Early Recollections of the Manhattan Project—Day of Criticality

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All of us realize that the effort, which was so successful on 2 December 1942, was made possible by a much larger group of people than those who were present when nuclear criticality first was achieved by man. Some of us were present because we were actively involved, some because they had peripheral duties related to the experiment, and some because they were curious. Unfortunately, some, who normally would have been present, were away that day because of other circumstances. One of the true tragedies of that era was that we were so obsessed with secrecy and so new at the business that few records were kept; and, most importantly, no photographs were taken because cameras simply were not allowed. I have become convinced that history really is chronicled by those who write about it after the facts and not by the persons who actually were involved because they are usually too busy at such times. It is only after some tens of years, when they read or hear of their experience, that they realize what is being recorded for posterity is at variance with their own recollection, which also may be lacking somewhat.

In 1967, at the 25th anniversary of this event, which was held at the University of Chicago, I listened to all the great people and remarked to Herbert Anderson that the speakers' recollections were not similar to mine. But, of course, their perspective had been from the top looking down. Since I was among the youngest of the group, I was looking forward to the 50th anniversary when I would hold forth with perhaps one to two other "Survivors", and we would rewrite history, set the record straight, and there would be no rebuttals. Now here in 1977, it is ten years later so let me start by giving you my perspective of how it all began.

To me the most remarkable feature of that whole era was the short time between the discovery of nuclear fission and man's first controlled nuclear chain reaction. Recall that James Chadwick discovered the neutron in 1932. Only six years later, during the period August–December, 1938, Otto Hahn and Fritz Strassmann separated barium as a product after neutron bombardment of uranium. Almost immediately upon hearing from Hahn of this finding, Lise Meitner and her nephew, Otto Frisch (who had joined her for a Christmas holiday in Sweden) coined the term "fission" and wrote their article (published in the 11 February 1939 issue of *Nature*). In January, 1939, Frisch returned to Copenhagen and told Niels Bohr of his and Dr. Meitner's concept of fission. Bohr came to the United States and while at Princeton received a wire from Frisch informing him of experimental confirmation of the discovery of Meitner and himself.

I. I. Rabi, who had been at Princeton, returned to New York and related confirmation of fission to Enrico Fermi, who was then at Columbia. Herb Anderson, who was one of Fermi's closest collaborators, recalls that Bohr also told him at Columbia about fission, presumably when he returned from Princeton. Within a few weeks confirmatory experiments had been performed throughout the world. Within six years of the discovery of the neutron particle, that particle had been employed to induce a new phenomenon, nuclear fission. Since that time many new particles have been discovered, yet in a practical way little of importance has resulted from these many findings. Perhaps the use of negative PI mesons to treat certain types of deep-seated carcinomas will soon prove to be feasible, but I am hard pressed to relate any tangible benefits to the general public that have resulted from the discovery of all the new particles that compare with the discovery of the neutron and what has ensued.

When problems were arising in Europe in 1939, several scientists, many of whom were recent arrivals to the

U.S. as a result of Nazi pressures in Europe, perceived the implication of fission for potential military purposes. One of the prime movers in this respect was Leo Szilard who drafted a letter, signed by Albert Einstein, and made a direct appeal through Alexander Sachs to President Roosevelt. This launched the *Manhattan Project*.

But how does one start a project of this potential magnitude in a field where there is no established technology base or literature, or even workers in the field? The government went to that segment of our society, which at the time was wholly responsible for basic research, namely, the academic community. The field of nuclear physics was very limited in scope then; the California group centered around Ernest Lawrence at Berkeley, who was really involved in building accelerators, the Chicago school of Arthur Compton, which comprised the cosmic ray investigators, and the Columbia group of Dunning, Pegram, Fermi, and others. Clearly these few individuals and groups could not handle such a project by themselves, so they conceived a very sensible procedure to assemble a work force with the necessary background—they called their friends. At each of these institutions graduate students had been trained who then were professors, located all over the U.S.A. These professors in turn had trained students and had students in training. To begin, specific technical problems were farmed out to various individual researchers at different campuses. As soon as it was decided to form the Metallurgical Laboratory, however, Compton's former students (now professors) and their students were "drafted" by chain telephone calls to come to Chicago to form the nucleus for the project. Had all the federal bureaucracy, and laws requiring advertising, etc., been operative, the project could never have started. The leaders knew where the talent was, and they recruited it in the quickest, simplest manner.

