Nuclear Data

The Numbers Needed to Design the Bombs

by Ben C. Diven, John H. Manley, and Richard F. Taschek

The Los Alamos Laboratory was established in 1943 to investigate whether nuclear weapons were feasible and, if so, to design and fabricate them as soon as possible. It was obvious that this task demanded many new nuclear data. Even the basic fission processes were very poorly known, and most of the interactions of neutrons with nuclei of potential weapon materials were unexplored.

It was also clear that obtaining the necessary nuclear data required accelerators. Because building accelerators would be time-consuming, even if they duplicated ones already in existence, several accelerators at other institutions in the United States were simply dismantled, shipped to Los Alamos, and installed in hastily constructed buildings. A 0.6-million-volt Cockcroft-Walton accelerator came from the University of Illinois. Two Van de Graaff accelerators (2.5 and 4 million volts) came from the University of Wisconsin. And a cyclotron that could produce deuterons with energies up to 11 million electron volts came from Harvard. These machines had been used for effective nuclear physics research at their home bases but now were destined for studies specifically needed for the design of a nuclear weapon, under conditions where the effort could be better coordinated. In a single community day-to-day discussions of physical concepts and experimental methods would no doubt stimulate and speed up the learning process.

To learn about the data that needed to be gathered and the difficulties of doing so, we interviewed three scientists who participated from the earliest days. They clearly had enjoyed the challenges and the rewards.

SCIENCE: *Among the first and most important jobs at Los Alamos was the hurried transport of accelerators to the site. Why were accelerators needed?*

MANLEY: Accelerators could be used as sources of fast neutrons. Before Los Alamos the fission process had been well studied for slow, or thermal, neutrons because thermal fission was the basis for the reactors that would produce plutonium for the bomb. But in an explosive chain reaction in a nuclear weapon, a bunch of neutrons would come out—boom—from uranium or plutonium with much higher energies, almost a million times higher, than typical thermal energies. These so-called fast neutrons would not be moderated, or slowed down, by graphite as they were in a production reactor but instead would bounce around in a big mass of uranium or plutonium and cause various reactions. At the start of the bomb project, we didn't know how effective fast neutrons would be in producing new fissions. We needed to measure the fast fission cross section and other fast-neutron processes, and the only way to produce fast neutrons for these experiments was with accelerators.

TASCHEK: Most neutrons emerge from the fission process with energies between 0.1 and 3 MeV [million electron volts]. But until about 1942 there were no neutron sources at those energies except for Cockcroft-Walton accelerators of the kind that John worked with at Illinois. That machine was used to bombard deuterons with deuterons [$D + D \rightarrow {}^3He + n$]. Incident deuterons with energies of 0.4 MeV produced reasonably monoenergetic 2.5-MeV neutrons. Then at Wisconsin, where I was prior to coming to Los Alamos, neutrons with a range of energies were produced by bombarding lithium with protons accelerated in a Van de Graaff accelerator [$p + {}^7\text{Li} \rightarrow {}^7\text{Be} + n$].

MANLEY: We needed neutrons covering as much of the relevant energy range as possible, and we needed them in a hurry. So we just moved the university accelerators to Los Alamos as a matter of convenience. Wisconsin, which was on government contract, supplied the two Van de Graaffs. They produced monoenergetic neutrons whose energy could be varied from a few tenths of an MeV to 1.8 MeV. We went to Harvard and convinced them to let us have their cyclotron. Bob Wilson, being an old cyclotron man and having his project on isotope separation closed down at Princeton, was the logical one to run it. The cyclotron produced an intense neutron source over a big smear of energies. But with a moderator it became a good source of thermal neutrons. Finally we just swiped my old Cockcroft-Walton that was built at Illinois.

I was the one in charge of getting all those damned machines up to Los Alamos in the spring of 1943, and that was work. We had to load them from boxcars into trucks, travel up the old road to Los Alamos, install them, and so on. I remember we couldn't get the Wilson Transport Company on the job very fast. They did give us a driver and a little pickup truck, which couldn't carry much. We had packed the Cockcroft-Walton acceleration tube in a hurry simply by running a long bolt through all the sections and clamping them together with wood. That was in the back of the pickup truck waving around. I had fidgets coming up here. Then for several months we worked to put it all back together again. It was a mess at the beginning. The wiring wasn't all in, and here we were trying to get things hooked up. We worked three shifts a day, and by July every one of those accelerators was operational—a real record.

TASCHEK: Accelerators were very primitive in those days. We didn't ask for the Princeton cyclotron because it really was put together with sealing wax and string. When the magnetic field was turned on, the

The Illinois Cockcroft-Walton

The Cockcroft-Walton accelerator requisitioned for Project Y had been developed by John Manley and his coworkers at the University of Illinois in the late 1930s. It was an improved version of the first such accelerator, which was built in 1931-32 by Cockcroft and Walton at the Cavendish Laboratory in Cambridge, England. As plans for establishing the Los Alamos Laboratory developed, Manley, now a member of the Metallurgical Laboratory at the University of Chicago, persuaded a young Bachelor of that group, Harold Agnew, to take on the job of overseeing the moving of the Illinois machine to Los Alamos and its installation in Z Building. It was to serve there as a source of neutrons, which were produced by bombarding deuterons with accelerated deuterons. The accelerator was installed in the basement area of the building so that its vertical acceleration tube could provide a beam on a target at the ground-level area. As expected, the reduced atmospheric pressure at the approximately 7000-foot elevation of Los Alamos decreased the voltage attainable with the machine by a moderate 25 per cent from its design voltage, that is, to about 450 kilovolts. Above that voltage electrical sparking occurred from the exposed elements of the high-voltage equipment. This photograph of the Cockcroft-Walton shows a condenser bank on the left and on the right the high-voltage electrode with the acceleration tube extending vertically upward.

