it out and switching polarity. Our experience was similar to that with the "4MV" machine, i.e. running negative we normally got about 1 MV lower peak voltage than running positive. Another problem was the electron gun. I have forgotten the details but I think that Walt discarded the HVEC gun completely and designed and built his own.

After Walt became Department Chairman and stopped research we made a trade with Stanford. They gave us two standard inclined field tube sections for our electron beam tubes. In any case when Walt and I finished all the negotiating we signed a contract with HVEC. The P.O. date is 4/11/66 and the total amount, $1,408,950. Looking back I am impressed that this was only four months from the Grant Date: Especially as we also did all the building negotiating at the same time. We also met our classes and continued helping our Graduate Students! Age has surely slowed me down. Of course there was much discussion to assure that the building would fit around the accelerator properly and would have the necessary facilities. A new electric feeder line was run and new transformers installed in the Tandem basement to make us independent of Nieuwland Hall. Problems with Delta vs. Y-connections always arise. For years Tandem power was very reliable. Reliability decreased a great deal when N.D. had to connect to the commercial power company. The building was nearing completion when we had the great good fortune to hire Dr. Ed Berners as a Professional Faculty member. Ed became the laboratory superintendant and surely the most vital person in the operation of the lab. and the design, construction and maintenance of experimental facilities. Ed's thesis work was done with Ray Herb at Wisconsin and was a large part of the development of the National Electostatic Co's line of accelerators. He is surely one of the leading experts in electrostatic accelerators. Jim Kaiser has been Ed's counterpart with electronic and computing facilities and equipment. The skill, loyalty and devotion of Ed and Jim was a critical factor in the success of the laboratory. They worked at all hours, all days and were on call day and night. They are of exceptional character and their friendship over the years is one of the best parts of my memories.

HVEC had some odd ideas about requirements. They insisted that we have a number of long heavy copper rods driven into the ground so "ground" connections could be made in each room or vault. Paul Chagnon pointed out that of course this was just what we didn't want. i.e. ground loops all over the place. It seemed we either put the rods in or they wouldn't install the accelerator. We didn't like the arrangement of the control panel. We had Spectromagnetic chassis for magnet controls and others of our own design and we also didn't like the long straight array of racks. The HVEC engineers were adamant. So one night Ed Berners rearranged all the chassis in the racks to suit his taste. The engineers were furious but they had to admit defeat. But this is getting ahead of the story. At various stages of construction some of us went out to HVEC in Burlington. Ed went four times, I went once for the positive operation test (or twice?) and Walt went (at least once) for the negative operation test. Ed enjoyed fall colors in New England. I remember being asked to sit on a high stool at the mocked-up console and actually run the accelerator for the first time. This gave me a very pleasant feeling!

Building had started in August of 1966 and by fall of 1967 we were installing the beam analyzer and switching magnet. Ed was now taking much of the responsibility. We laid out accelerator center line, beam lines, and magnet positions by surveying and mounting targets on the walls and bench marks in the floors. Ed was a bit doubtful that we could do it, but he and I, essentially alone, placed the two magnets. The switch magnet was hard because it is in the wall and the traveling cranes couldn't be used. The story of the magnets is for another chapter. Their installation was complete by November 1967.

Shipping of the Tandem started about Christmas time of 1967 and trucks carrying large crates began to arrive in January 1968. Ed had been told by the railroad that we could get daily reports on the exact location of the flatcar with the tank. As I remember we were told when it left Burlington, but somewhere in the wilds of Pennsylvania it disappeared! One noontime, on a wild hunch, I asked Cynthia if she would like to ride to Elkhart with me to have a look at the railroad switchyards. This was in February 1968. Sure enough a large yellow tank with "N. D." scrawled on the end covers was sitting on a flatcar in the yard. It was clear that the forward brace on the car was damaged. Sure the railroad would be gentle with it! To our astonishment, just as we turned the car around, the train started to move. We followed it all the way to South Bend and saw it pass the old Union station.

Then it took the railroad a couple of days to get it from downtown out to the N.D. siding. Of course Ed called the riggers and we asked how long it would take to move the tank from the N.D. siding into the building. We were told “about two days, unless there is a wreck on the railroad”. They had a contract with the railroad to immediately move all their needed heavy equipment to the sight of any wreck. This did not seem to be much of a hazard for us but in the event, just as they were swinging the suspended tank from the flatcar to the trailer truck, a call came from the railroad. They put the tank back down and drove the cranes away! Moving and installing the tank in the building proceeded without a hitch, once the railroad was put back together. The move was spectacular and attracted passersby. The Observer ran a picture with the caption “Power Plant gets new boiler”. In the age of riots we were probably much safer than if they had put “Nuclear” in the caption. We all have pictures of the tank easing through the opening in the end wall with a few inches clearance on each side. After the riggers got it mounted on its supports Ed sighted it with the transit and called to the riggers, “now move it left an eighth of an inch”. For a moment they thought he was serious. I think really it showed his respect for their competence.

