Those early days as we remember them

From the Metallurgical Laboratory to Argonne National Laboratory

Argonne traces its birth from Enrico Fermi's secret charge — the Manhattan Project — to create the world's first self-sustaining nuclear reaction. Code-named the "Metallurgical Lab", the Manhattan Project team constructed Chicago Pile-1, which achieved criticality on December 2, 1942, underneath the University of Chicago's Stagg football field stands. Because the ensuing research experiments were deemed too dangerous to conduct in a major city, the operations were moved to "Site A" in a forest preserve near Palos Hills and eventually renamed "Argonne" after the surrounding forest.

On July 1, 1946, the laboratory was formally chartered as Argonne National Laboratory to conduct "cooperative research in nucleonics." At the request of the U.S. Atomic Energy Commission, it began developing nuclear reactors for the nation's peaceful nuclear energy program. In the late 1940s and early 1950s, the laboratory moved to a larger location in Lemont, Illinois, and established a remote location in Idaho, called "Argonne-West," to conduct further nuclear research.

In 1971, to celebrate the 25th anniversary of Argonne's charter, the *Argonne News* published a series of recollections titled "Those early days as we remember them" by former or current staff members who had worked at the Metallurgical Lab and participated in the birth of Argonne. Many shared unique memories of this special time that can't be found in history books. Below is a listing of the authors and the date their stories were published in *Argonne News*:

January 1971 — Austin M. Brues, Nora L. Watson, Mary L. Erickson, Ace L. Singleton, Mary K. Walsh

February-March 1971 — Lester C. Furney, Arthur H. JaHey, Leonard Bogorad

April 1971 — James F. Schumar, John L. Armstrong, David E. Walker, J. Howard Kittel, George O'Keefe, Maurice D. Odie

May 1971 — William P. Norris (from a tape recording made in 1965)

June 1971 — Farrington Daniels

July-August 1971 — Lester C. Furney

September 1971 — Norman Hilberry

November 1971 — Elmer W. Rylander

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Those early days as we remember them

Austin M. Brues
Biological and Medical Research Division

I would like to start my story a few months before I joined the Metallurgical Laboratory in the fall of 1944. I was engaged in some wartime research on battlefield shock, and was involved in some therapy and research with P^{32}; in the course of which I was accustomed to carrying tubes of it between M.I.T. and my laboratory in a shirt pocket.

I became aware of a hush-hush project in Chicago which was recruiting an odd bag of people: a nuclear physicist, a couple of radiologists, a pathologist, a radiation botanist and finally, a biochemist who was my colleague in a little study on DNA. I confided to the latter that I was beginning to put 235 and 235 together — and before long I found myself under some pressure to move to Chicago: as I have suspected, so that my thoughts could be given the proper security classification.

The scene of my Chicago career was called Site B, a building south of the Midway with an ancient, imposing brick facade. It had lately been a brewery (hence the B?) connected by a single passage, which resembled the door to Rhamses' tomb, with a wooden labyrinth in the rear that had been a riding stable. The front part was occupied by Metallurgy proper and contained massive equipment which rolled, pounded, and thundered away while a bevy of young ladies in a room directly above attempted to see — let alone count — blood cells under the microscope.

Far in the rear was a sort of broom closet with a hood and a lot of lead bricks crammed into it. This was a hot lab. On the wall was a neatly printed sign which read:

IN CASE OF A SPILL

1. Hold your breath
2. Leave the room promptly
3. Report to your supervisor
   [and at the bottom, in pencil, someone had written:]
4. Take a breath

We had an animal farm with a remarkable number of small animals, mostly mice. This section of Site B was air-conditioned with a variety of equipment, and at rather frequent intervals — mostly at night — a valve would stick in a remote part of the building so that the radiators would fail to shut off and we would have to move all the animals out into the corridors until the valve was unstuck.

Our activities were coordinated with some at a place in Tennessee known as Site X, a half-hour drive from a milk siding on the Louisville and Nashville railroad. It was felt important to keep secret the relation between this place and Chicago. This was not entirely easy. Mice which were being exposed to neutrons there also had to be exposed to X-rays.

However, Army procurement saw no sense in buying an X-ray machine for "X" if there already was one in Chicago; while Army security forbade ordering the same things at the same time for the two laboratories. Thus we spent the wartime years buying mice every fortnight and then reshipping half of them to Oak Ridge by a truck that brought us back fission products.

All in all, those days were exciting and full of purpose, even though a lot of people saw no reason for a biology section in the Metallurgical Laboratory just because massive quantities of radioactivity were being manufactured. When enough plutonium had been manufactured so that there was enough to spare to measure in dogs and goats, one of the younger dogs slipped out of Site B past the guard who was posted to control the movements of people and packages. The runaway was pursued over several blocks of Woodlawn by an assortment of people in white coats and finally was cornered and returned to his cage.

And finally, to complete this attempt to give a little of the flavor of those days, I will mention the couple I saw, lying in the grass of the Midway as I returned to Site B from one of the nocturnal mousecooling expeditions. And they were doing what? Under the light of a street lamp, playing a game of chess.

Nora L. Watson
Materials Handling Department

I worked as a clerk at the Armory, 53rd street and Cottage Grove in Chicago. My job was to issue office supplies. Friends often would ask, "What do they make where you work?" I never knew. There was no assembly line, nor any sign of defense activity.

But one day the significance of our work was made clear. We were asked to assemble in the main room of the Armory. As we gathered, knowing only that some "brass" were present and some University of Chicago officials, we wondered what this was all about. The place of assembly, usually used for horses, was large but, well, not exactly beautifully kept up.

A truck had been brought to the center of the room. The officials were standing on the platform bed of the truck. Dr. A. H. Compton, first Director of the Metallurgical Laboratory, was a main speaker and I remember vividly a part of his speech. The day was August 6, 1945. "We have been successful in completing the race before Germany," he said, "in being first to produce the atomic bomb." Then, his voice never faltering, Dr. Compton gave great praise; to the scientists of course, but also to everyone who had been involved, "even the person who passed out stationery and pencils for use at the Laboratory," were words he used.

I felt extremely proud. I felt I had made a definite contribution. However small, it had helped make history.

Mary L. Erickson
Central Shops

I was a Site A-er. I started on the hill in September of 1946. After bring stopped at the lower lodge by Guard Ross Chilvers (deceased), it was to me slightly frighten ing to go up the narrow, winding road to a world unknown to the local residents. A few late summer flowers were in bloom around the base of the flag pole in the yard just outside the main office building. I entered the building to go down a long, dark, drab hall past time clocks to enter the small, approximately 12 x 12, office occupied by Larry Reed (deceased), foreman of the machine shop, and Gus Knuth (deceased), foreman of the carpenter gang. This was to be my home for the next couple of years.

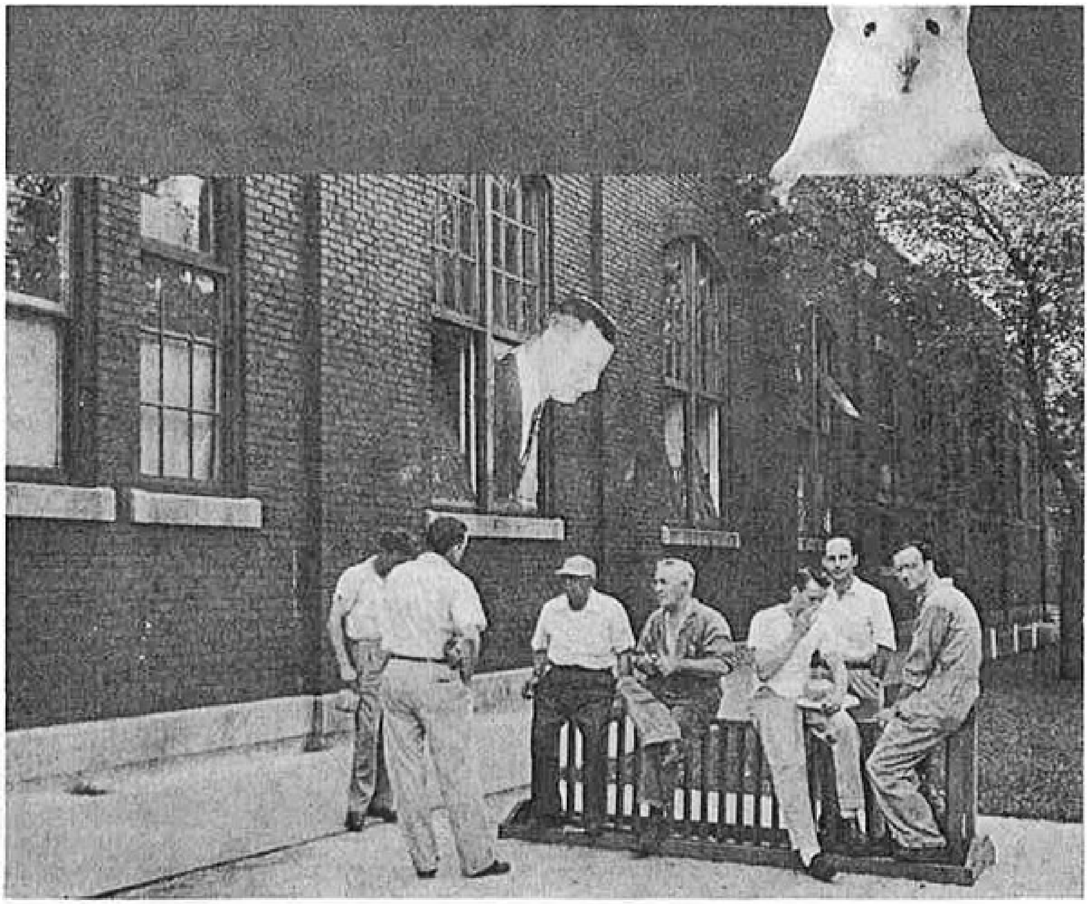
I vividly remember Christmas. The smell that hit you as you entered the building came from the chemistry lab and was nothing other than a huge bucket of glug that cooked for two or three days, it seems to me, in preparation for the day before Christmas party.

Springtime at Site A was pure beauty. The roadway from Archer Avenue all the way to the site was lined with wild crab apple trees in blossom. The ground was literally covered with both purple and dog-tooth violets. And one could find jack-inthe-pulpits and buttercups with very little effort.

A summer recollection is Dr. Nathan Sugarman running wildly down the hall in his shorts — at that time rather strange attire.

There were buses that loaded just outside the office area, and a constant late arrival was our nurse, Ruth Rottman. She seemed to think that since she was in no hurry to get home, no one else was either. The cafeteria was a wooden building where everyone managed to meet midmornings and afternoons for some of Bess Altman's and May Simons' (deceased) goodies. Often at lunch we were able to order the kind of pie we would like for our three o'clock break. Then, come Friday it was clean-out-the-refrigerator day, more commonly known as smorgasbord. Anyway, we ate everything left from the rest of the week.

It was in the cafeteria that we heard the announcement. Standing atop a Coke box that (it seems to me) wasn't too stable, Dr. Zinn told us about the purchase of the present DuPage site. This meant progress, but also change, and we received it with mixed feelings. It heralded the end of Site A, a place and a time which would be remembered always with affection.



Men of Bio-Med and Metallurgy, gathered for lunch at the bicycle rack outside Site B under the (contrived) gaze of Dr. Austin Brues and presided over by a member of the omnipresent mouse colony. The photo and embellishments were probably the cooperative caper of Jane Glaser and Atlee Tracy, long-time photographers in BIM.

Ace L. Singleton
Personnel Division

I first worked for the Metallurgical Laboratory at New Chern but as a member of the Reclamation Department, I was one of the team of experts assigned to the restoration of West Stands, Site B, New Chern, and in 1954-56 Site A. In this work we were required to wear two pairs of coveralls, three pairs of gloves, hard hats, safety shoes and shoe covers, and to breathe through Scott supplied air masks. At Site A we dismantled CP-2 (which in turn had been built of the dismantled and moved CP-1), removing among other materials 282 tons of graphite, a dirty material like soft coal. All but 72 tons, which were disposed of as dry active waste, was vacuumed, washed, stacked on pallets, and banded, then used at Site D to construct the Argonaut reactor, a subcritical assembly used for instruction in the International Institute of Nuclear Science and Engineering.