Those physicists who had gone into management became managers; those who had stayed in research took over specific research responsibilities; those who were in electronics became the electronics experts, etc. Although the approach was simplistic, it worked. Perhaps the easiest way to describe the situation is to relate simply my own experience.

I was a senior undergraduate student at the University of Denver, had taken courses in physics under Dr. Joyce Stearns, who had obtained his degree in cosmic ray research under Compton, and had set up the high altitude observatory on top of Mt. Evans in Colorado. Shortly after the Japanese attack on Pearl Harbor occurred, Stearns convinced me that I should go to Chicago where he had been making frequent trips. From the very beginning, it was clear that Fermi was the leader at Chicago, although Compton was the boss and R. L. Doan was the Director. Stearns, who was recruiting for Compton, placed me in Fermi's group to work for Herb Anderson. I had never worked with anyone as brilliant as Fermi and Anderson, so I was completely dazzled by the joy of working in science with them. Having been at a small school where, in science, there was a professor in mathematics, a professor in physics, and a professor in chemistry, I thought all professors were created equal. At Chicago, in the field of what we might today call neutron physics, however, all the professors were acting as if they were students under *the professor,* namely, Fermi. Many of the professors were not much better off than I; we all were reading Rassetti's book on radioactivity, learning about half-lives, mean-lives, and all the rest. Fortunately, since Stearns had been working on cosmic rays, I knew a little about Geiger counters and counting circuits, and, compared with some of the people present, I was almost a professional nuclear physicist by the standards then.

It is hard to comprehend how ignorant everyone was with regard to the fission process. It was known that neutrons could interact with uranium and sometimes cause fission, sometimes be captured and subsequently produce plutonium, and sometimes do "something else." Fermi's work in Italy had led to the discovery that "slow" neutrons were more effective than "fast" neutrons in the production of nuclear reactions in most elements; and it was known that neutrons produced in fission were relatively fast. Thus, the objective was to minimize the interaction of neutrons with uranium until they had been sufficiently slowed, so that the probability for fission was increased. The concept was to have a matrix in which globs of uranium were embedded in a "slowing down" material, but it was important that this have a nuclear mass close to that of the neutron. If the material had the same mass, such as hydrogen−1, then it would slow the neutrons best, but hydrogen −1 also captured neutrons, so that was not attractive. Deuterium would be very good, but in those days heavy water just was not available in the quantities required. So the matrix material decided upon was graphite, chosen independently by Fermi and Szilard. Graphite has a fairly low atomic number, is easy to handle, and, if pure, has low absorption properties for neutrons.

The first significant experiment was conducted at Columbia under Fermi's direction. The "pile" at Columbia was about eight feet on a side and literally was covered with galvanized sheet iron. Albert Wattenberg recalls it as being smaller, but it seemed enormous to me. The problem was that the available uranium was not as pure as stated and neither was the graphite. But Fermi and his workers did not find that out until their pile had been constructed. The term "pile" was coined at that time.

The physicists worried that the nitrogen in the air was "gobbling up" the neutrons; therefore, the pile had been literally "canned," and pumps around the perimeter were used to evacuate the air from the pile. Since removing the air did not help, the "can" was filled with a hydrogenous material to effect better slowing of the neutrons. It was realized that it was not much of a job to fill the can with a butane/propane mixture, but the question arose—how can it be removed safely? Consider that here at Columbia University was potentially the world's first nuclear reactor, with no guarantee as to what would occur if criticality were to be obtained, but no one openly worried about that aspect! The overriding concern was the potential for a gas-air explosion when the propane was removed from the reactor after the experiment had been attempted. Since the decision was made not to risk an explosion, the whole team was moved to Chicago to conduct the project.