vacuum would break. The Harvard machine was the only reasonably well-designed cyclotron, so it was simply pre-empted—and at a ridiculously cheap price. The Short Tank Van de Graaff from Wisconsin was also a pretty poor specimen when it came here. It had been designed by the graduate students and was redesigned and rebuilt here under the direction of one of them, Joe McKibben.

Then we faced the technically difficult job of producing monoenergetic neutrons from the proton-lithium reaction. First we had to get monoenergetic protons out of the Van de Graaff. Then we needed a method for making very thin lithium targets so that the neutrons produced in the reaction would not be scattered as they left the target.

We had people working on making the neutron sources better and trying to generate new sources. Other people were working on how to measure the neutron flux [number of neutrons emitted per second], and some people of course were actually measuring the quantities of interest—of which there were a great number.

DIVEN: During the first year at Los Alamos the nuclear data work occupied the attention of a substantial fraction of the staff. It was an extremely important effort.

SCIENCE: *What were the crucial questions that had to be answered by nuclear data measurements?*

DIVEN: When we first came to Los Alamos, it was very poorly known how much uranium-235 or plutonium would be required to make a bomb because their critical masses for fast neutrons were unknown. The most important quantities to determine were the fission cross sections for uranium-235 and plutonium and the average number of neutrons emitted per fission. We also needed to know the fraction of fission neutrons that gets captured and does not take part in the chain reaction. We were going to try to decrease the amount of fissile material in the bomb by surrounding it with a so-called tamper that would reflect neutrons and prevent their escape from the nuclear core. So we had to measure the scattering properties of a huge number of materials in order to guess which would work best for this purpose.

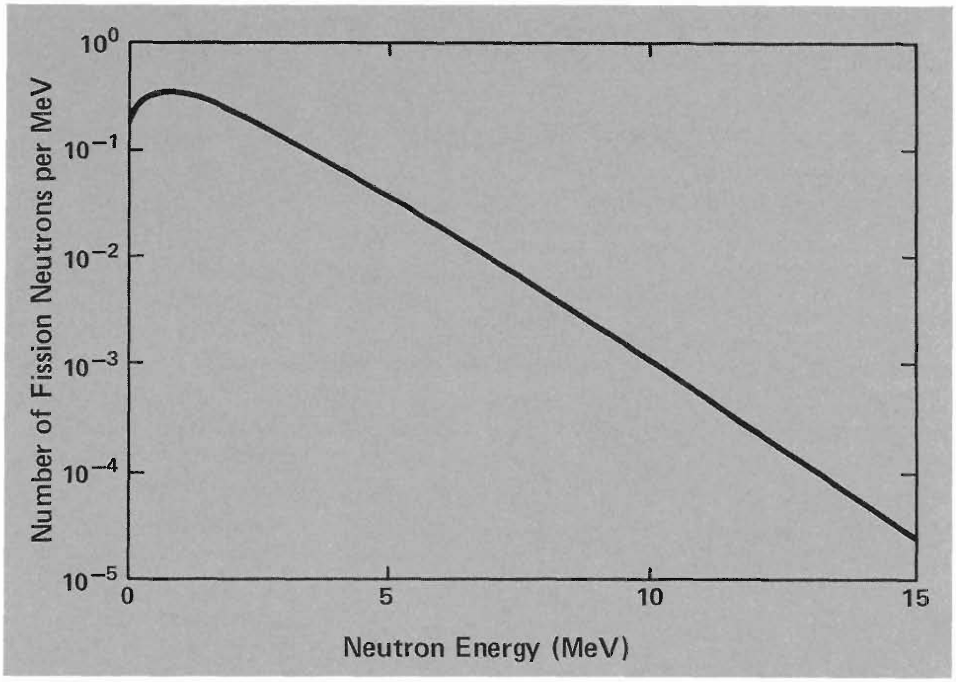
MANLEY: We had to know the elastic scattering, the inelastic scattering, and the capture cross sections for every single element we wanted to try as a tamper.

DIVEN: And we needed to know these cross sections as a function of neutron energy. A bomb contains a big mass of fissile material, and any one neutron can undergo many reactions as it bounces around in the nuclear core. It can scatter elastically or inelastically, it can be captured, or it might cause a fission. And every time it does one of these things its energy changes. It isn't enough to know a cross section at some particular energy. We needed to measure accurately the energy spectrum of fission neutrons and to measure the various cross sections over this whole spectrum. Making this enormous number of measurements in a short time was a staggering problem.

SCIENCE: *Did the nuclear data work begin at Los Alamos?*

TASCHEK: It started before at various universities and other institutions, and then the same people came here to continue it. For example, the need for a tamper was known very early, and people at Wisconsin working with the Short Tank, the small Van de Graaff, were trying various heavy elements like tungsten and gold. They had a rather impressive supply of gold there for that purpose.