I was glad when it was over, had developed a thorough distaste for graphite and thought I'd seen the last of it. But not quite.

In 1967 as a guest at the first 25-year Service Award dinner, I received two important mementos. One was a service award pin set with a diamond. The other was — yes — a small bar of graphite. Quite innocently, it was a potent reminder of a trying time in my Reclamation career. However, handling this piece was no problem. It was encased in lucite and inscribed: "Graphite from CP-1, the first nuclear reactor, December 2, 1942, Stagg Field, The University of Chicago."

Mary K. Walsh
Chemistry Division

In memory, our work in the infant atomic energy field during the middle and late 1940s, Hyde Park, and The

University of Chicago "on the south side of Chicago", as that popular song of a few years ago put it, seem all intertwined.

For those of us who could, to walk to work in a big city was a rare privilege: one got one's exercise automatically, saved money, and avoided the sardine syndrome of public transportation. Thoughts of the 20 minutes it took me, on bright, leafpopping spring mornings, or on crispy, cloudy fall mornings, evoke feelings of well-being and peace still.

Then there were spring and summer lunches, in the shade of a nice tree in Washington Park or on the university grounds. And the walk south on Ellis Avenue from 56th Street with its vista of historic West Stands and its ancient-looking Gothic towers. And 55th Street's myriad collection of stores, now long gone, replaced by the phenomenon known as Urban Renewal.

To walk across the Midway to 63rd Street — a city in itself — is another whole chapter of memory. To see the students headed for their various destinations and the great buildings of the university was to feel a thrill in being connected, however modestly, with it all. One wished destiny had arranged that one could have been a student there.

Enough for the externals. Our group, the Spectrographic Laboratory, had a four-room "suite" in New Chemistry. The building had been erected at the beginning of the Manhattan Project. It was a far cry from the whole wing and a dozen or so rooms we have now. We had an instrument room where the spectrographs were housed, a small office, a small room for special laboratory work, and a general sample preparation room. The latter had an Lshaped arrangement of tables in the center, accommodating our analytical balances as well as a space for grinding samples. These were mostly uranium oxide, beryllium oxide, thorium oxide, zirconium oxide, and aluminum oxide. Along one wall was a small darkroom and two hoods where we prepared samples. Along another wall was a sink and a bench for drying glassware. Benches for miscellaneous other work lined the remaining two walls. On one was a furnace for heating samples, and on top of it was an old-fashioned oscillating fan which we kept going full speed in summer. The office had a big old-fashioned radiator; I remember one warm Indian Summer day when it was going furiously. We also had a small room down the hall, known as a densitometer room, with a few more instruments.

When we moved to DuPage and the new Argonne, a special weighing room seemed a mixed blessing. It was great to be able to keep the analytical balances away from acid fumes; but to go even next door to the sample preparation room seemed a far piece to walk.

Time marches on, but it's still a thrill to have been a part of those days and to have seen the many advances the Laboratory has been responsible for. I'm sure I'm not alone in these feelings.

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Argonne's early history

as written by Lester C. Furney for Argonne's first Employee Handbook, published in October 1951. Mr. Furney at the time was Staff Assistant to Walter H. Zinn, Laboratory Director. He accompanied Dr. Zinn in 1956 when the latter resigned from Argonne to found the General Nuclear Engineering Corporation at Dunedin, Florida. Mr. Furney is now a resident of Clearwater, Florida.

This plaque was unveiled December 2, 1947, during the Fifth Anniversary reunion of those who participated in the event it commemorates. Officiating at West Stands were (l to r) R.F. Bacher, member of the new USAEC; Farrington Daniels, Director in 1945-46 of the Metallurgical Laboratory; Walter Zinn, Director of Argonne National Laboratory 1946- 56; Enrico Fermi, director of the experimental program which culminated in the event the plaque speaks of; and Robert M. Hutchins, then Chancellor of The University of Chicago.

Argonne National Laboratory was originally organized as the Metallurgical Laboratory of The University of Chicago in January, 1942 under a contract between the University and the Office of Scientific Research and Development of the United States Government (OSRD). Its operations were housed in various University buildings and certain temporary structures which were erected on University property. By the end of March, 1942 there was a total of about 150 individuals on the Laboratory's payroll. Of these, over ten percent are on the payroll of the Laboratory as of the end of March, 1951 and the majority of the others are still associated with the present Atomic Energy Commission activities at other laboratories or in various active consultative capacities. In its operations under the OSRD, the Laboratory carried out an experimental program which culminated on December 2, 1942, in a historic achievement — the establishment of the first self-sustaining nuclear chain reaction together with the equally significant demonstration that the accompanying release of nuclear energy could be controlled positively and simply. This first reactor was moved early in 1943 to the original Argonne Laboratory at the Palos Park site.

In this same period the Laboratory selected the site for the Clinton Laboratories (now Oak Ridge National Laboratory) and in cooperation with the E.I. du Pont de Nemours & Company designed its facilities. Included were the reactor to produce significant experimental quantities of plutonium, the new synthetic fissionable element, and the chemical separation plant designed to recover the plutonium produced in the pile and to serve as a pilot plant for the future Hanford production process. Simultaneously, again in cooperation with du Pont, the Laboratory developed the basic design for the Hanford production reactors. It also carried out essential chemical studies on the new elements, neptunium and plutonium, which existed only in submicroscopic quantities and developed a tentative chemical separation process for the Hanford production plant. The Laboratory was also primarily responsible for the development of supplies of metallic uranium and of graphite of the extreme purity required for use in nuclear reactors.

By May, 1942 it became clear that there was a real prospect for success, both in the production of plutonium and in its chemical separation. Such an achievement would inevitably involve the design and construction of major production facilities. Since the OSRD was not authorized to administer production activities, the Government assigned this responsibility to the Corps of Engineers, U. S. Army. The Corps of Engineers promptly established the Manhattan District in order to carry out this assignment. By May, 1943 the basic design work on plutonium production and processing had been essentially completed and the Laboratory's major task was the prosecution of the development program required in carrying out the detailed design of the Hanford facilities. The University of Chicago contract for the operation of the Laboratory was therefore transferred from OSRD to the Corps of Engineers on May 15, 1943.

While its activities in connection with the Hanford processes were given overriding priority, the Laboratory proceeded to formulate various alternative designs and processes in case major unforeseen difficulties should arise when the Hanford plant went into operation. In this connection, the first heavy water moderated nuclear reactor was designed and built at the Laboratory and was placed in operation at the Palos Park site in the spring of 1944. The groundwork was laid for alternative separations processes and the various experimental programs were carried to the point of establishing relative feasibilities for actual production use. Likewise, alternative metallurgical processes were developed in connection with fuel element fabrication problems.

In July, 1944 the Hanford plant was nearing readiness for startup. The Laboratory, as it had done previously in the case of the Oak Ridge staffing problem, met the need for experienced technical personnel by transferring to Hanford a considerable group of its own staff.

Meanwhile, the Los Alamos Laboratory had been carrying forward its bomb development program. While the Laboratory here was never actively engaged in this work, one of its original assignments was the responsibility for organizing these activities. This responsibility was discharged by recruiting a separate staff for this work with the Laboratory serving primarily as a temporary operations center. With the essential completion of its Hanford task, the Laboratory was urgently requested by Los Alamos to make available all of the experienced staff possible in order to expedite their program. As a result, a considerable number of the remaining staff of the Laboratory was transferred to Los Alamos during the fall of 1944.

In spite of the uncertainties regarding the ultimate future of the Laboratory which were prevalent during 1945 due to lack of long-term objectives, its research and development programs moved ahead with vigor. For example, in both physics and chemistry basic research results of interest and importance were obtained. The groundwork for the Experimental Breeder Reactor was laid, and the engineering studies which led to the mastery of the techniques for the use of liquid metals for cooling media were started. Active steps were taken in the development of a new and more effective separation process. The intensive biological and medical program not only produced new and vastly better data on radiation tolerances, but also made a signal contribution by calling attention to the hazards of beryllium and by carrying out important experimental investigations on the biological effects of beryllium poisoning just at the time that this material was becoming available for reactor work.

In the meantime, the problem of developing satisfactory solutions to the basic problems of the Laboratory’s future devolved upon its administration. Three major questions demanded answer. First, what were the principal needs for such a laboratory operated under government sponsorship on a long-term basis, and what should be its corresponding objectives? Second, could such a laboratory be organized and staffed in such a manner that these objectives could be achieved? Finally, what could be done about the long-term housing of the Laboratory in view of the fact that the major share of the Laboratory’s facilities were situated either in university buildings or in temporary structures on university property which due to their character would have to be abandoned very shortly?

After considerable study, it became clear that there were research and development functions which should continue and that, in addition, steps should be taken to encourage active participation in the atomic energy program by the qualified universities and research institutions of the Middle West. This planning was carried out throughout 1945 with the active cooperation of the Corps of Engineers. These activities culminated in the appointment, by the Corps of Engineers, of an advisory committee comprising leading scientists of the Middle West. The first meeting of this group was held on December 2, 1945, the third anniversary of the date of the startup of the first reactor. The committee worked actively during the winter and spring of 1945-46 and developed a proposed Plan of Organization and Statement of Operating Policy for Argonne National Laboratory. They simultaneously recommended that The University of Chicago continue as operating contractor. This plan was accepted by The University of Chicago and the Corps of Engineers. It was then ratified by representatives of the major universities and research institutions of the region in May 1946. These organizations thus became the charter members of the Laboratory’s Participating Institutions. The necessary contractual supplements were executed and Argonne National Laboratory began its official existence on July 1, 1946 with Dr. Walter H. Zinn as Director.

After an exhaustive study carried on in cooperation with the Corps of Engineers, a site was chosen in DuPage County to serve as a location for the new Argonne National Laboratory. The site was agreed upon both by the contractor and by the representatives of the Participating Institutions and was submitted to the Corps of Engineers for their approval and acquisition.

In the meantime, Congress passed the Atomic Energy Act of 1946 and soon thereafter the President appointed the first Atomic Energy Commission. The decision with respect to the acquisition of the DuPage site as a permanent location for Argonne National Laboratory was referred by the Corps of Engineers to the Atomic Energy Commission. In January, 1947 the Commission approved the new location and acquisition of the site was actively undertaken.

For the first year and one-half of its official existence under the new plan, the Laboratory was primarily concerned with work in the fields of basic research investigations and fundamental development studies. As a result of a Commission decision on January 1, 1948, the Laboratory was requested and agreed to assume the responsibility of serving as a principal reactor development center, in addition to the above research and development responsibilities. As one of the results of this decision, the Naval Reactor Division was established and a phase of its work is now nearing completion in cooperation with the Westinghouse Electric Corporation. Some phases of the Materials Testing Reactor Project, now being built at the Reactor Testing Station, were developed at Argonne in cooperation with Oak Ridge National Laboratory. More recently, the Laboratory is participating with the du Pont Company in the design and development of new production facilities. In the meantime, the development and design work for the Experimental Breeder Reactor has been finished and construction has been completed at the Reactor Testing Station in Idaho.

Even before the final decision had been made with respect to the exact location of the Laboratory's facilities, work had been started in the Laboratory on the design of necessary buildings. With the decision to acquire the DuPage site, the design activities were immediately stepped up and a group was organized in the Laboratory to assume the responsibilities in connection with the design of the new buildings. The planning for the facilities was still in an early stage when the decision was made to assign major reactor development responsibilities to the Laboratory. This decision introduced new requirements into the construction program, enlarging its scope beyond that previously contemplated. Ground for the first of the new buildings was broken in 1948 and construction has proceeded actively ever since. The East Area, consisting of a number of quonset-type buildings, was completed in 1949 and the Laboratory is now in the process of occupying the permanent buildings in the West Area.