The object was to build a structure containing uranium that would multiply neutrons. This goal clearly was possible because shortly after fission was discovered, it was shown that more than one neutron was released in the process, so a multiplying assembly was clearly feasible. To work with neutrons, however, it was necessary to have a source of neutrons. Fermi had produced neutron sources using alpha particles bombarding beryllium (Be). The alpha particles were obtained usually from 3.8-day radon, but when funds were available, a steady source using radium (Ra) and Be was preferred. So we all worked primarily with Ra-Be neutron sources. The source strength was specified in terms of milligrams or grams of radium in them.

It may come as a shock now, but in those early days there was essentially no such "activity" as radiation monitoring for health safety purposes. Our experimental work consisted mainly of activating foils, mostly indium, with neutrons, then wrapping the indium foils around thin-walled Geiger counters and detecting the decay particles. I have wondered in recent years how much beta radiation our hands were exposed to from handling the neutron-activated foils day after day. The counters used were thin-walled glass Geiger tubes shielded in cylindrical lead cylinders. One experiment employed a 0.25 gram Ra-Be source encapsulated in a brass cylinder about one-half inch in diameter and one-half inch high. Every 15 minutes I took the source out of its lead container (no shield for the neutrons) and placed it in a paraffin and wood box about 18 inches on a side. I then would place an indium foil a few inches from the source, leave it for a few minutes, remove it, put in another foil, perhaps covered with a layer of cadmium (Cd), and repeat the process. The paraffin box was completely unshielded. I moved the source with a string tied to it. I carried the capsule at arm's length, but never did I have any idea as to the total body dose I received. After a month or so of this experiment, I went to the Indiana dunes on a picnic and went for a swim. The next day it was obvious that somehow the sun had complemented the radiation I had received, and all skin that had been exposed to the sun had developed small, bright, red spots about one to two millimeters in diameter. The point of this story is that in those days we had no quantitative idea as to the amount of radiation one received. There were tasks to be accomplished that involved radiation, and one did them as quickly as possible.

Although there had been small graphite structures, and several water tanks in which critical parameters were being determined, it was not until the move from Columbia that the big pile was started in the large double squash court under Stagg field (the abandoned football stadium-Ed.). Calculations indicated that a self-sustaining assembly could be constructed in the volume available in the squash court. Unfortunately, the materials available were not of uniform quality or in ample supply. Since a sphere has the optimal volume-to-surface ratio, the best materials were to be placed in the center of the pseudosphere. Although the structure would resemble a cube externally, the objective was to have the best materials in the center in the approximate geometry of a sphere.

In any event the bottom and initial structure was constructed of regular wooden 4 × 4's. Because of the worry about the absorption of neutrons by air, Fermi had decided that the whole structure should be built inside a balloon. Herb Anderson had contracted with Goodyear to build a cubical balloon with one side that opened so that the building could take place in a manner not too dissimilar from piling blocks inside a tent with the tent flap open. The basic building blocks were "logs" of graphite 4 in. by 4 in. by about 3 feet, and wooden timbers. One slid the graphite logs on top of each other, similar to sliding a puck on ice, or in shuffle board. You could tell who was involved in the pile work by how dirty their hands and fingernails were. The dirty hands did not bother me as much as did my dirty knees. With Grant Koontz I had the job of installing the vacuum system to evacuate the pile using the blimp bladder, so I actually only stacked graphite for about a week. Although I was able to get my hands clean eventually, graphite, which had become imbedded in my pants, kept making my knees black. For months I had black pores on my knees. Only when I threw the pants away did the phenomenon cease.

Understandably, one of the main items of concern and conversation was what were the Nazi's "up to." Many of our "team" had come from Europe, and since fission had been discovered there, it was natural for us to be concerned that their efforts might be ahead of ours. Usually, those working with Fermi ate lunch together at the University Commons at one big table. From time to time one of us conceived of ideas to thwart the Germans. I somehow believe that the raid to destroy the Norwegian heavy-water plants was initiated at lunch at the Chicago Commons. It is hard to try to project the concern we had with regard to the war in Europe, which was not going well for the Allies in 1942. In that year alone we lost 900 ships to the U boats.