MANLEY: The very first experiment done at Los Alamos was to answer the question of just how soon, relative to the fission itself, the so-called prompt neutrons are emitted. It was a go/no-go experiment—if the neutrons



Accelerators were brought to Los Alamos to provide neutrons with energies similar to those of the majority of neutrons produced by fission of uranium-235 or plutonium-239. Shown here is the spectrum of neutrons emerging from the fission of uranium-235; the fission neutron spectrum of plutonium-239 is similar.

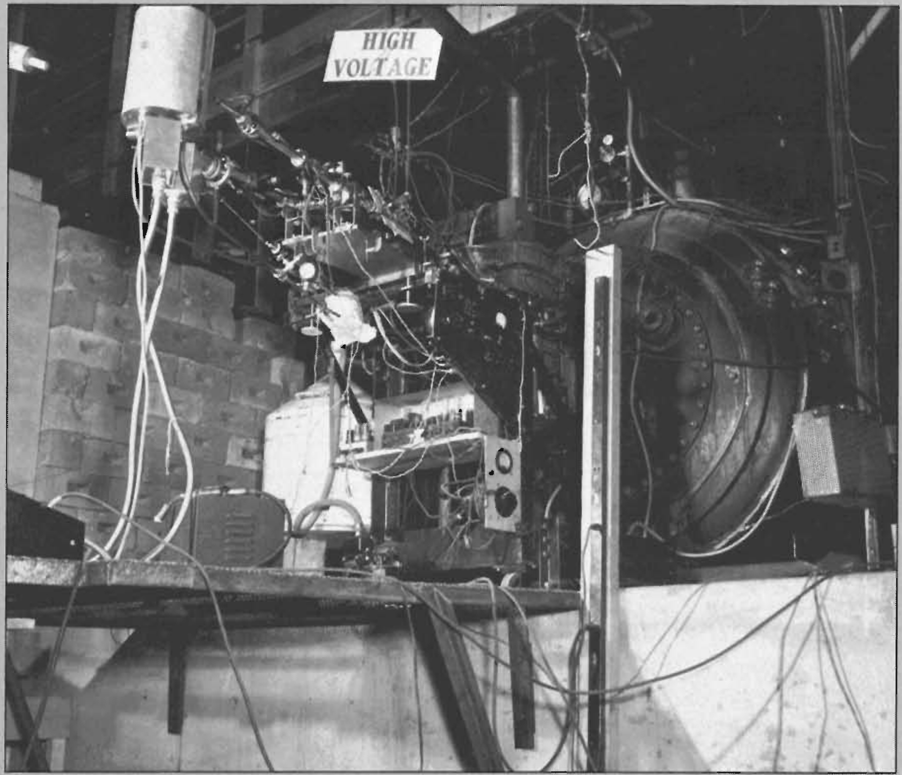
didn't come out soon enough, we couldn't have an explosive chain reaction. The presence of some delayed neutrons is what makes control of a reactor possible, but you don't want to control a bomb—you want it to go bang. Delays of a hundredth of a microsecond would have meant the end of the project. Some people here did a very cute experiment that could detect time delays of a billionth of a second. None were detected, so we were OK.

DIVEN: The experiment was really elegant because you didn't have to know the efficiency of the fast-neutron counters, you didn't have to know how much uranium-235 was in the target, and you didn't have to know the incident neutron flux. All you had to do was irradiate a uranium target with neutrons to induce fission and count the number of fast neutrons with a gas or vacuum between the uranium target and a fast-neutron counter. If neutron emission was delayed relative to fission, the neutron count would be less with gas between the target and the counter than with vacuum because, by slowing the fission fragments, the gas causes neutron emission to take place farther from the counter. Since the velocity of a fission fragment is about 10^9 centimeters per second in vacuum, a distance between target and counter of a few centimeters gave a pretty good time scale. Within the limits of the experiment, no difference in count rates was detected. So an upper limit of 10^{-9} second was established for the delay in prompt neutron emission.

MANLEY: That experiment was fairly easy to do because all we wanted was an upper limit. But as soon as we wanted absolute numbers for fission cross sections, we ran into serious difficulties. I remember tearing my hair out because we couldn't be sure how much uranium-235 was on the target foils. The assays were very difficult, and the results wandered all over the place. It wasn't even easy to determine how much total uranium we had.

TASCHEK: The fission cross-section experi-

The Wisconsin Long Tank



The Van de Graaff accelerator known as the Long Tank was the latest of a series built during the late 1930s by Ray Herb and numerous graduate students at the University of Wisconsin. Both the Long Tank and the Short Tank, a lower voltage, higher current Wisconsin machine that was the product mainly of Joseph McKibben, came to Los Alamos in the spring of 1943 along with much of the Wisconsin research group. The Long Tank was probably the best tool of that period for precision research on nuclear reactions, and after becoming operational again in June 1943, it became the workhorse for the Laboratory's investigations of neutron interactions with fissile materials and other bomb materials. Monoenergetic neutrons were produced by bombarding a lithium-7 target with accelerated protons. The energy of these monoenergetic neutrons could be varied between a few tens of keV and almost 2 MeV, a major fraction of the interesting part of the fission neutron spectrum. With accelerated deuterons, a different range of neutron energies could be reached but with more difficulty and rather bad backgrounds. This photograph of the Long Tank shows the neutron-producing target and an experimental target (foreground) and the pressure vessel (right background).