This, then, is your Laboratory ... Its history and certain of its personnel date back to the beginning of the atomic energy project as a "business" and its record of achievement is one of which you may all be proud. It is your opportunity and your responsibility, as a member of the Laboratory, to make your contribution to the activities effective in maintaining this record.

Those early days as we remember them: part II

Arthur H. JaHey
Chemistry Division

In 1942, once the decision had been made to attempt to make plutonium-239 (Pu^{239}) for an atomic bomb, it was important to find "macroscopic" properties of plutonium, both as metal and as compounds. In a bold and imaginative program, it was proposed to make micrograms of Pu by cyclotron bombardment, and then to investigate its chemical and physical properties on a microscopic scale.

In the period of August 6 to August 22, 1942, I was engaged in extracting such Pu from 300 pounds of uranium nitrate hexahydrate (or UNH, as it was called).

I had been with the Metallurgical Laboratory only a few weeks, learning to precipitate and oxidize another isotope of plutonium (Pu238) when the first shipment of Pu^{239}-containing substance arrived from the cyclotron at Washington University in St. Louis. It had been formed by bombarding UNH with neutrons. The material — 300 pounds of it — came packaged in small plywood boxes of various sizes, made to fit the various niches available around the cyclotron target. Glenn Seaborg, the project leader, estimated that from this entire mass the yield of Pu^{239} would be about 200 micrograms.

Dr. Seaborg gave me the assignment of helping to organize the separation process. I was much impressed with two aspects: the need for speed and the preciousness of the material we were working with. During the last of the separation stages, the latter reached trepidation proportions, one might almost say fear. For the first time in my life I was handling something terribly precious, very important and not to be recovered if lost.

Practically the entire manpower of Seaborg's section became engaged in this separation process. We also were much impressed by the scale of the operation. Most of us had no acquaintance with handling large amounts of chemicals, our experience having been limited to what might be called table-top chemistry.

The standard method for separating Pu from UNH, developed by Seaborg, took advantage of the fact that UNH dissolves in ether, which then obligingly separates into two phases: one of ether containing about 9/10th of the uranium nitrate, and the other of water containing the plutonium, many fission products, and 1/10th of the uranium.

One run-through thus would bring the 300 pounds of UNH down to about 30 pounds — still an enormous amount from the point of view of the concentration of Pu in it. So the procedure would be repeated; but since the UNH was now dissolved in water it had to be reduced to the solid hexahydrate (crystals) before it could be ether-extracted again. The second extraction would bring the mass down to about three pounds. For prudence' sake, this would then be divided into four batches, each to be separately treated by "tabletop" chemistry. This process involved evaporating the uranium nitrate solution down, extracting with ether again, then precipitating the plutonium out of the uranium.

This over-simplified account sounds a little like whipping up a batch of fudge. Although I won't go into all the details, it wasn't that easy ...

The large-scale extraction work was carried out in an old storeroom in the attic of the fourth floor of the University of Chicago's Jones Laboratory. The west end of the floor was occupied by laboratories, but the east end had never been finished and was used as a storeroom for discarded apparatus. By pushing things aside, we cleared an area in which we could work. From this large room one could open a door onto a roof-space and this we used as an outdoor laboratory those sunny days of August 1942. During the evaporation phase we needed ventilation, since both ether and acid fumes came off.

"We" were pretty young, ranging from 20 to probably 30 years old. The whole operation was carried out in the spirit of what one might say was boisterous fun. At any one time there might be as many as eight or 10 of us shaking up the ether solutions and extracting it. At other times, as during the evaporation, there were of course fewer people involved. There was a lot of kidding and joking. By this time we had come to know each other well, we were all single, we ate lunch together, many of us had dinner together. We might even take a few minutes for a beer together. We were like a close-knit, small family.

Despite the high spirits and joshing, everybody worked hard and enthusiastically and for very long hours. There was a sense of urgency about getting the Pu^{239} extracted. We all had worked with tracer experiments on Pu^{238}, we knew how elusive the stuff was, and we knew it would not be possible to determine the real properties of Pu until a sizable amount had been extracted. The effectiveness of our operation was registered by the fact that the entire procedure lasted sixteen days, and this in cluded all the stumbling efforts to gather equipment and to learn how to do things most effectively.

Our equipment ... The techniques we were using were those of table-top chemistry, just blown up in size. We needed a very large separatory funnel — a size hard to come by. Fortunately, somebody at the university had been involved, for some reason, in handling large volumes of chemicals and had left a number of 1- and 2-liter size funnels in the stockroom. Other than that we mainly had only some large evaporating dishes and some hot plates. For later bombardments, we worked out an extraction method with some remote control features, in which the ether was surrounded by a lead brick shield. But for this bombardment we suffered the exposure from the gamma-rays from fission products.

During the period from August 24 to mid-September I worked at extracting Pu from one of the earlier-mentioned four batches, separating it from the residual uranium and from fission products. The separation method then available involved successive cycles in which plutonium was oxidized, then reduced and precipitated with lanthanum fluoride carrier. This involved procedure led to a feeling of strong frustration. For one thing, I didn't understand the mechanism of carrying (I'm not sure anybody did then), and I was concerned that in the long process something might be going wrong that I didn't understand. I was constantly concerned that I might heat too fast or let the solution get too close to dryness, either of which could lead to a loss of material by spattering. I of course hoped in great suspense that, working with Batch 3, I would come up with at least my share (one fourth) of the quantity of Pu (200 micrograms) Seaborg had estimated the 300 pounds of UNH would yield.

Several times in this procedure, materials appeared for which there seemed to be no reason, although they may have derived from impurities in the ether extraction process or from dirt picked up in the largevolume separatory phase. Each appearance had to be tracked down to be sure that the solids did not contain the precious plutonium.

Before any separation was started, I had made an assay of the quantity of recoverable plutonium in a small volume of the original solution. Now, after the final precipitation process, I was very pleased to find that I had made a very good recovery; my assay of the amount of plutonium recovery from Batch 3 was very close to that early assay. The activity recovered was 6.7×10^6 disintegrations per minute, meaning that I had ended up with 67 micrograms of Pu^{239} (based on the then accepted value of the Pu^{239} half-life). I turned the plutonium over to the microchemists and Michael Cefola assayed the solution. He checked my result within three per cent, very good precision for those days.

So! That ended my part in the separation from this first shipment of bombarded uranium. The plutonium delivered from Batch 3, and that from two of the other three batches, served as the primary material for the microchemists' use until the next bombardment was worked up, quite a number of months later. It was this Pu which was made into the first observed and weighed Pu compounds, later described by Burris Cunningham and G. T. Seaborg.

Leonard Bogorad
Engineering and Technology Division

I wonder how many of us remember St. Valentine's Day 1949? A freezing rain developed toward closing time, turning Site D (Argonne's present Illinois site), then in early stages of construction, and all roads leading to it into one endless, glistening ice field. For the Argonne buses leaving the site, it was impossible to negotiate the hill on Bluff Road near Route 83.

I was in one of the last of them, so saw the whole tableau. Walter Zinn, Laboratory Director, was sliding up and down the road in his Chevrolet offering to drive back to the Lab anyone who wanted to return. About 15 of us accepted.

While we thawed out in the temporary lunchroom in Building 10, Dr. Zinn searched the cabinets and refrigerator for something we could eat. At that time the prepared food was brought to Building 10 from the cafeteria at Site A. Dr. Zinn was disappointed-what he found was pretty "slim pickin's."

About seven o'clock, with the buses still literally spinning their wheels, Dr. Zinn took us in relays to the Guest House and there, through his or someone else's magic, sandwiches appeared from the Site A cafeteria. We spent a comfortable night at the Guest House, and learned later that the buses didn't get out until about 11 o'clock. Dr. Zinn stayed at the Guest House too, and in the morning drove us in several trips to Site A for breakfast.

Of all the weather-prompted crises that have been my lot, this one stands out sharpest. I still wonder how and when those who chose to stay with the buses got home that night.

While Argonne was moving from Chicago to its new DuPage Site, and for some time afterward until employees could relocate or arrange their own commutation, the Laboratory operated a fleet of buses to get people to the site and home again. These are the buses that, in Mr. Bogorad's story of Valentine's Day 1949, couldn't negotiate the hill on Bluff Road near Route 83. This picture was taken on a summer day with the buses parked, as usual, at the Meter House (now Building 90), through which badgeflashing employees had to pass before entering the east technical area

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Those early days as we remember them Part III

This lunch bunch, all veterans of the noisy (metallurgical) half of old Site B, shared reminiscences of 25 years ago with an Argonne News reporter for the benefit of this series. From left around the table are: J. Howard Kittel, EBR-II; George B. O'Keeffe, MSD; Helen Harman, the News, PER; John L. Armstrong, DES; David E. Walker, EBR-II; James F. Schumar, MSD; and Maurice D. Odie, MSD.

They — six men, each with 25 or more years at Argonne — gathered by prearrangement in the West Area Cafeteria. An Argonne News reporter was already there. Any inhibitions born of the tape recorder ostentatiously (but efficiently) occupying the center of the big table soon evaporated. The men had come together to share memories of the period just before and after July 1, 1946, when the Metallurgical Laboratory became Argonne National Laboratory.

At that time James F. Schumar was Acting Director of the Metallurgy Division. When Frank G. Foote joined the Laboratory as the division's director in July 1948, Schumar served as Associate Director, MET, until March 1960.

John L. Armstrong, then as now, was involved in the design of experimental test apparatus. David E. Walker was a Staff Sergeant in the Army assigned to the Laboratory as a member of the Special Engineer Detachment but was soon (December 1946) to become a member of MET's regular staff. J. Howard Kittel was on loan to the Met Lab from the National Advisory Committee for Aeronautics but became an Argonne staff member in September 1947. George B. O'Keeffe and one or two others had been asked to come to the Met Lab with Frank G. Foote in July 1943 when the operation at Massachusetts Institute of Technology was transferred to Chicago. (Dr. Foote returned to MIT in 1946 for a two-year period.)

Maurice D. Odie had been transferred to the Metallurgy Division from another operation at the Met Lab, as a technician.

James F. Schumar; Senior Metallurgist, Materials Science Division (MSD): To me the period around July 1, 1946, is the most fascinating in all of Argonne's history. Because at that time a whole new philosophy and surge of motivation swept the Laboratory. The Metallurgical Laboratory had been organized under the Manhattan District for wartime purposes — to learn to work with plutonium so that it could be used in a weapon that would end the war. That chapter was closed. Now what had long been just a hope was actually going to happen. The talent and drive responsible for the Met Lab's success were going to be turned to peacetime endeavors, to research, science, and engineering in humanitarian uses of atomic energy. Ideas for projects ran strong and now we can look back and see that most of what Argonne has accomplished since that time, and much of what we're still working on, already was being thought about at that time.

John L. Armstrong; Engineering Specialist, Design and Engineering Services (DES): Yes. It was like going from one room into another. In 1945, when the bombs were dropped and the war was over, there was a big cut-back in personnel and we just didn't know what the next step would be.

Schumar: The University wanted its classrooms back, its buildings and facilities. The companies from which the Met Lab staff had come wanted their people back.

Armstrong: One of the head engineers at DuPont for the Hanford reactors told me there was no future in atomic energy. A lot of people did go out and look for other jobs. Of the five in our drafting room, I was the only one who survived. I was glad to be there. Now we knew we had a peacetime mission.

David E. Walker; Associate Metallurgical Engineer, EBR-II Project: I sure wanted to stay on. I'd come from a small school in Pennsylvania Grove City College. At the Met Lab I found myself working among people I never thought I'd meet. Here the consultant I talked with was George Kehl of Columbia University, author of a textbook on metallography laboratory practice and physical metallurgy I'd studied in getting my bachelor's degree. And although I didn't get to talk to him, Farrington Daniels of the Daniels Pile at Oak Ridge was one of the authors of a book on physical chemistry I had studied.