The neutron sources were items of critical importance to the experimental work. These ranged in size from a few tens of milligrams to a gram of radium, mixed with finely powdered beryllium. Since a "point source" always was assumed in calculations, Herb Anderson decided we would try to compact the physical size of the source. He designed a small screw press made of steel that could be taken apart and put in a suitcase. It was a beautiful piece of workmanship with a hardened steel die that Tom O'Donnell had made for us. Herb had me get some Be metal and file it into fine powder. We then picked up our gear and flew to New York.

The next morning we went to a place that was a principal supplier of radium. It was a chemistry laboratory in a loft of an old building with all windows open (since this was in June or July). There were a few hoods with vent stacks that went through the ceiling and stopped at the roof, providing ideal conditions for the exhaust to come right back through the window.

We had prepared a small brass cylinder with about a one-millimeter wall and a screw top to screw into the cylinder when the pressed source had been placed inside it. The correct amount of Be was measured into a small porcelain evaporating dish, and the solution of radium chloride was poured over the Be powder. We placed the evaporating dish on a small electric hot plate, stirred it with a glass rod until the water evaporated—all of this in an open hood! Anyway, it was *only* a gram of radium that we believed had been milked of its daughters so that the radiation should not have been too much of a problem. The background in that laboratory, however, "must really have been something." After the powder had been dried thoroughly, we scraped it into our die, pressed the powder into a pellet, used tweezers to place the pellet in the brass cylinder, screwed on the top, and soldered it tightly (we hoped). We packed up our gear, put the source in my briefcase, and went to LaGuardia airport. "I placed my briefcase containing the one-gram Ra-Be neutron source under my seat on the flight back to Chicago. Placing it beneath the seat *ahead* of me, as is done today, greatly would have reduced my exposure to radiation!"

In Chicago we checked the source for leaks by wrapping it with Kleenex and checked the Kleenex for radium daughter activity, and fortunately it was tight. In a few days, as the daughters grew back, we had a fine, compact, neutron source. This, I believe, was the first time that a pressed source had been made. After that task was accomplished, Al Wattenberg got the duty, and I believe he became the expert thereafter. I mention this incident to indicate the way things were done—very directly, no fanfare.

Meanwhile, construction of the pile was going ahead; in addition to machining and cutting the graphite and wood, uranium oxide was being pressed into small cylinders, which then were inserted into cavities that had been made in the graphite logs. The objective was to place the best materials in the center, but since it was not known how big the pile would have to be, the position of the center really was not known.

Although we had been working with U_3O_8, uranium (U) metal was desired very much. When the first uranium was received, many of the cylinders of the sintered metal actually burst into flames when the boxes were opened. A large number of the cylinders resembled glowing clinkers from a coal furnace. So each time a batch was received, it was a traumatic experience to open the containers, to set aside those which resembled small volcanic eruptions, and to place the acceptable stable ones into their graphite nests.

After the stacking had proceeded for awhile, a boron trifluoride counter was introduced into the assembly and employed to measure the multiplication of a source of neutrons. The normal cadmium-neutron absorber control mechanism was backed up with a Cd scram system as well as with liquid solutions of Cd salts, which Sam Allison and others were prepared to dump on the assembly by cutting a rope with an ax. All of the activities of that day in 1942 have been related many times in many articles. I must say that Fermi's decision to go to lunch just before they reached criticality flabbergasted me more than anything else. I would like to quote Herb Anderson's recollection of that time, from Jane Wilson's book *All in Our Time*.

"On Monday, November 16, we opened the rubberized balloon cloth envelope and started erection of the pile inside it. We organized into two shifts: Wally Zinn took the day shift, mine was the night shift."

The frame supporting the pile was made of wooden timbers. Gus Knuth, the millwright, would be called in, and we would show him by gestures what we wanted. He would take a few measurements, and soon the timbers would be in place. There were no detailed plans or blueprints for the frame or the pile. Each day we would report on the progress of the construction to Fermi, usually in his office in Eckhart Hall. There we would present our sketch of the layers we had assembled and indicate what we thought could be added on the following shifts. Since some of the graphite was of better quality than the rest, it was important to arrange its disposition carefully. Fermi spent a good deal of time calculating the most effective location for the various grades of graphite on hand.