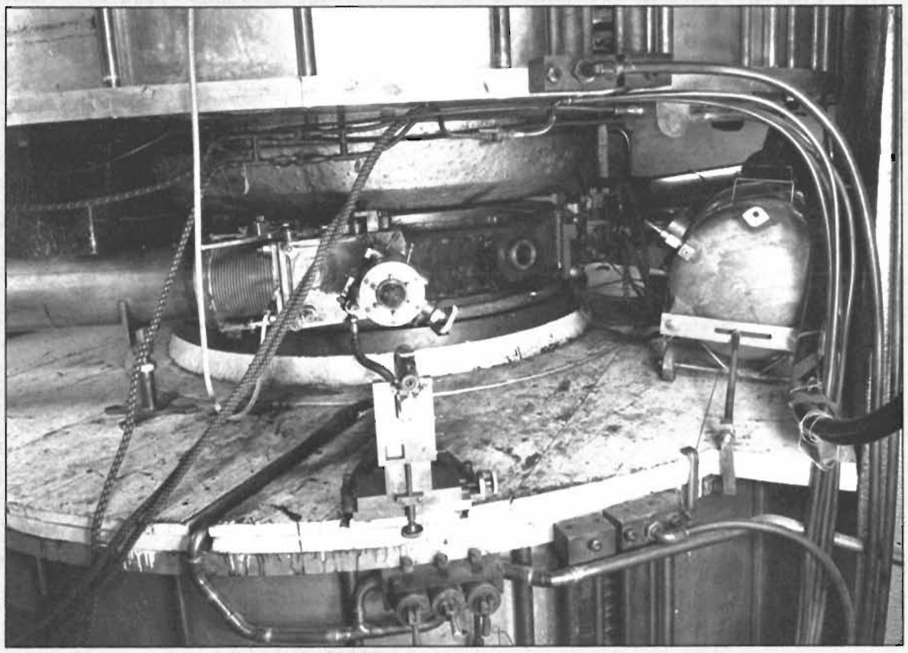
ments were done by coating platinum foils with a thin layer of uranium, maybe 10^{-5} centimeter thick, bombarding the foils with neutrons of a certain energy, and detecting the fission fragments with ionization chambers. By counting the fission fragments for different neutron energies we were able to make relative measurements of the cross section. For measurements of the absolute cross section we needed to know the neutron flux of the source. That problem plagued us for the next twenty years or so and still does a little bit.

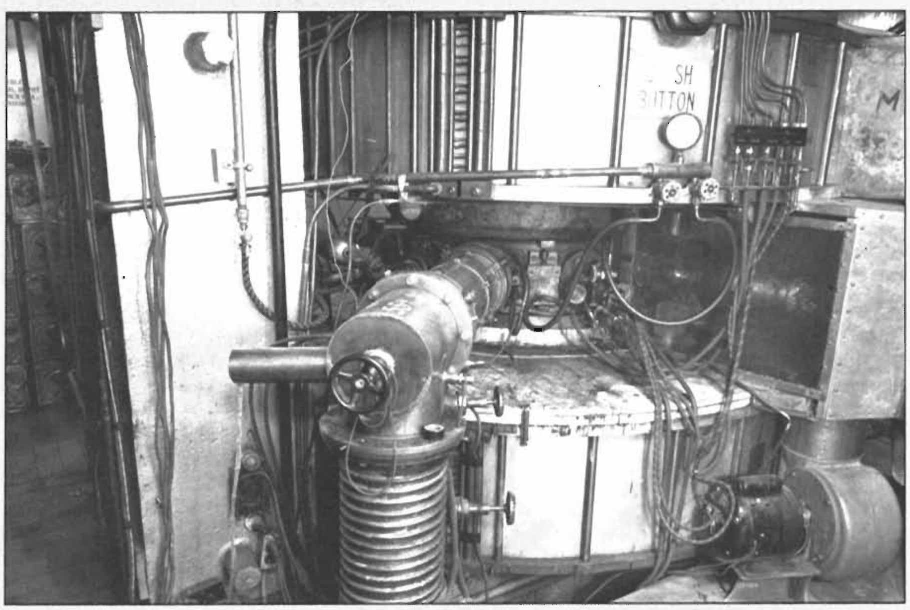
We also had great difficulty measuring $\bar{\nu}$ [the average number of neutrons emitted per fission] as a function of energy. That quantity could be measured fairly well and quite easily for thermal neutrons, but it was hard to measure for fast neutrons because so many neutrons—on the order of 10^8 or 10^9—must go through the sample before one fission takes place. In other words, the signal-to-noise ratio is very, very low. It was a long time before we could measure $\bar{\nu}$ for fast neutrons. During the war we simply assumed that $\bar{\nu}$ was a little bigger for fast neutrons than it was for thermal neutrons.

The neutron-capture cross sections were also very difficult to measure and cross sections for the emission of two neutrons weren't being measured at all except in a few cases where one of the final fission products is a radioactive nuclide. It took about twenty years before we could make systematic measurements of all the cross sections involved. The measurements John participated in during the war, that is, the angular distributions of inelastically and elastically scattered neutrons, were also very, very difficult. Not until the '60s did we begin to get some fairly decent measurements. Most of our wartime difficulties arose from lack of appropriate techniques and, most important, suitable electronics. From today's standpoint, electronics was at the cave-man level during the war.