J. Howard Kittel; Senior Metallurgist, EBR-II Project: But there was always a tremendous morale and spirit at Site B, which you who'd been there longer than I perhaps took for granted. I was loaned to the Met Lab from the National Advisory Committee for Aeronautics (NACA, later the National Aeronautics and Space Agency, NASA). Nuclear energy was of great interest for the propulsion of aircraft and I was sent to learn about the metallurgy of nuclear materials. At NACA we had terrazzo floors and beautiful equipment. We also had formal procedure channels for every aspect of our work. You can imagine my dismay when I first saw Site B. It was dirty, mice and rats ran in the halls, there were smells and noise and soot. But it took only a couple of weeks of being there and getting acquainted with the fellows to appreciate the great *esprit de corps* of the place. I managed to stretch my stay to three years, then returned to NACA only long enough to fulfill my obligations before returning to what by then was Argonne. In those days it was a pretty informal place. If we needed a piece of equipment, we told someone down the hall and many times it would be there the same afternoon!

George 0'Keeffe; Engineering Technician Senior, Materials Science Division: It was informal all right. At one point what I was doing required melting lithium in an Armco iron pot. Lithium, of course, reacts strongly with water. So, since we wanted to reuse the pot and it had to be cleaned, instead of doing this out in the yard I took it over to Lake Michigan, tied a rope on the handle, threw it as far as I could out and into the water, let the reaction take place, then hauled it in and took it back to the lab.

Kittel: Everyone just did things without a fuss, the best way he knew how. I remember my first encounter with Jack (Armstrong). I'd planned a new experimental set-up and asked him to design the equipment. A couple of days later I bumped into him in the hall. He said he was sorry to be keeping me waiting. I said, "Don't worry, I know it takes time to make the drawings for something like this." "Oh," Jack countered, "I got the drawings made right off. The job's in the shop but won't be ready until tomorrow." He was apologetic because the equipment hadn't been both designed and fabricated within two days!

Maurice D. Odie; Scientific Assistant, MSD: George's was one way to keep from adding to the smells and noise of Site B. I couldn't always manage as well. I was a technician in metallography, doing metallography on gallium with dry ice. Every so often my nitric acid alcohol etching mixture would fume up into a horribly noxious, chocolate brown cloud!

Walker: That was a strange situation, with the metallurgists in the front of the building and the biologists in the back ... The two were pretty far apart in their views and work.

Odie: Mostly it was their smells we couldn't stand, and our noise they couldn't stand.

Schumar: One of the smells we contributed actually was good, like a lot of geraniums. It wafted through much of Site B and came from the Dow-Therm that Larry Kelman and Lowell Lloyd used as a heat exchange fluid in connection with their — didn't we call them thermal harps? They were dynamic loops in the shape of harps and were used in sodium potassium (NaK) corrosion studies. Every time Larry asked Jack to design one, Jack would say, "Now look, this will be expensive — about the price of a Cadillac" — around $3,200 at that time. That's how we measured costs in those days.

Kittel: There was concern that the metals in a reactor, in a sodium-potassium environment, would stick together. So Jim (Schumar) said, “Why don’t you do some studies on solid-state bonding of similar and dissimilar metals in NaK?” And that became my first assignment.

Schumar: One of the interesting things we found out about sodium potassium in the molten condition is that it will clean the surface of other metals.

Walker: Then there was the electropolishing of uranium. Dr. Zinn and the others at Site A always had thought of uranium as being black, like coal. That’s the only way they’d ever seen it. Then Bob Noland, for whom I was working, and I, in the course of trying to electroplate uranium — a tough job because of the material’s chemical activity — discovered a way of electropolishing it. Electropolished, it was as bright as stainless steel, really metallic, not at all like a lump of coal. Jim showed a piece to Wally Zinn, who was so amazed he showed it to all of his staff. Jim left the sample with him, but the next time Zinn looked at it, it had turned black again. To stay bright it had to be sealed in a really dry, inert atmosphere.

O’Keeffe: This makes me think of the foundry we had. Not all of the mice and rats running around the place were of the city and sewer variety. Once in awhile we’d see a white one, a fugitive from Biology’s animal colony. I cornered one in the foundry one day and thought I’d do a good deed. I carried him by the tail to the biology section. A girl there took him from me and called him by name. “Why Horace,” she said, “you naughty little boy!” Then she promptly broke his neck and threw him away. “He’s been roaming around the building and no longer can be used in a controlled experiment,” she explained. That was the last mouse we returned!

Walker: We never did get used to some of Biology’s ways of doing. Remember how the machine shop opened on a little yard? In summer some of the machine tools were set up out there. One day some of the shop men were eating lunch on a bench in the yard, enjoying the sunshine, when one of the biologists came along and asked to use the band saw. “I’d like to cut something with it,” he said. Someone told him to go right ahead. The biologist opened a box of dry ice and took out a rat, frozen solid. He set it up on the band saw and began slicing sections, about a quarter-inch thick, out of it. It didn’t take the shop men long to empty the bench.

O’Keeffe: Well, maybe Biology had some right to complain about our goings-on too. We were working a lot with highly reactive materials: sodium potassium, lithium, and so on. And we had many fires. They weren’t serious fires, and we had our own fire department who knew how to deal with them, but they produced a lot of very caustic smoke. Of course water on some of the materials we were working with really would have spelled catastrophe! I didn’t think Frank Foote, the division director, was serious, but he told me that if ever we had a real fire and the City Fire Department came, my job was to run out with an ax and cut the hoses before they sprayed water into the building.

Schumar: But we found out one day he really meant this. The Chicago Fire Department did arrive one day. A neighbor, seeing white smoke coming from Site B — we were cleaning up some sodium potassium-contaminated apparatus — had called in an alarm. But George was spared the axing job because the guard at the entrance of Site B followed instructions. Laying his gun on the counter, he said to fhe fireman, “If you come in, I’ll have to shoot!”

Kittel: I guess we kept the neighbors in the apartment buildings around Site B pretty nervous over our disposal of NaK. We did this in a little courtyard in the back. It would react violently with water, often accompanied by explosions. We enjoyed all this, but the neighbors didn’t. Especially when, as happened every once in a while, a window somewhere would break.

O’Keeffe: We sure kept the neighbors guessing as to what we were working on. I lived in an apartment that overlooked the back yard of Site B and my mother lived with me. She was terribly curious as to what we were doing and I, of course, couldn’t tell her. Then one day she said she knew, she finally knew. “You’re making poison gas,” she said with conviction. She’d deduced this from seeing the many tanks of argon and helium and whathave-you going in and out of the trucks.

Schumar: I remember one warm summer day when we were out throwing a softball around during our lunch hour. A man came along and asked one of the biology technicians who was standing there just what went on inside of that building. “Shh,” she said in a stage whisper, “don’t tell anyone, but this is where we make all the red tape for Washington.” He seemed to take her at her word and went on his way.

Kittel: A good clue as to what the neighbors thought of us is this. Remember the Pontiac garage at 61st and Cottage? Several of us at Site B used to take our cars there for mechanical work. When we learned we were moving to DuPage I told the service manager I was sorry but that most of us wouldn't be coming around much longer. He said, "Well, I've enjoyed your business but I'm not at all sorry to see you go." I suppose I looked a little surprised. "We all know you're making bombs over there, even though the papers say you're not. I've just been hoping that when you went you wouldn't take us and the whole south side of Chicago with you." He was quite serious.

Walker: That reminds me of the forge we had in the foundry. Its foundations apparently went down to bed rock. When we ran the forge the vibrations would carry through this bed rock and come up, not in the immediate vicinity, but mysteriously at a point under an apartment building about a block away. Upon which dishes would fly out of cupboards, pictures would fall off of walls, tables would walk, and the tenants couldn't be blamed for thinking they'd been invaded by ghosts.

Odie: Remember Mrs. Rosen, who ran the little delicatessen and grocery next door?

O'Keeffe: The one who used to change the dates on the milk bottles?

Walker: It was a handy place at lunchtime, but sometimes we'd eat with the students over at the Commons. Fermi almost invariably would be there too, rather than in the faculty dining room where most of the staff would be.

O'Keeffe: He always had the same lunch, too, if you remember: a hot dog, piece of apple pie, and a glass of milk.

Kittel: Wasn't his assistant Leona Marshall?

Schumar: Yes — the only woman who was present at the first chain reaction, now Mrs. Willard Libby.

0'Keeffe: About once a month we attended seminars, I think in Eckert Hall, and whenever possible the speaker was Enrico Fermi. All the leaders of the project would be there — people like Arthur Compton, John Chipman from MIT, Frank Foote, Al Grenninger, Walter Zinn, Norman Hilberry, you could go on and on. Waiting for Fermi to begin, you could hear a pin drop. He could talk about the most complicated concepts in the simplest of terms. Some in the audience of course already understood nuclear phenomena, but I think I can say that after hearing Fermi everyone in the audience felt he did.

Armstrong: I did some work for Leo Szilard, another really brilliant man. He seldom sat still. If you wanted to talk to him, you usually had to follow him up and down the hall.

Schumar: We weren't without our characters at Site B. We had some fellows who never could work until 5 or 6 o'clock in the evening but then would work till 6 or 7 the next morning. And they were highly productive. They either just liked to work alone, or their work went better in a quiet, vibrationless building. What one fellow was doing required him to work when the temperature and dew point were down.

Walker: Not many women are metallurgists, even these days, but we had one at Site B, if you remember. She was Alice Smigelskas, now the wife of George Fischer of the Reactor Analysis and Safety staff.

Schumar: The Personnel people no doubt enjoyed having her on the staff, just as they did the military service people, because for them it wasn't necessary to write draft deferment requests. That was really a difficult job. They couldn't say exactly what you were doing, but neither could they give the impression that it was top secret. What came of it all could really be something to read.

Kittel: (to the reporter, indicating the tape recorder): Are you sure that machine is really working?

Reporter: Good grief! It better be!

She checked and it was. Someone glanced at the clock and there was a general scraping of chairs. Whether or not the flow of memories was over, the interview was.

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Those early days as we remember them Part IV

William P. Norris
Division of Biological and Medical Research

Ed. note: *The following was transcribed from a tape recording made in 1965. George Svihla of the Division of Biological and Medical Research, with no specific purpose for the material in mind but appreciating its historical value, asked several Met Lab alumni in his division to thus record their memories. We expect to use others of the monologues in this series of Argonne News articles and thank Mr. Svihla and the authors for permission to do so.*

I arrived in Chicago to take up duties with the Met Lab in July 1944. My association with the Project came about pretty much by accident, as was true for so many people. I had received my doctor's degree in organic biochemistry early in 1944 from The University of Illinois. By the latter part of June 1944 I had one foot in the Navy when my friend and immediate superior at the Dow Chemical Company mentioned my situation to Dr. Warren Johnson, who was at that time associated with the Manhattan Project, during a conversation on a train. Almost before I knew what had happened I had made a date to come to Chicago for an interview early in July.

I went to the old Armory at 51st and Cottage Grove Avenue in Chicago past an imposing array of armed guards and was taken into the office of a man (also named Johnson) who was in charge of Personnel. After talking with him for perhaps half an hour and spending two more hours filling out forms, I was put on a shuttle bus and taken to a place called Site B at 61st and University Avenue.

Site B was a pretty disreputable looking building but it also had an assortment of guards in uniform, with pistols strapped to their belts. In a new appendage to the rear part of the building I was interviewed by Dr. Kenneth Cole, whom everyone called Casey because his initials were K. C. Dr. Cole was very mysterious and gave me very little information except to assure me that this was a very important operation. Dr. Cole himself was a fascinating man who wore one of the most battered hats I ever in my life have seen. He also was extraordinarily flexible. With this flexibility he managed to sit in a chair with his feet up under him, a position he apparently preferred to any other since he tended to push himself around the room on the casters of the chair. To make a long story short, I took the job and reported to work two weeks later.

When I came to work I was told, although people were very nice about it, that I could not be given any information about what I was doing until my security clearance came through. It did, about three weeks later.