HVEC sent two “engineers” to guide the installation. We soon discovered that one had never done this before and was a trainee. The other was experienced, and with frequent phone calls to Burlington, guided the assembly smoothly. I don’t remember any real problems through column assembly and spring loading of the column. This was a milestone. Then it was time for tubes. Fortunately the engineers, Ed and I, and surely others were watching when the cover was removed from the crate of the first tube section. Everyone saw that a glass section was shattered. The company did respond by saying they would ship another tube immediately and we would argue about responsibility later. This led to a three or four way legal tussle between the company, the moving company, the insurer, and Notre Dame. At last one of the lawyers found a former HVEC employee who stated that he had seen a carpenter miss with a hammer blow as he was nailing the carton. Ed thinks they hit the side of the tank-port as they removed the tube at disassembly. HVEC claimed they were too broke to pay money but would give us a choice of equipment as recompense. We took a three inch quadrupole. The N.D. lawyer promised me that there would be no cost to N. D. for the litigation. I was naive enough to let him do this alone with me in my office. Then he sent a bill for $5K to N. D. and our business people said they were taking it out of the grant! Nowadays I’m sure this would result in government action. And this guy was a member of the firm that I now use for my private affairs!

The procedure for aligning the tubes called for sighting with the telescope which Ed and I had mounted on the special jig provided on the beam analyzing magnet. The jig fixed the axis of rotation of the telescope at the intersection of the incoming and outgoing beam paths. We sighted the bench mark at the far end of the vault and also a plumb line to the floor bench mark at the the ion source position. Jigs were then put in each end of each tube section and the sections moved so that their center lines lay on the line of sight. The HVEC people used their own Taylor-Hobson telescope (somewhat the worse for wear). Our experience caused Ed and me to be suspicious of this instrument. Probably our bad eyes were a help. We had to refocus the eyepiece for each of us and found this changed the line of sight. The engineers would not listen to our cautions. We came in one evening while they were out on their usual night prowl and Ed realigned the entire accelerating tube. We used all our tricks and decided we really did have the tube axes on a straight line from magnet center point to ion source position. We never told the HVEC people until a beam was achieved and the accelerator passed its tests. This may have cut weeks off installation time. Another crisis occured when the engineers reported that the Company had decided they were not responsible for demonstrating specified beam at specified voltage because we had not bought an HVEC beam analyzing magnet. They said they would not allow the beam line to be connected to our magnet until we accepted the machine. At this point I completely lost my temper. They had been told when we placed the order that this was what we were going to do and had accepted the order under these conditions. As I remember I told them we were going to make the connection ourselves and if they tried to interfere we would have them evicted from Notre Dame and there would be no final payment. They seemed to consider the fact that there were more of us than of them and that they were on our turf. No blows were struck! I have no idea whether we could have done this but the intimidation worked. The atmosphere was very strained while they ran the voltage up to 5 MV as read on the GV and we set the magnetic field to the value calculated for 10 MeV protons going around the radius specified by Spectromagnetic (manufacturers of the Beam Analyzer). When the shutter was removed beam was read at the exit slits! The celebration was unrestrained and all was forgiven. It turned out that we had a good deal of luck. The HVEC calibration of the GV was correct only at 5MV. If we had picked a different voltage we would have had to search for the beam and I imagine there would have been a good deal of finger pointing. As it was we had set the world record for time to install an EN. The prescribed voltage and current tests were run over the specified times.

A few moments after we agreed that the specifications were met and we would accept the machine, there was a big spark, and all the vacuum systems crashed. We quickly saw that tube pressure appeared to be headed to tank pressure. Clearly there was a rupture of the vacuum system inside the tank. Of course the HVEC people wished us luck and left. What to do? How do you leak sniff inside a pressure tank filled with N2 and C02? I recalled Walt telling about having himself sealed inside the 4MV and run up to some pressure while he squirted helium on the tube. No way! Ed and I rigged a set of copper tubes along the length of the inside of the tank with openings spaced from one end of the tank to the other and having exterior connections to a helium bottle. We got the Helium Leak Sniffer going on the tube, ran tank pressure up to where the leak started and then put a short burst of helium in one of the copper tubes. We timed the response of the sniffer and noted the magnitude. Of course you could do this only so often because the helium stayed inside the accelerator tank. It worked. It was pretty clear that the leak was in the terminal. We found that the main gasket on the stripper box lid was made of four separate sections simply laid against each other at the corners. How this had sealed even for a short time is a mystery. A proper new one piece gasket cured the problem for keeps. At least we weren’t headed for Space when our 0-ring leaked!

The accelerator itself has been very reliable and seems to get better with age. It has had very skilled and devoted care and feeding by Ed Berners. One important improvement he made was to bolt the column resistors in their mounts rather than to rely on spring contacts. Ed’s care in checking resistance values each time the tank is opened and thereby maintaining a uniform gradient, contributed to reaching a voltage maximum of 9.7 MV and usually reliable running between 8 and 9 MV. Early replacement of the mercury diffusion pumps with Turbo pumps and the use of refrigerated baffles has made a large difference. We probably had the only EN with the original tubes. These were rebuilt once in 1980 and we have now replaced them in 1994 and also replaced the resistor strings with new improved models. A voltage of 10.7MV on the column was achieved with the addition of some sulfurhexaflouride. A new foil changer in the terminal became essential when we began to accelerate heavy ions. The original 15 foil holder was replaced with a 300 foil exchanger. A second stripper was installed in the high energy tube. Bud Darden built a polarized ion source as part of the original installation and later replaced it with one from Wisconsin. More recently U. of Michigan bought a SNICS source for use on our machine and this has been used for almost all beams except helium.