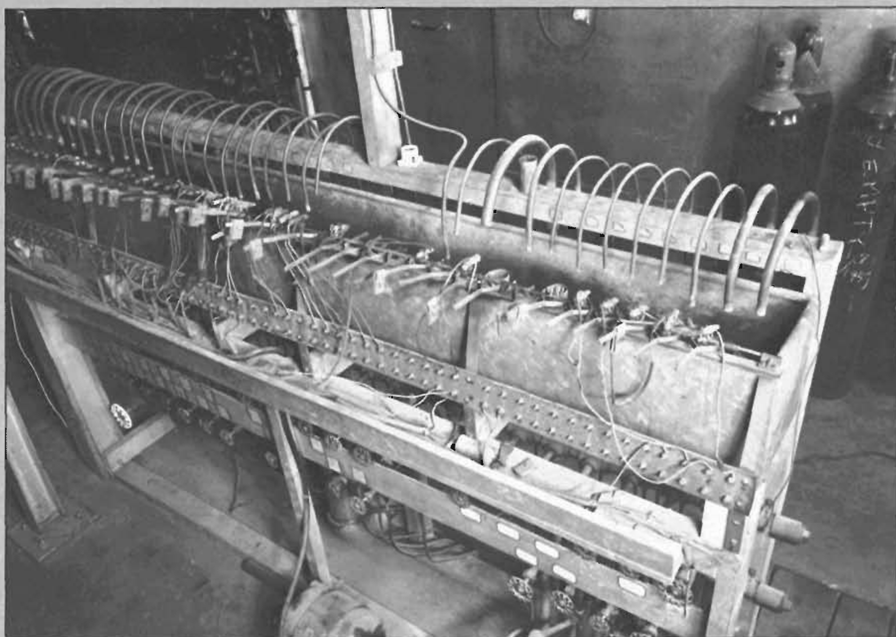
MANLEY: All these measurements were aimed at determining critical mass and ex-

The Harvard Cyclotron





The cyclotron commandeered for use at Los Alamos belonged to Harvard University and had been built there during the 1930s. It produced 7-MeV protons and 11-MeV deuterons with a maximum beam current of 100 microamperes. The two largest pieces of its magnet each weighed 18 tons, and the magnet's total weight was 70 tons. In preparation for



transport to Los Alamos, the cyclotron was disassembled and packed under the supervision of a group from Princeton University that later became the Laboratory's cyclotron group. Most parts of the cyclotron arrived at the site in early April 1943. By that time remarkable progress had been made toward turning the Los Alamos Ranch School into Project Y: most utilities were in, roads were built, and a special building for the cyclotron was complete. The magnet was reassembled by riggers using only jacks, rollers, and timber. Within about two months, albeit after many hours of overtime, the cyclotron produced its first deuterons for the war effort. These deuterons were used to generate neutrons by interacting with a beryllium target. In 1954 the cyclotron was relocated and rebuilt as a variable-energy machine. It was last operated in August 1974 and has since been dismantled.

The top left photograph shows the side of the cyclotron from which the deuteron beam was extracted. A bending magnet then steered the beam to a scattering chamber. The telescope in the middle was used to monitor the position of the dee feeler, which was very critical to the cyclotron's operation. To the right is the deflector capacitor. The bottom left photograph shows the other side of the cyclotron. From left to right are the diffusion pump, the ion-source control with water lines and cables attached, and behind the copper screen the two pyrex dee bells that support the dees. The top right photograph shows an unlikely contraption that assured proper flow of cooling water to various parts of the cyclotron. Return water flowed through the pipes into the little buckets, which had lead weights positioned along their "handles" and small holes in their bottoms. Mercury switches that monitored the flow of input water were mounted at the pivot points between the buckets and their handles. The set points of these switches were controlled by adjusting the sizes of the holes and the lead weights.

plosive yield. There were two main paths to arrive at the critical mass. One might be called the Edisonian approach—you just amass enough material and see if it works. But at the start we didn't have enough fissile material for this approach, so instead we tried to measure all the nuclear constants and all the cross sections that go into making a bomb critical and then summed up all the measurements to predict the bomb's behavior. This is the differential method.

TASCHEK: As more enriched uranium arrived at Los Alamos, we began to do an integral experiment known as a neutron-multiplication experiment. We added more and more enriched uranium to a uranium assembly with a neutron source at the center and measured how the number of neutrons multiplied with each addition. By extrapolating these measurements, we could determine the mass that would be needed to make the bomb go critical.

DIVEN: Finally we had enough material to achieve prompt criticality in an experiment called tickling the dragon's tail. We had a near-critical assembly of uranium hydride with a hole though the middle of it. We would shoot a small slug of uranium hydride through the hole, and for an instant there was enough material to make the assembly prompt critical. It was pretty exciting.

For some time there wasn't enough plutonium for any kind of integral measurement, so there was very heavy emphasis on differential measurements for that fissile material. The tamper materials were available, however, and John was doing integral measurements on them. Then the first significant quantities of plutonium began coming from Oak Ridge. At a fraction of a gram you could begin to measure some multiple effects. As more arrived, we were able to amass larger and larger quantities and get closer to what a real bomb would be like.