I was placed in a section under the direction of Dr. Raymond Finkle. My job was to work with fission products which were sent up from Oak Ridge to determine what their toxic manifestations and metabolisms might be. From the standpoint of my training, these were rather unexpected elements. They included strontium-89, yttrium-89, zirconium-95 and its daughter columbium (later renamed niobium), cerium-144 and its daughter praseodymium-144, and several others.

Included in Ray Finkle's section were Walt Kisieleski and Bob Snyder, who were concentrating primarily on plutonium-239, Dave Anthony who stayed through the end of the war, Catherine Lathrop, Bill Brown, Blanche Lawrence, Cary Armstrong, Tom Mulhaney, and Corporal Cliff Nordine. Dr. Richard Abrams headed the second section which was working mainly on inhalation of plutonium. With him were Clarence Beilman, Sergeant Sheldon Himmelstein, Bill Lohr and several others whose names I don't remember. Paul Tompkins, Sheldon Wish, and Abraham Broido ran the hot laboratory and did most of the calibration work, and Dr. Ladd Prosser was in charge of Physiology. Drs. William Bloom and Ray Murray did pathology for the division, and George Svihla was in that general area too, doing radioautography for the most part. There were perhaps 60 or 70 people in the division all together.

Shortly after I arrived, Dr. Austin Brues and Dr. Herman Lisco came from Harvard and were important additions to the division. Somewhat later Ray Zirkle came from Oak Ridge where he'd been working on gamma irradiation of mice near the reactors. We were all tied together by a peculiar sort of bond because we couldn't talk to anyone, not even our wives, about what we were doing. Also, there was considerable suspense in the division because we all knew that the time was rapidly approaching when the tests would be made.

This actually happened on July 16, 1945, when the first shot was conducted at Alamogordo. We were all very excited about this and somebody went out there and brought back pieces of fused desert sand which were very pretty, looking almost like a green pottery glaze, and which were also quite radioactive. Most of us spent the next several days to a week playing with radioactive determinations of these various bits of fused silica. Ralph Whitford, one of the machinists, devised a way of embedding them in lucite to make souvenirs, and I still have mine.

I think perhaps the most interesting situation of all developed just after the two bombs were dropped on Hiroshima and Nagasaki and the war ended. At that time, of course, we were permitted to talk about the work we had been doing. Sheldon Himmelstein, I remember, had a tremendous grudge against his landlady because all during the war, when he was in civilian clothes, she kept asking him why, young and healthy as he was, he wasn't in uniform. So the first thing he did was to put on his sergeant's uniform, with — as I remember — four or five hash marks on the sleeve, and go to visit her.

The University of Chicago almost immediately made plans to set up Institutes of Nuclear Studies and Radiobiology, which would be independently financed and operated rather than government subsidized. In the fall of 1945 we were told that now that the war was over the Metallurgical Lab would be disbanded and that it would be well for us to look for other jobs. Most of us did, but we also were asked to stay around and finish up any work we had going. Ray Finkle and Richard Abrams left rather quickly, and I took a position with Dr. Zirkle in the U. of C.'s new Institute of Radiobiology. However, since the Institute had no physical plant, we all stayed on at Site B until facilities could be made available. Of course, by spring 1946 the formulation of Argonne National Laboratory was underway and that settled questions for many of us.

Just a few more memories of Site B days ...

I haven't mentioned the elderly couple who ran the delicatessen next door to Site B. These people really did a rushing business around noon when fellows from the lab would go next door to buy one hard roll, two slices of salami, and fish a dill pickle out of the barrel. And if business wasn't too brisk, they also were allowed to listen to the complaints these people had. The Metallurgy Division had imported a large vertical press and mounted it next to the wall that separated the lab from the delicatessen. When the stamp press was being used, the vibration was such that canned goods tumbled off the shelves in thestore. The owners were of course furious.

They also contended that this was a very peculiar operation because there obviously was no product. Everything went in and nothing came out. Well, something did come out, but this didn't please them either. We used to incinerate large numbers of animal carcasses — mostly rats and mice and an occasional rabbit — in a muffle furnace which was ventilated through a stack on the roof. The roof was level with the store owners' living quarters, and the stench from the stacks frequently blew through their apartment. It also, on pleasant days, denied them the use of their porch which looked out over our roof.

Of course, Oak Ridge was a part of the early game. We made frequent trips down there to collect radioactive isotopes to work on in Chicago. At that time Oak Ridge had the distinction of being the biggest mud hole in the country. We slept in an army-type barracks and washed in cold water. No one in his right mind went there without equipping himself with a pair of boots and a bottle of bourbon. It was really a lot of fun.

Fond as our memories are of Site B, I doubt that anyone mourns that old building. It grew like Topsy and nobody knew where the electrical fuses were. I can remember that once, when the electricians started looking for a main fuse which had blown at about 3 p.m., they found it about 9 that night.

I moved out of Site B one day in November 1952 and never went back. Some two years later I did drive by and was startled to see that the building had disappeared and that only grass grew where we had spent the wartime years.

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Those early days as we remember them Part V

Farrington Daniels
Professor Emeritus of Chemistry The University of Wisconsin

Editor's note: *Dr. Farrington Daniels was Director of the Metallurgical Laboratory when it became Argonne National Laboratory on July 1, 1946. At that time he returned to his chemistry professorship at the University of Wisconsin, from which he had been recruited for the Manhattan Project in its early days. He was active in the Participating Institutions Program (Argonne's first vehicle for interrelationships with the academic community), serving as a member of the organization's Council Executive Board, and he has continued his association with the Laboratory in various capacities ever since.*

Dr. Daniels went on to become Professor Emeritus of Chemistryat the University of Wisconsin, continuing his research on the utilization of solar energy, a field in which he was a pioneer, at the College of Engineering of the University.

The Argonne National Laboratory is one of the great laboratories of the world, and I am proud indeed to have had a small part in its establishment when it was created by transferring to it the assets of the old Metallurgical Laboratory at the University of Chicago. On this, the 25th anniversary of its founding, I am glad to be invited to record some of my recollections of that important event.

I remember vividly the many long conferences which, as Director of the "Met Lab," I had with Arthur Compton and Norman Hilberry in 1945 and 1946 concerning the future of the laboratory. The question was: How could this great asset for the newly emerged science and technology of atomic energy best be used after the war to maintain the leadership of the United States and to assure development of peaceful uses of this great new source of power? And how could the technical staff, the unique laboratory equipment and "know-how" be effectively shared with the universities of the Middle West?

We had to find new quarters because the University of Chicago needed the many buildings and facilities which it had so generously given to the Metallurgical Laboratory for the duration of the war. Our first attempt was to use Site A, the 40-acre site with the nuclear reactor in the Cook County Forest Preserve which also had been loaned to us for the duration of the war. But the Park Commissioners refused to consider an extension of our lease. They stated that they had had great difficulty in eliminating from the park church properties, food vendors, and a cemetery, and they could not give up even a small area of the park. We pointed out the historic value of the early nuclear reactor as a tourist attraction, but they replied that they could attract more tourists by exhibiting a twoheaded calf. We were disappointed, but in retrospect we were most fortunate. Imagine the present Argonne Laboratory crowded onto 40 acres.

We decided then to try to move to the present site of 3,700 acres in DuPage County. The land was not expensive at the time, and it was not too far from the University of Chicago and the airport. I remember the heavy fog which greeted General Groves and Colonel Nichols when they came to inspect the site that we had chosen. We took them through it with a visibility of only 100 feet, but they approved the site. The reaction of the people located in the area was mixed. One farm had been in a family for three generations and moving was painful. Some feared that the influx of personnel might change the political complexion of the community. Others welcomed the new project. I recall the eager visit of the members of the newly created Atomic Energy Commission as they were trying to obtain a background for meeting their heavy new responsibilities. I remember the challenging offer to me to continue as director of the new laboratory but which I was prevented from accepting by the transfer to Oak Ridge of the new power pile project and my obligation to return to the University of Wisconsin.

Later, I participated in the Policy Advisory Board meetings with President Kimpton and then with President Beadle of the University of Chicago.

Vivid in memory, also, is the experience while director of the Metallurgical Laboratory in 1945-46 of seeing the awakening of the social conscience of the scientists. Before the war was ended, in part through the use of the atomic bomb, the atomic scientists were deeply concerned over the social and political responsibilities of their work. This pioneering in the discussion of the human implications of atomic energy met with objections from the Army, which insisted on complete secrecy, but the young scientists of the Laboratory went to Washington at their own expense and on their own time to educate Congress on the implications of the atomic bomb and to appeal for civilian control of atomic energy. They founded the *Bulletin of the Atomic Scientists*, which ever since has continued vigorously concerning itself with limitations on atomic warfare and matters of science and public affairs-national and international.

Accomplishments of Argonne

It took three years to go from the discovery of nuclear fission to a self-sustaining nuclear chain reaction; and three years more to the atomic bomb and the end of the war. And now controlled nuclear power reactors are here on a large scale, just in time to help meet the ever-increasing demands for more electrical power. These achievements involved the imagination and cooperation of scientists, engineers, Army and administrators on a scale perhaps never before achieved. Many new technical problems were solved so quickly and so successfully that confidence in the power of organized research perhaps became overrated.

Calculated risks of the dangers involved and the money invested had to be taken, but the record shows that great wisdom was shown by Arthur Compton, General Groves, Colonel Nichols, and down through a long line of administrators. The safety record has been extraordinary. The quick scale-up from less than a microgram of plutonium to more than a kilogram, a billionfold increase, was phenomenally successful. Glenn Seaborg was active in this program.

The new developments have been extraordinary-industrial electrical power on a vast scale, the widespread use of isotopes, the development of new radiation instruments, high energy testing facilities in Idaho and elsewhere, and high voltage facilities for the basic study of nuclear physics. Throughout the atomic energy program, scientists have been eager to build new facilities and the Atomic Energy Commission has had to decide which of many worthy projects to support.

Frustrations and disappointments have been keen when decisions have been made not to complete new and worthwhile projects. The fact that there have been many more good ideas than could be supported financially attests to the vigor of the research staffs at the national atomic energy laboratories and the universities. The contributions of the Argonne staff have been outstanding. They are too numerous to record, but as one example, one can point to the achievements of the Chemistry Division in developing completely deuterated chemical and biological compounds; discovering the chemical compounds of xenon and radon, gases which were thought to have zero valence; and the hydrated electron.

Many pioneering programs were carried out as recommended by the Argonne staff and the cooperating midwest universities, including new nuclear reactors, high voltage accelerators, and adequate housing and dormitory facilities for visiting scientists from around the world.

Walter H. Zinn (left) and Farrington Daniels studying map of Argonne's proposed site.

Early Leaders

The unquestioned early leader and pioneer was Arthur Compton. The first Director of the Argonne National Laboratory was Walter Zinn, who was in the project from the beginning. He was an active experimentalist who got things done. I remember the admiration which the Russian scientists expressed for him at the first international conference in Geneva in 1955 when he described the experiments in which a new boiling water reactor was allowed to blow itself up under conditions which could be studied. Without Dr. Zinn's early drive for nuclear reactors, the present nuclear power program would not now be so far advanced. Norman Hilberry followed Dr. Zinn as director, and he was enthusiastic and friendly and strengthened the cooperation within the laboratory and between the laboratory and the midwest universities.

The University of Chicago has had the responsibility for operating the laboratory, starting with President Robert Hutchins' decision to accept the secret project in 1941 before the war started. William Harrell was in charge of the business matters for a quarter of a century, and he was able and efficient in meeting these responsibilities. Warren Johnson represented the University of Chicago on scientific and professional matters, and he was always sympathetic and made wise policy decisions. Presidents Kimpton and Beadle took active and detailed interest in the Argonne National Laboratory and its relations with the midwest universities.