A particularly difficult point was where to put the uranium oxide and where the uranium metal. We knew that because of its higher neutron reproduction factor, the metal should be in the central part of the pile, but we had to decide at what layer to begin to install. After the construction was well underway a substantial amount of uranium metal of high quality arrived from Frank Spedding's group in Ames, Iowa. The plan was changed immediately to take advantage of the improvement this would give. We finished with a metal core, neither spherical nor central, but it did not matter.

The details for construction of the pile were determined day-by-day at those meetings in Fermi's office. One important consideration was the location of the cadmium control strips. These were needed to absorb neutrons to keep the pile from becoming too reactive once it began to approach the critical size. We wanted a number of control rods distributed widely in the structure. This meant that some had to be installed at a rather early stage. A simple design was developed for a control rod, a cadmium sheet nailed to a flat wood strip was inserted in a slot machined in the graphite for this purpose. The strips had to be inserted and removed by hand. Except when the reactivity of the pile was being measured, they were kept locked inside the pile with a simple hasp and padlock, the only keys to which were kept by Zinn and myself. One special, particularly simple control rod was built by Zinn; it operated by gravity through weights and a pulley and was called "Zip." It was to be pulled out before the pile went into operation and held by hand (Zinn's) with a rope. In case of an emergency, or if Zinn collapsed, the rope would be released, and Zip would be drawn into the pile by gravity.

Once the 15th layer had been reached, we introduced the practice of measuring the neutron activity at a fixed point in the structure. This was done with a boron trifluoride counter at the end of each shift, once the construction quota had been filled. Each day the measurements of the activity of this counter were reported to Fermi who used it to improve his estimate of how much bigger the pile would have to be. Thus, we always had a good idea of how much more we had to do.

As the pile grew, the estimate of its critical size became increasingly accurate. Thus, we could tell that on the night between December 1st and 2nd, 1942, during my shift, the 57th layer would be completed and the pile could be made critical. That night the construction proceeded as usual with all cadmium control rods in place. When the 57th layer was completed, I called a halt to the work in accordance with the agreement we had reached in the meeting with Fermi that afternoon. All the cadmium rods but one were then removed and the neutron count taken, following the standard procedure that had been followed on the previous days. It was clear from the count that once the only remaining cadmium rod was removed, the pile would go critical. It was a great temptation for me partially to withdraw the final cadmium strip and to be the first to make a pile chain react. *But Fermi had anticipated this possibility.* He had made me promise that I would make the measurement, record the result, insert all cadmium rods, lock them all in place, go to bed, and nothing more. The next morning, 2 December 1942, as Wally Zinn remembers

> "It was a very cold day. To those of us who worked in the West Stands (of the stadium—Ed.), cold was not a new experience. That gloomy structure with its high stacks of graphite bars filling all corridors, stairwells, and wherever 500 tons of the black stuff could be stored was completely unheated. Perhaps the importance of our jobs had something to do with it, but we really worked fast to keep warm. To help, we tried charcoal fires in empty oil drums—too much smoke. Then we secured a number of ornamental, imitation log, gas-fired fireplaces. These were hooked up to the gas mains, but they gobbled up the oxygen and replaced it with fumes which burned the eyes. The scientists and technicians could use physical activity to keep warm, but the security guards had to stand in one place at the entrances. The University of Chicago came to the rescue. Years before, big league football had been banned from the campus; we found in an old locker a supply of raccoon fur coats. Thus, for a time we had the best dressed collegiate-style guards in the business."

Fermi had prepared a routine for the approach to criticality. The last cadmium rod, attended by George Weil, was pulled out step by step. At each step a measurement was made of the increase in the neutron activity, and Fermi checked the result with his prediction, based on the previous step. That day his little six-inch pocket slide rule was busy for this purpose. At each step he was able to improve his prediction for the following. The process converged rapidly, and he could make predictions with increased accuracy. When he arrived at the last step, Fermi was quite certain that he could make the pile go critical.