MANLEY: I was here when the first significant amounts of plutonium were delivered. Dick Baker fabricated some into a little sphere, and my group had to make neutron-multiplication measurements on it. That little sphere was so impressive to hold because it was warm from all the alpha activity. It was really quite a thrill to see this manmade element—it hadn't yet been discovered in nature—and to measure its neutron output.

DIVEN: By the time we had enough plutonium to make a bomb, nobody was interested in getting more differential data because it had been decided how to make the bomb. Everybody then began to work on how to diagnose what the bomb did. As a matter of fact, we *had* to stop making new differential measurements because that much plutonium didn't sit around in the lab with people petting it! It went right into making the Trinity device.

TASCHEK: In the last year the most important measurements were probably integral measurements. But the differential cross sections were used right up to the time of Trinity because they were needed for the first yield calculations from the Feynman and the Bethe-Feynman formulas. They were also used in calculations to check theory against the integral experiments. But of course no integral experiment short of detonating an actual weapon could include the implosion dynamics.

DIVEN: We did use the differential measurements to calculate the implosion, but in many respects the implosion device was a static device. The neutron generation time didn't change significantly over the many generations of neutrons produced before the bomb exploded. The thing we didn't know was the density of plutonium at different radii from the center during the implosion.

I should emphasize, however, that, as soon as the war was over, the differential measurements were once again the most important because they are the fundamental measurements. And for ten years or so after the war, a large effort was devoted to developing a reliable nuclear data base for weapon design.

TASCHEK: That's right. Nuclear data are needed because there is a basic technological difference between making a bomb and making, say, a steam engine. You can make anything from a little toy steam engine to a great big locomotive engine, and you can test it without completely destroying the engine. But a bomb does get completely destroyed in a full-scale test. So theory and computer simulation are very important in its design. And this is the main reason that computer development was worked on so hard at this Laboratory—to investigate the mechanics of implosion and to utilize all those complex nuclear data.

MANLEY: Apart from questions about critical mass, we had another big worry, and that was pre-initiation. If too many neutrons are around before the assembly of the critical mass is complete, you will get a fizzle. You want the neutrons to start the chain reaction at the moment the fissile material is in its most compact, or reactive, configuration.

DIVEN: At first the worry was that the alpha particles spontaneously emitted by plutonium and uranium would react with light-element impurities to make neutrons, and these neutrons would then initiate fission and produce a fizzle. Segrè wanted plutonium for the gun design that was pure enough to eliminate this source of neutrons. But when the plutonium from the Oak Ridge reactor arrived, he discovered a contaminant—plutonium-240—that was undergoing spontaneous *fission*. It came as a big surprise.

MANLEY: We had gotten word from France about the spontaneous fission of polonium, although it wasn't definitive. That was the reason why Segrè started doing spontaneous fission measurements.

TASCHEK: The discovery of spontaneous fission in plutonium-240 was really a blow to the bomb project because it meant that we couldn't use the gun design. Seth Neddermeyer's experiments with implosions really paid off then because the presence of plutonium-240 was not a problem with the implosion method of assembly. There wasn't enough time to build a plant to separate out the plutonium-240 for the gun device, so we went ahead with an expanded effort on the implosion work. As a result, the Los Alamos staff almost doubled.

SCIENCE: *Can we talk a little bit more about the development of detectors and electronics for the nuclear data measurements?*

MANLEY: We mentioned that electronics was primitive. We had to design amplifiers and timing equipment to pick up appropriate signals from the particle detectors, which were usually ionization chambers. Then we made scalers to count the electronically recorded signals.

DIVEN: We had a large fraction of the very bright people working on electronics during the war because it was so important. We made enormous improvements in electronics.

MANLEY: I should emphasize that these developments were not the result of physicists and electronics people getting together. Rather, many of the good electronics people were the good physicists.

DIVEN: As for detectors, some of the detectors used then are still used in almost exactly the same way. Ion chambers aren't significantly different now than they were at the end of the war. During the war Geiger counters, proportional counters, and ion chambers were the work horses. What was needed most was better electronics to record the output of the detectors. Also we had to arrange the Geiger counters or proportional counters in some kind of geometry that would let us do what we wanted to do. For example, the long counters were designed to detect neutrons with uniform sensitivity over a wide energy range.

TASCHEK: Initially we used ion collection—the old academic tradition—for most measurements. But ion collection was slow, and in addition the detectors were so sensitive to vibrational noise that they had to be suspended very carefully so they wouldn't vibrate during the long collection times. One improvement that combined electronics design and insight was the collection of electrons rather than ions. Since electrons move much faster than ions, the counting rates were higher, the collection times were shorter, a good share of the vibrational noise problem was eliminated, and the signal-to-background ratio was improved. I did one of the first fast-neutron measurements on plutonium, and with ion collection the measurement was almost impossible because the alpha background of plutonium, which has a relatively short half-life, was so vast. On the other hand, with electron collection the counting rate was a thousand times faster, and the measurement was sort of a lead-pipe cinch. The electron collection idea came from Rossi and Segrè.