Farrington Daniels (left) presenting Arthur H. Compton (right) with a certificate authored by the scientists of the Metallurgical Laboratory "in appreciation of his broad vision, courageous and inspiring leadership, and his unfailing sympathetic support in the research and development required for the production of plutonium which contributed to the successful termination of the Second World War." Looking on is Colonel Arthur V. Peterson of the Manhattan District. Image Credit: University of Chicago Photographic Archive, [apf1-01872r], Special Collections Research Center, University of Chicago Library.

Starting in 1950, Dr. Joseph Boyce was appointed to increase participation by the midwest universities in the Argonne programs. At the end of the first year, he reported seven faculty and nine graduate students from seven universities in temporary residence. He initiated film badge services to 18 institutions, and he worked diligently to expand the cooperative programs.

It has been a great satisfaction to note the successful careers of many of the division leaders who were appointed to the staff of the Metallurgical Laboratory in 1946 and transferred to the Argonne National Laboratory. This list includes, among many others: Norman Hilberry, Walter Zinn, Winston Manning in Chemistry, Stephen Lawroski in Chemical Engineering, Frank Foote in Metallurgy, Hoylande Young in Library and Records, John Rose in Health Physics, and Austin Brues in the Biological and Medical Research Division.

Conclusion

The record of Argonne's service to the midwest universities is an impressive one. The universities also have made significant contributions to Argonne through their visiting scientists and the review committees which for over a decade have continuously studied and evaluated the research programs of the different divisions of Argonne National Laboratory. These efforts have involved the devoted services of many men. The Laboratory has an excellent record of achievement and service in the development of atomic energy and in the advancement of science over the past quarter century. May it so continue.

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Learn more on Argonne's Nuclear Science & Technology Legacy at http://www.ne.anl.gov/About/legacy/

Those early days as we remember them Part VI

Lester C. Furney (second from right), who formerly handled public relations at Argonne and is author of the article below, is pictured here in February 1956 with (l to r) Major General D. J. Keirn, Major General James McCormack, Jr. (Ret.), and Lt. General James H. Doolittle (Ret.) during a coffee break in a briefing session with the Research and Development Group of the U. S. Air Force.

Lester C. Furney
former Assistant to Laboratory Director Walter H. Zinn

Ever since receiving an invitation to contribute an article to the *ARGONNE NEWS* about my impressions of the early days, I have been trying to decide what was my most unforgettable experience. No one could have lived through World War II, through the exciting days of Trinity, Hiroshima, and Nagasaki, and through the hopeful months when Argonne National Laboratory was just a dream in the minds of a few of the wartime scientists at the Metallurgical Laboratory, without having amassed enough memories to fill a lifetime. All of these days were interesting and exciting and I have many fond memories of them. The remembrance, however, that keeps coming back time after time has to do with my introduction into the Plutonium Project. The first few days were the most unusual ones I have ever experienced. It all goes back to May 22, 1944, when I disappeared into Eckhart Hall and began a career that was supposed to last a few years but stretched into more than twenty.

Upon arriving in my office I learned that both my boss, Laboratory Director Samuel K. Allison, and my immediate supervisor, Assistant Laboratory Director Harcourt "Ace" Vernon, were out of town. I was told by Mr. Vernon's secretary that one of his capable assistants, Mr. Leroy Thompson, would explain my duties to me and would help me get started.

Mr. Thompson attempted to explain my duties to me without telling me anything about the Laboratory's program and objectives. He told me that I was to concern myself, initially, with the receiving, inventorying, and safekeeping of a number of rare and valuable materials which were used in the as yet unexplained research and development program. He provided me with a list of the names of the division directors and told me that I should interview them to learn about their problems with these as yet unnamed materials. When I asked questions, I got vague answers; and when I really pressed for more information, Mr. Thompson told me that he couldn't tell me anything more.

After gulping once or twice and organizing my thoughts as best I could, I started out for the New Chem building where I talked with the Director of the Chemistry Division and with a number of his senior scientists. I was armed with a pencil and with a notebook in which all of the pages were prenumbered. The notebook's outside cover was stamped, at top and at bottom, with the word "SECRET." I was instructed to use this notebook for all items that I wished to record and to avoid writing on any other paper.

As I interviewed the scientists, I made careful notes of what I had learned. One of the top scientists in New Chem told me that he got a shipment of “hot” and “cold” slugs each week from Site X. I asked him how the stuff was delivered. He told me that it came on the truck which shuttled each week between the Laboratory and Site X. I hadn’t learned a great deal, but I had learned that the project was big enough to warrant running a truck each week to somewhere.

In due course I had accumulated a lot of pieces of information in a real-to-life jigsaw puzzle. When Mr. Vernon asked me, a week later, to tell him what I had learned, I was able to give him a report which made him sit up and take notice. He didn’t know — and I didn’t tell him — that I had not forgotten the many speculative articles about uranium fission which I had read in 1939 and 1940. The news of a project to harness the energy of uranium fission had not taken me completely by surprise.

As I continued to interview the scientists that first week, I learned that “cold” slugs meant ordinary uranium metal which was usually in a cylindrical shape. “Hot” metal meant uranium that had been made radioactive in the uranium pile at Site X. I learned that Site X was something of a branch of the Metallurgical Laboratory which for security reasons was located in the hills of Eastern Tennessee. I learned that Site A was a secret laboratory located a few miles southwest of Chicago, and that Site B was an old brewery just south of the U. of C. campus. I soon began to hear a little about Sites W and Y, but it took me some time to find out where they were located and even longer to find out what went on there.

These weren’t the only codes that were used around the project. I soon learned that several of the key scientists traveled under pseudo names and that the man you thought was Dr. Smith might not be Dr. Smith at all.

When the atomic bomb was dropped on Japan and when the government released the famous Smythe Report, I learned a lot more than I had been able to pick up around the Laboratory and I guess this was as it should have been.

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Those early days as we remember them Part VII

Norman Hilberry, director of Argonne National Laboratory (seated) and Dean E. Dalquest (standing), superintendent of the laboratory's Graphic Arts division. They are examining an historic galvanometer recording of the fluctuation in a neutron density in Chicago Pile No. 1. The document, restored and framed in 1958, could be called a birth certificate of the atomic age.
Image Credit: University of Chicago Photographic Archive, [apf1-02809], Special Collections Research Center, University of Chicago Library.

By Norman Hilberry
Argonne Director, 1956-1961
Associate, then Deputy Director, 1946-1956

My association with the nuclear energy business began with the kind of startling abruptness that was to become characteristic of the way in which so many of the Metallurgical Project staff joined — or, as some of them felt, were shanghied.

Returning home late on the evening of Saturday, December 20, 1941, from a New York University Faculty Club Christmas party, I found a telegram waiting for me. It said, “Need you for important war job. Please arrange immediate leave of absence and report to my office at earliest possible time.” It was signed “Arthur C.”

I rousted Martin D. Whitaker, my department chairman, out of bed early Sunday morning, read him the telegram, arranged the leave of absence, caught the Century at midmorning, and was in Compton’s office at The University of Chicago on Monday morning, December 22, 1941, almost as soon as he was. And from that moment my pattern of life changed. After 20 years of total immersion in university activities, I found myself plunged, literally overnight, into a new career, one in which my responsibilities suddenly shifted from those concerned with my own doing to those involved in getting others to do, and in ensuring that they were given the means for carrying out that doing with the fullest possible effectiveness.

Having just completed a thesis on extensive cosmic ray showers under Compton's aegis the previous year, I was under no illusions as I traveled westward that my services were wanted because of my scientific genius. "A.H." had long since established the fact that he personally possessed a supply of that ingredient fully adequate to meet any scientific or technological situation with which he was likely to be faced. Having had a broad experience in industrial research and development as well as in academic work, Compton was well aware that the task on which he was embarking would be a very major enterprise and would inevitably involve the kind of investigative teamwork that in those days was still rare in university activities, although increasingly prevalent in industrial operations.

The character of much of the research and development work that had to be undertaken was such that essentially only members of the university research community possessed the scientific background vital to formulating and carrying out the necessary R&D programs with the sure ness and speed that the crucial nature of the project made imperative. And by far the largest majority of these qualified investigators were certain to be individualists who could be fully effective only in a working environment that simulated that to which they were accustomed.

Compton's problem in the present instance, was that of finding an administrative mechanism that would provide the freedom of individual action that constitutes the strength of university research in both its originality and productivity, while at the same time ensuring that the results so obtained would mesh together as well as if accomplished under the more tightly coordinated industrial operating structure.

The obvious answer was to so centralize the R&D activities that all concerned would be in essentially continuous, intimate personal contact. With sufficiently general work assignments and sufficiently broad potential overlap in the assignments to the different R&D groups, competitive professional drive would provide the best assurance obtainable that wide scientific and technological consideration would be brought to bear on the Project's problems and that all results would be subjected to the most critical evaluation that could be hoped for.

To achieve such a working environment clearly demanded the establishment of a centralized R&D organization devoted exclusively to the Project's problems. This course of action was further supported by the fact that only in this way could the Project satisfy the imperative need for an absolute secrecy barrier between its activities and the outside world, while simultaneously maintaining that freedom in internal information flow that is essential, at least at the upper scientific and technological personnel levels, for effective prosecution of research and frontier development activities.

Having explored the possibilities of taking over some established industrial R&D laboratory as a going operation which could then be modified and expanded as the situation required, and having further found that any industrial laboratory that could serve this purpose was already fully occupied with essential war work, Compton decided that he had no choice but to set up his own organization essentially from scratch.

This is where I came in. I had known "A.H." for nearly 20 years. This long-established friendship had expanded into a close-working relationship as well in the period immediately preceding the initiation of the Project. At Compton's request I had taken an active part in organizing and carrying out cosmic ray research expeditions to Mt. Evans, Colorado, in the summers of 1939 and 1940. In the summer of 1941 I had helped organize and carry on The University of Chicago-U.S. State Department "Good Will" Cosmic Ray Research Expedition to South America which Compton had arranged and in which he took an active part. Although largely a matter of understanding rather than of explicit designation, my role in each case was fundamentally that of a general utility field aide to Compton himself. The arrangement worked, the expeditions were successful despite the unexpected difficulties to which such enterprises are always subject, and consequently, when A. H. accepted responsibility for the Project, I was recalled to serve in the new enterprise as I had in the past on the expeditions. Thus I found myself plunged into all of the headaches involved in building an organization and in getting it functioning effectively and in high gear on a desperately tight time schedule.

The objective, the task

January 3-4, 1942, witnessed the first official meeting of those whose research on the fission process had eventually led to the establishment of what would henceforth be called the Metallurgical Project. The Project's work assignment had been firmed up. Essentially it was to carry out all of the research, development, and associated activities needed to ensure the production of plutonium-239 in quantities of military significance.

Basically this would accomplish two objectives: 1) It would furnish, for whatever help it might provide, the basic experimental data on the slow neutron chain reaction as a precursor to the fast neutron chain reaction studies required for the nuclear weapons work. In addition, if successful, it would 2) provide an alternate fissionable material that could be obtained by neutron irradiation and chemical separation methods, thus avoiding the enormous difficulties inherent in the isotope separation processes required to produce uranium-235 sufficiently free from uranium-238 to serve weapons purposes.

To achieve these objectives, however, would be no small matter. First it would be necessary to establish a controlled, selfsustaining nuclear chain reaction using normal uranium. This seemed a probability, were sufficient heavy water available — tons of it — to serve as the moderator; and a possibility if enough graphite of sufficiently high purity could be obtained. To get the heavy water would require a major isotope separation operation and, while a much less difficult task than the separation of the uranium isotopes, it would still be a slow business to produce adequate quantities. So graphite seemed the best hope for an early achievement, and it was made the prime choice for the Metallurgical Project work. Heavy water production was recommended to the national project authorities as a vitally important backup moderator, and the recommendation was implemented, but for the Metallurgical Project, graphite had to be the choice.