When the cadmium control rod was pulled out to the position he asked for next, the increase in neutron intensity was noticeably quickened. At first you could hear the sound of the neutron counter, clickety-clack, clickety-clack. Then the clicks came more and more rapidly, and after a while they began to merge into a roar; the counter could not follow anymore. That was the moment to switch to a chart recorder. But when the switch was made, everyone watched in sudden silence the mounting deflection of the recorder's pen. It was an awesome silence! Everyone realized the significance of that switch; we were in the high intensity region, and the counters were unable to cope with the situation anymore. Again and again, the scale of the recorder had to be changed to accommodate the neutron intensity which was increasing more and more rapidly. Suddenly Fermi raised his hand: "The pile has gone critical," he announced. No one present had any doubt about it! Then everyone began to wonder why he didn't shut the pile off. But Fermi was completely calm. He waited another minute, then another, and then when it seemed that the anxiety was too much to bear, he ordered, "Zip in!" Zinn released his rope and there was a sigh of relief when the intensity dropped abruptly and obediently to a more modest level. It was a dramatic demonstration that a nuclear chain reaction was controlled.

No cheer went up, but everyone had a sense of excitement. They had witnessed a great moment in history! Wigner was prepared with a bottle of Chianti wine to celebrate the occasion. We drank from paper cups and then began to say things to one another. But there were no words that could express adequately just what we felt.

Only 43 persons were present at the experiment; they were mostly the scientists who had done the work. But there was also Crawford Greenewalt of the du Pont Company. His judgment would be critical.For him the demonstration was impressive; it was this performance that convinced him that the du Pont Company should build the plutonium production piles.

Here we are today with nuclear energy a fact. Last winter when coal piles were frozen solid, rivers were frozen so that the movements of oil barges were blocked, with natural gas supplies overtaxed, it was nuclear power that shouldered the load. In January in Connecticut nuclear power supplied 50% of the electric load. Chicago and northern Illinois received 45% of its electricity from nuclear reactors. The Zion plant ran 109 consecutive days at 100% full power, 1100 megawatts. New York City received 32% of its power from nuclear plants. Duke power in the south provided 30% of its load from nuclear reactors.

Considering the world's population today and that which is projected for the next century, and coupling this with expectations of an improvement in the standard of living, there is no question that if these expectations are to be realized, plentiful and inexpensive energy must be made available. Clearly nuclear power must play a major role in our lives. If expectations are not to be realized, then the world as we would like it to be, never will be. Strife will continue between the developed nations, who more and more are becoming dependent upon the underdeveloped nations for raw materials, and the underdeveloped peoples whose expectations will never be realized fast enough to satisfy them.

The nuclear pioneers have provided one means of attaining a better life for all mankind. We can only hope that the politicians will have the wisdom to utilize fully the benefits that are available to all of us in the nucleus.

Those present on December 2nd, 1942, when criticality was reached were: H. M. Agnew, S. K. Allison, H. L. Anderson, H. M. Barton, T. Brill, R. F. Cristy, A. H. Compton, E. Fermi, R. J. Fox, S. A. Fox, D. K. Froman, C. C. Gamertsfelder, A. C. Graves, C. H. Greenewalt, N. Hilberry, D. L. Hill, W. H. Hinch, W. R. Kanne, P. G. Koontz, H. E. Kubitschek, H. V. Lichtenberger, G. Miller, G. Monk, Jr., H. W. Newson, R. G. Nobles, W. E. Nyer, W. P. Overbeck, H. J. Parsons, G. S. Pawlicki, L. Sayvetz, L. Seren, L. A. Slotin, F. H. Spedding, W. J. Sturm, L. Szilard, A. Wattenberg, R. J. Watts, G. L. Weil, E. P. Wigner, M. H. Wilkening, V. C. Wilson, L. Woods, and W. H. Zinn.

[Little did I realize as a young Army Air Force Cadet, stationed at the University of Chicago for special training, that I ran a contrived obstacle course on Stagg field and stadium daily, directly over this tremendous historical development. It was well after World War II that I became cognizant that those "professor types" with their briefcases whom I had seen hurrying under the stands (toward the areas with the heavily padlocked doors) were famous scientists, now part of our history.—Ed.]