DIVEN: When a charged particle enters a gas-filled ionization chamber it produces some ions and free electrons in the gap between two charged parallel plates. The electrons are attracted to the positive electrode and the ions move the other way. However, in most gases the electron attaches itself instantly to a gas molecule and forms a negative ion. The positive and negative ions drift slowly apart, taking about a millisecond to go some distance. Since electrons with their much smaller mass would move more rapidly across the chamber, Rossi searched for gases in which the electrons would remain free. Among those he found, there was a huge variation in the speed with which the electrons would move. Eventually Rossi found that in argon electrons moved roughly a thousand times faster than the ions, so counts could be registered a thousand times faster.

TASCHEK: The gas became impure very fast, but a recirculating system was developed that kept the system working.

SCIENCE: *To return to the experimental work itself, what nuclear data measurements were crucial to the development of thermonuclear weapons?*

TASCHEK: The most crucial was the measurement of the cross section for fusing deuterium and tritium. The original idea for a thermonuclear weapon was based on using the energy released in fusing two deuterons [$D + D \rightarrow {}^3He + n$]. But then tritium was seen at the Berkeley cyclotron in some highly irradiated targets, and Bethe persuaded the Purdue group to measure the DT fusion cross section [$D + T \rightarrow {}^4He + n$]. They accelerated tritium, which probably came out of the accelerator as HT or something like that. Neither Bethe nor anybody else anticipated such a big cross section for the DT reaction. But the Purdue group didn't have enough energy resolution to really understand their results. Then the work on the DT cross section was transferred to Los Alamos. In 1944 or thereabouts Bretscher and his group measured the DT and DD cross sections again. At that time Los Alamos had the world monopoly on tritium, and Bretscher's group had enough to make a target from water enriched to 25 or 50 per cent in tritium. The water was frozen onto a plate and bombarded with deuterons. They measured quite a piece of the DT cross section as a function of deuteron energy, and although the energy resolution in the low-energy region of interest was not all that good, they were able to determine that the DT cross section was higher than the DD cross section by a factor of 10 or more. That was the most important breakthrough for thermonuclear weapons.

MANLEY: It is amazing how early that work started. In the summer of '42, which was before the Purdue group was established, all the theorists, including Bethe, Teller, and so on, were together under Oppenheimer at Berkeley. In May of '43 Oppenheimer was put in charge of the Rapid Rupture Project, a delightful code name for fast fission. That group in Berkeley was giving theoretical direction to all the contracts connected with bomb development, and I was chasing around the country trying to see that the contracts got done, the experimental measurements got done, and so on. Whether the direction for the DT work at Purdue came directly from Bethe or Teller or by way of Oppenheimer and me doesn't matter.

TASCHEK: As far as Schreiber, who was in charge of the Purdue project, was concerned, his channel was through Bethe. The only surprising thing was that Bethe didn't predict the large cross section that was found.

DIVEN: It's interesting that the first laboratory building finished at Los Alamos was the cryogenics building to make liquid deuterium for a hydrogen bomb. By the time the building was finished, it was realized that hopes for developing a hydrogen bomb in the time available were futile, and so the building was used as a warehouse.

MANLEY: We might add that no one knew how to make a fusion bomb until 1951.

TASCHEK: After the big push for the H-bomb started in 1950, Jim Tuck and his group remeasured the DD, DT, and D^3He fusion cross sections. Since heating the material to thermonuclear temperatures would be very difficult, it was important to have accurate measurements of the low-energy region. The cross section varies extremely rapidly below deuteron energies of 150 keV, and the results of previous measurements were in disagreement. Tuck used very thin gas-cell targets to minimize uncertainties introduced by energy losses of the incident deuterons in the target material and was able to achieve what are still considered the definitive measurements of the DD and DT cross sections.

SCIENCE: *What were some other important or surprising nuclear measurements done at Los Alamos?*

MANLEY: Measurement of $\bar{\nu}$ for fast neutrons. That wasn't done anyplace else.

TASCHEK: Another important first at Los Alamos was observation of the width of the neutron resonances in uranium-235. The fact that these resonances were so narrow in energy and therefore long-lived was initially surprising to the theoreticians. They expected any resonant structure to be very wide.

DIVEN: One surprise was the amount of tritium produced from lithium-7 [${}^7Li + n \rightarrow n' + T + {}^4He$]. Only after we had unexpectedly large yields from the first solid-fuel thermonuclear devices because of this reaction did we measure its cross section accurately.

TASCHEK: In a more philosophic vein we developed a systematic approach for going from first principles to the development of a complex device. The necessary steps between science and technology were worked out and in the last thirty years have been applied to many other technologies. Inventions such as Edison's electric light have a scientific basis behind them, but they were made by playing around in the lab. Now most things are too complicated for that to take place.

MANLEY: The fast-neutron measurements made at Los Alamos on almost any isotope in the natural world made a big impact in the outside literature.

TASCHEK: That's right. Our fast-neutron work dominated all other similar work for at least ten years. This work was important as pure science and it also formed a large part of a solid quantitative basis for weapon design.