Once the nuclear chain reaction was achieved there would be the tremendous engineering development task of designing a reactor that would operate successfully at the relatively enormous power levels that would be required to generate militarily significant quantities of plutonium. In the proposed scheme, the plutonium would be generated in the same normal uranium that served to establish and maintain the nuclear chain reaction. A completely artificial element, it would perforce be generated one atom at a time as a uranium-238 atom captured a neutron from the flux produced by the nuclear chain reaction and then transmuted into plutonium by spontaneous radioactive transformation. One atom at a time, with many kilograms required to achieve military significance, meant that a fantastic neutron flux in the nuclear reactor would be essential. But for every two neutrons, approximately, in that reactor flux, one fission event with its relatively enormous energy release must have taken place.

From the first, therefore, the goal was the construction and operation of a nuclear reactor with a capability of operating at a power level of the order of a million kilowatts. This would produce plutonium at the rate of something like the kilogram a day needed to make the operation of very real military significance. For the present purpose the important fact is that this proposed power level was 10 times that of the then-existing average big electric power generating station, so that the straightforward engineering development problems involved, while not insuperable, were nonetheless clearly very considerable.

But success even to this point would not get the project out of the woods. To get a nuclear reactor capable of operating at a production rate of a kilogram a day was going to require a large quantity of normal uranium fuel not only to achieve criticality but also to provide adequate heat transfer surface in order to keep the fuel temperature within a feasible operating range. Something of the order of at least a hundred tons seemed to be a fair guess as a minimum quantity. With a ton being roughly a million grams, this would mean that each day the one thousand grams of plutonium produced would be imbedded in the matrix of some one hundred million grams of normal uranium. While not uniformly distributed in the uranium loading, it would still be true that the plutonium produced in a day’s operation would constitute an impurity in the uranium present in some tens of parts per million. Thus on any feasible production run, the plutonium to be recovered from the irradiated uranium would be present only to a small fraction of a percent.

To make the situation even more challenging, for each plutonium atom produced at least two highly radioactive fission fragment atoms would also be formed within the uranium, so that any chemical separation of the plutonium, from this now complex matrix in which it was embedded, would have to be carried out by remote control methods in the presence of an intense radiation field. And in addition, for the process to be acceptable under the militarily useful criterion, the recovery of the plutonium would have to be essentially complete.

That the development of such a process for a well known and thoroughly studied chemical element would be enormously difficult goes without saying. In this case the enormousness was compounded in multifold fashion by the fact that up to that time the world’s supply of plutonium was counted in atoms. In consequence, except for its fission characteristics and the sketchy chemical information gained in carrying out the carrier separation of these atoms from the irradiated matrix in which they were formed, there were no experimental data on the physical and chemical properties of plutonium. Moreover, save for the microgram quantities that could be produced through irradiation of large quantities of uranyl nitrate by neutrons being produced by around-the-clock operation of several cyclotrons, for some time to come there would be no plutonium with which to experiment. And yet, if the second objective were to be achieved successfully it was imperative that a chemical separation process be developed speedily and that the necessary chemical separation facilities be built in time to process the uranium loadings of the reactors as soon as the irradiation process in them had formed a feasibly recoverable quantity of plutonium.

Complicating the whole plutonium production process was the fact that it would require operations involving levels of radiation and quantities of intensely radioactive materials that previously would have been completely unimaginable. This not only called for a comprehensive program of shielding and containment research and development activities, it required in addition an intensive review of the field of radiation biology and medicine and the initiation of an all-out research program on the interactions of radiation and radioactive materials with biological systems of all kinds. Only to the extent that this program led the way could the requisite measures be taken that would ensure the health and safety of the operating personnel and of that segment of the public that might live or work within the region of potential influence of the proposed operations.

These then were the tasks that faced the Metallurgical Project as the first meeting of the Project staff convened in the conference room of Eckhart Hall on that January 3, 1942.

“Time was our basic currency”

The colossal nature of the above tasks, however, gives but one facet of the situation the Project faced that first Saturday in 1942. The world situation and our national position in it provided a psychological environment that pervaded every Project activity and weighed continually on every staff member right up to the final surrender of the German forces. Nearly three years had passed since the announcement of the discovery of fission and the publication of the fact that each fission event was accompanied by the release of something more than two neutrons on the average. The U.S. investigators had attempted to have this discovery held in secrecy but the French refused and published their results; the U.S. teams then reluctantly published theirs.

It was then evident to scientists everywhere that a nuclear chain reaction might be possible. With the German scientists ranking among the world’s leaders in this field, it was simply inconceivable that they were unaware of the possibility of establishing a nuclear chain reaction and of all of the implications of that fact, including the possibilities of constructing enormously powerful nuclear weapons. Evidence from visits to German laboratories, after World War II started but while U.S. citizens were still being welcomed, convinced the visitors that neutron research of the chain reaction type was being actively pursued. If so, this could well mean that the German effort might be as much as eighteen months to two years ahead of our own.

Several other possibilities were equally clear. If Hitler had any inkling of the military potentialities, it was inconceivable that he would not push his scientists and his industrialists to the limit to get such a weapon at the earliest possible moment. That German science and industry were entirely capable of carrying such a project to a successful conclusion, we had no doubt. Their total engagement in the war might slow them up, but many, at least, of the Project staff simply could not "believe that Germany was not already well along on the path to a nuclear weapon before we really got started. Hitler’s occasional hints about his secret weapon did nothing to allay our fears.

And it was obvious that if Hitler laid his hands on such a device first it would be used without scruple in its most devastating fashion. Defeat for us as well as the rest of the nonfascist world would be inescapable. No major holiday of the Allied world approached without our holding our breath as to whether or not this would mark the unveiling of the nuclear bomb, for Hitler’s penchant for using such occasions as a proper time to spring his surprises was well known.

This sort of universal belief on the part of the Project staff that national survival itself was the stake for which we were working and the corollary belief that therefore time was of the imperative essence tided the Project over the host of organizational and operational shoals upon which it would otherwise most certainly have foundered. Characteristic of the feeling is the wail with which I was occasionally assailed by one of my best friends when some requested action was delayed for 24 hours: "Damn it, Hilberry, is the Project office going to insist on losing this war?" Fear of national catastrophe should we fail, or fail to achieve in time, was the constant companion of the most of us. Time was our only basic currency.

These were the tasks and this the conviction of urgent necessity as the Project staff from all of the participating research groups gathered on January 3, 1942, to plan the Project work programs under the new OSRD sponsorship.

Assets and deficiencies

What were the Project's assets? There were essentially only two, but both were of high value. The second of the two, without which the first would have been helpless, was the backing for an all-out effort by the highest Washington command, and its conviction of the absolute necessity that such an effort be made. With such support guaranteed, the Project's first asset was comprised of the men who were gathered there in that soon to become familiar Eckhart conference room. They, their research and development skills, their unique knowledge and understanding of the nuclear fission field in particular and of the broad range of physics in general, and their total conviction as to the imperative and urgent necessity for swift achievement of the envisioned goals constituted without question the Project's greatest, if not indeed its only, parochial asset. It was a necessary, but a far from sufficient, condition for successful accomplishment.

Which, of course brings up the question, not of the Project's liabilities — of which, it is true, there were a few — but more importantly of its deficiencies, of which there were a multitude.

The first set of deficiencies were those associated with staffing. Except for the California plutonium group, who were chemists as well as physicists, essentially the entire group of investigators associated with the project work at the start were physicists. This was natural and indeed up to this point proper because, unless it could be shown that a controlled, selfsustaining nuclear chain reaction was indeed a scientific reality, effort expended on the other tasks would be unjustified. On the other hand, as soon as the probabilities of establishing a nuclear chain reaction began to look more promising than questionable, it would be imperative to put full steam ahead on all of the other Project tasks as well, for many of them could prove to be even more difficult and timeconsuming than the chain reaction studies themselves. This would be markedly true of the biological and medical studies, and could prove to be so for the chemical studies as well. Even the chain reaction physics studies were then staffed at a university research level rather than on the basis of facing a crucial national emergency. And as the meeting progressed from Saturday on into Sunday, it seemed increasingly obvious that, given the purer uranium and graphite with which it should be possible to produce industrially in industrial quantities, the chances for establishing a controlled self-sustaining nuclear chain reaction were shifting from the possible to the probable.

"We were completely unprepared"

The time had come to get the project into high gear on all fronts. But how? It not only lacked staff in disciplinary variety, it had no facilities in which to house them or the essential equipment required to make their research and development ef forts effective. It likewise had no experienced body of skilled technological services to support the investigative work, and no team of administrative personnel who were accustomed to the idiosyncracies of research and development work and of the practitioners thereof.

In fact, faced with the probability that an enemy was well on the way to a scientific and technological breakthrough that would result in giving him such an overwhelming military superiority that our sole choice would be to choose between complete societal destruction or the acceptance of military defeat and the endurance of political subjugation for generations, the United States found itself disastrously unprepared to take effective scientific and technological counteraction. We were so completely unprepared in terms of communication between the scientific community and the government that the better part of two years had been lost, and if it had not been for the bridge established by Bush and Conant the time gap could have been much longer.

We were also unprepared in terms of having adequate national scientific and technological establishments properly staffed, organized and equipped to carry on effective research and development activities in essentially any area of scientific or technological crisis that might face the nation.

It might be complained that the nation was obviously well enough prepared because we did in fact win out and did produce the atom bomb and do it first. This would be subscribing to a fatal form of wishful thinking. We won, it is true, but it was German failure that ensured our victory, not the excellence of our preparedness. And another time our adversary — whether Mother Nature, societal change or military adversary — might well prove to be less accommodating in committing errors in our favor.

From 40 to five thousand

Obviously, it is beyond the scope of this note to trace the step by sometimes agonized step by which the 40 or so members of the Metallurgical Project in March, 1942, grew to encompass the Met Lab, Clinton Labs, the Ames Lab, the research groups at Berkeley, Battelle and MIT, plus numerous other groups with at one time more than 5,000 employees all told. In disciplines the physicists were joined almost immediately by the chemists, who burgeoned, and by the engineers, who multiplied somewhat less spectacularly. By July 1942 the biomedical work was in full swing, and in August both metallurgists and chemical engineers joined the force. Here indeed was a true interdisciplinary operation. And team activity grew as the work progressed, both within and between groups, so that by the fall of 1944 the Metallurgical Project had developed precisely the sort of research and development establishment that would have done so much to expedite the Project's tasks had it existed in perhaps a somewhat less expanded but readily expandable form in January 1942. And with this organizational achievement accomplished, the tasks on which the Project had been fully engaged approached completion.

Job finished; laboratories too?

However, as Hanford came into full and fully satisfactory operation, the only further need for the Project and its staff was to serve in a standby capacity as an emergency scientific "fire fighting" force. This was not merely a matter of administrative decision by the higher "powers that be." Despite countless ideas the scientists longed to pursue, it was the brutal fact that there was no money available that could be spent for any activity not contributing directly to the winning of the war.

The continuing existence of the Metallurgical and Argonne Forest Laboratories in Chicago and of the Clinton Laboratories in Oak Ridge hung in the balance. These R&D organizations that had at long last been brought to a state of full effectiveness in the national scientific and technological interest seemed to stand at the brink of dissolution despite the toil and funds that had gone into their development. The priceless web of interdisciplinary, interpersonal relationships which had been so difficult to establish, which are so imperative to group creativity, and which are so easily dissipated by any form of psychological disarray, appeared to be on the verge of destruction. To many staff members it seemed that an array of unique and vital national assets faced doom, the apparent cause being a case of the hardening of governmental arteries with its attendant bureaucratic inflexibilities that made it impossible for high government to respond to crises outside the bounds of the routines of established "policies and procedures."

The peril to the laboratories was real, the apparent lack of interest in high governmental places was not. Bush and Conant were fully as aware of the need for continuing research and development work in the nuclear field as were the staff. They also were well aware of the continuing need for national, scientific and technological preparedness. And they were doing all that was within their power to resolve the problems involved.

General Groves and Colonel Nichols also were concerned, and they stretched their authority to its limit in order to keep the laboratories going. However, due to the stringent requirements for secrecy, it had been necessary to initiate and to maintain the support of the Project essentially as an activity carried on under the direct authorization of the President as Commander in Chief. This meant that any change in the basic charter under which the Project operated had to be sanctioned either by the President under his war powers, or eventually by Congress once the secrecy bonds were broken.

Clearly, throughout the fall of 1944 and on through 1945 until the bomb was dropped on Hiroshima, only direct Presidential authorization could have sanctioned the changes needed to keep the laboratories operating with their peak effectiveness. Needless to say, at this particular period in history both President Roosevelt and then President Truman were involved in some of the most delicate diplomatic negotiations and some of the weightiest decisions ever to face the nation. That it took time to arrive at a new policy with respect to the Project laboratories is far from surprising. In fact the real surprise is that it was accomplished as soon as it was.

In the meantime the laboratories did suffer serious attrition, but they did survive. And in their eventual restructuring not only were the "mission" needs of the nuclear energy program met but every effort was made to assure that as far as possible the nation would never again be faced with the sort of scientifically and technologically defenceless situation that it did face in January 1942. In the facilities provided and in the interdisciplinary spread of the staff authorized, the laboratories were in a position to initiate action on almost any scientific or technological crisis that might threaten the nation, and to expand swiftly and effectively in whatever direction might be necessary to cope with the crisis.

National Laboratories: today, tomorrow

It is now 25 years later. What of the situation and the role of the laboratories, particularly Argonne, today?

Clearly the nuclear energy mission is a finite endeavor and as such has a definite ending as a major enterprise somewhere down the line. But new scientific and technological missions, some fully recognized and others as yet only suspected, face us on every side. The nation will always need capably staffed and well-equipped laboratories to meet these new challenges. And as our population grows and, in consequence, our society becomes ever more dependent on its science and technology, its requirement for effective preparedness to meet recurring crises in these areas will also grow rather than diminish.

But while the missions change, and the nature of the scientific and technological emergencies with which society must cope are ever new, the core of scientific and technological disciplines required in achieving the necessary solutions does not change. Nor, in general, does the nature of the supporting facilities, of the implementing equipment, or of the basic organizational structure and administrative policies. Specialties and specifics may alter but the fundamentals remain the same.

It is clear to me as I review the inception, the growth, and the present maturity of Argonne that it and its sister National Laboratories are, if anything, even more necessary to the national welfare today than they were 25 years ago, and are enormously more capable of discharging their responsibilities effectively. I see but one change that I would make today in the plans that were drawn up in 1946 and 1947. Today the range of interdisciplinary competence necessary for effective solution of the scientific and technological problems of an affluent and numerically burgeoning society demands the inclusion of the social sciences on an equal footing with the natural sciences, and of the arts of social as well as of technological applications. This one rather considerable expansion in National Laboratory capability seems to me to be imperative.

But, once achieved, I am convinced that the National Laboratories can, and I believe will, continue to play a critically important part in serving the public welfare in times of serenity and in defending its existence in times of scientific and technological storm.

Read more articles on Argonne’s early days at http://www.ne.anl.gov/About/early-history-of-argonne/
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Those early days as we remember them Part VIII

by E. W. Rylander
Director, ANL Information Services 1959-1969

It was cold that night in ’43. The Bluebird bus ground to a halt at Budniks Drugstore in Lemont. As I walked down the main street, the stillness of the night was invaded by the bells of the countless churches in that quaint town. It was New Years Eve. Midnight. I reached the end of the street and my goal: Matt’s Tavern. Engulfed by the warm, yeasty air, I found compassion in a stein of beer. I had spent the day in the “Counting Room” of Site A measuring the radioactivity of air samples taken near CP-2.

Elmer Rylander inserting corn tassels into the thermal column of CP-3 in 1946. The irradiated pollen was used in a research program conducted by Thad Pittenger at the University of Nebraska.

Site A was the secret laboratory nestled in the Argonne woods of the forest preserve, about five miles from our present site. And, as everyone knows, the woods was named after the World War I Battle of the Argonne. CP-2, Chicago Pile No. 2, was the original West Stands reactor (a term not used then) rebuilt, enlarged, and shielded. It was a leaky reactor — a pile of graphite and uranium metal and oxide surrounded by concrete — a 30-foot cube with many experimental openings.

Site A at that time consisted of one building in which we worked, ate and slept. We had a ladies’ dormitory, too — for the one girl in our midst, Leona Woods, today Leona Libby, who was Fermi’s right-hand girl. The counting room was one of two air-conditioned rooms in the building (the other was the adjacent control room). Located below ground, it contained an array of counters or scalers built by Tom Brill. Each had a cute name such as Heffalump or Winnie Poo. Walter Zinn, (V.P. Combustion Engineering, retired) — he with the hard-shell finish, later to become Argonne’s Director — frequently slept in the room during hay fever season.

Just inside the guard post of Site A, and about 100 yards from the reactor, stood a poured concrete bunker. I often wondered of its purpose. Years later I was told that it was intended to be the control room for the first test of chain reaction (CP-1). The experiment was carried out at The University of Chicago because Site A had not been completed. Because of wartime shortages, wire-carrying conduit was exhumed for use elsewhere.

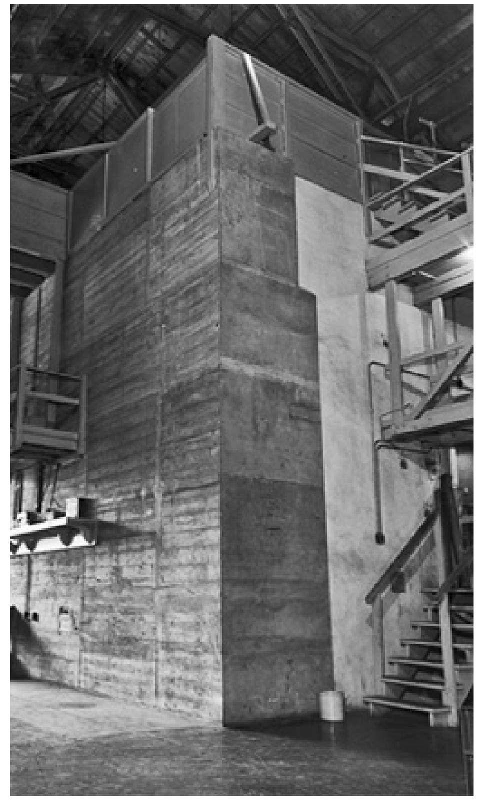
CP-2 had a thermal column atop it — a graphite wick which brought slow neutrons out of the reactor below. I recall Leona Woods irradiating metal foils in the graphite, quickly removing them, slipping them to a wire she had installed. The foils slid down the wire to the waiting hands of Fermi, who would dash to the counters to measure their short half-lives.

I mentioned that CP-2 was well-ventilated. Its reactivity would fluctuate to a small degree with changes in barometric pressure. I recall an experiment of Alex Langsdorf’s (Physics), whereby he sealed the cracks with children’s play clay and attempted to replace the air in the reactor (nitrogen was the culprit) with carbon dioxide. Otto Hillig (since retired to Denmark) was in charge of carbon dioxide production. Now Otto is a versatile, practical man. A machinist by title, he possesses an ingenuity that made him a valuable contributor to many experiments. He took particular delight in working for Zinn or Fermi, who referred to him as the “Great Dane.”

Otto was also a practical jokester! To produce the carbon dioxide gas, Otto had a 55-gallon drum which was filled with dry ice and then sealed. To provide heat he had a series of Bunsen burners under the drum, and a hose connected the drum to the reactor.

During the day, Zinn, escorting a visiting Navy officer, stopped by. He looked at the apparatus with a quizzical eye and remarked to Otto. “You had better be careful here. If that lead-off tube gets blocked, you’ll have an explosion.” As the intruders disappeared around the comer of the reactor, Otto slammed a huge piece of sheet metal to the floor. He was last seen that day dashing out of the reactor room with Zinn close after.

Al Wattenberg (University of Illinois) was a frequent user of CP-2 in his study of photoneutron sources. He would irradiate massive samples of sodium, removing them from the reactor only after the bus to Chicago, the Brown Bomber (or was it the Blue Goose?), had departed. He did his measurements out in the woods. Al will also remember his encounter with a ruptured half-gram radium source and the resultant decontamination under the able direction of Fred Pancner (FPD).



Chicago Pile No. 2, located at Site A. During the waning days of this site discussions were held with representatives of the Department of Interior relative to preserving the reactors for public education. A forest preserve official remarked "You’d get greater public attention if you had a two-headed goat".
Image Credit: University of Chicago Photographic Archive, [apf2-00480], Special Collections Research Center, University of Chicago Library.

Speaking of the necessity for decontamination, it is appropriate to note here Darragh Nagle’s (Los Alamos) encounter with a skunk while walking to Site A through the woods following Bob Nobles’ (AW) wedding in September of 1943. Darragh lost.

Charles Eggler (Physics), in the 1940s, was the nation’s authority for neutron source calibrations, using CP-2 as the tool. The national standard neutron source was located at Site A. Universities and other laboratories would send their sources to Eggler to be measured against Source No. 38, 500 mg radium-beryllium sourcewhich, I’d bet, is still located in the Physics Division. Eggler had a unique physical way of measuring radiation; ask him someday to elaborate!

A favorite pastime during the first winter at Site A was playing game called “peggity.” It involved moving wooden pegs on a board with a cross formation of holes. Fermi was its chief proponent. I recall taking my board home with me at Christmas and my parents watching me with exchanges of knowing glances.

The physicists were as much at home in the student machine shop as in their laboratory. It was not uncommon to see Zinn or Fermi or Herb Anderson (The University of Chicago) at a lathe or drill press. Machinist helper Frank Gotlund (retired), lumped us all together as “Jack Legs” — never did know what that term meant, but I don’t think it was complimentary. To improve our proficiencies, Machinist Herb Ross (OTD) and later Harold Lichtenberger (Combustion Engineering) offered a night course in machine shop practice. My wife still prizes a tack hammer made under Ross’s tutelage.

During the summer of 1943 ground was broken adjacent to CP-2 for a new building to house the world’s first heavy water reactor, CP-3. It seems incredible that the reactor went critical in May of the following year. It was built without complete drawings. I remember, years later, that draftsman Jim Riddick (retired) would seek us out with a question like “How much space do you think there is between the graphite and the heavy water tank?” We’d gesture, “About this much.” Jim would say “Hold it.” He’d pull out his ruler, measure the distance between hands, and head back to his drawing board. His beautiful set of drawings must still be in existence today.

I recall Zinn’s return from an inspection of the just-completed heavy water reactor in Chalk River, Canada. He was particularly enthusiastic about their white tiled, antiseptic building, the antithesis of ours, and with their use of round beam ports. The latter struck a responsive chord since several of us had spent weeks fitting our square, paraffin-lead-shot-filled plugs into the square, warped ports of CP-3.

With the completion of CP-3, it became possible to provide service irradiations to outside requestors: One of our first customers was Dr. Donalee Tabem of Abbott Laboratories. He would convert irradiated gold into a colloid and ship it to MeHarry Medical College for Hodgkin’s disease research. A somewhat unusual experiment involved raising corn plants at Site A and irradiating the tassels for Thad Pittenger of the University of Nebraska. Thad is now at Kansas State University and a member of the AUA Biology Committee. One of our most prolific users of irradiated chemical compounds was a young professor at the University of Illinois by name of Robert Duffield.

In the early days of the project, we had an English visitor with us for about a month. He had come to Site A after a stopover in Canada. We spent long evenings in the control room of CP-3 discussing the project. His name was Allan Nunn May — he was later to be picked up by Scotland Yard and thrown in the brig for spying for the Russians. One day when I was discussing the event with Zinn, I remarked that I had told May all I knew about our reactors. He looked me straight in the eye with a sense of relief and said, “Elmer, if what you know is all that he learned, we have nothing to worry about!” Candid.

One evening in September of 1944, I was in the control room of CP-3 manning the reactor when in burst a bevy of our top scientists. They were concerned. There was trouble at Site W (Hanford) and — Whoops. Out of space.

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