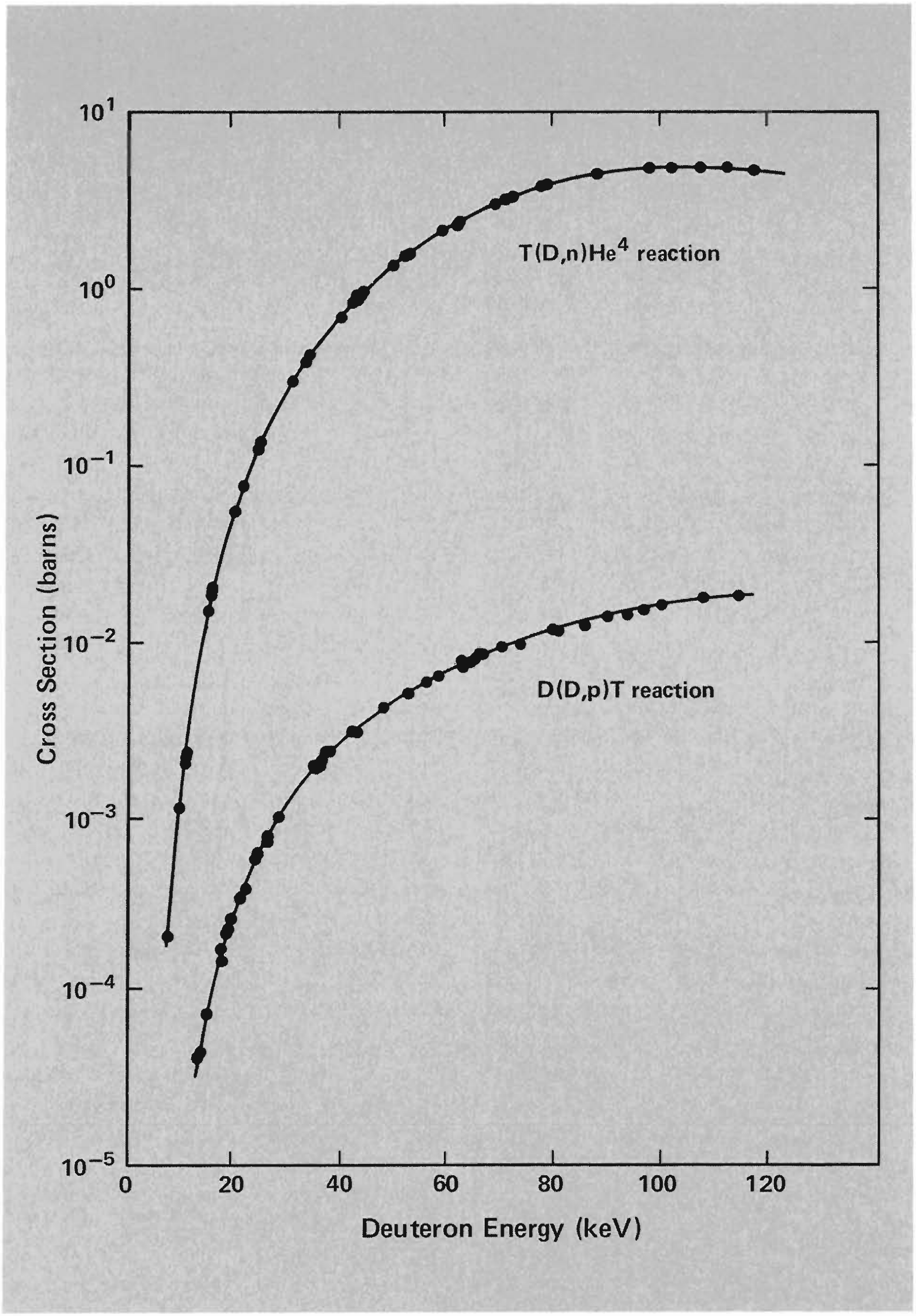
DIVEN: That work was also directly applicable to fast reactors. Probably for twenty years after the war most of the fast reactor data involving fast neutrons came out of Los Alamos.

TASCHEK: And those data were used in thermal reactor work as well because, depending on how a reactor is designed, how much moderator is used, and so on, a good fraction of the fission in a thermal reactor is fast-neutron fission.

I'd like to point out that prior to the Los Alamos work most measurements in both charged-particle physics and neutron physics were just relative measurements. People didn't bother to measure anything very accurately. They got a counting rate, but they didn't know the cross sections very well as a function of energy. Data like that can't be put into a design.

MANLEY: Calibration of the sources was the key to getting reliable numbers. We set up a special small lab just for that purpose.

DIVEN: The systematic approach to fast-



Definitive measurements were made at Los Alamos in the early 1950s of the cross sections for two fusion reactions that might form the basis of a thermonuclear weapon. From W. R. Arnold, J. A. Phillips, G. A. Sawyer, E. J. Stovall, Jr., and J. L. Tuck, Los Alamos Scientific Laboratory reports LA-1479 and LA-1480 (January 1953).

neutron data also had an important influence on postwar theory. For example, the statistical models of nuclear reactions were developed as a result of that work.

SCIENCE: *How has the relationship between theory and experiment evolved over the years?*

TASCHEK: Before the war nuclear theory was really crude. I went back and looked at the first review papers of Bethe and Bacher. They contain an awful lot, but a lot is missing too. The situation is quite changed around now: theory can explain everything that experiment can do plus a little more. Nowadays you are likely to believe the theory.

DIVEN: In some cases relevant to weapon phenomena, you have to believe the calculated cross sections because the isotopes present are so short-lived that they disintegrate before you can collect them to do the experiments.

TASCHEK: However, the detail of the calculations is often still not adequate to the design problem. For instance, we are still measuring the uranium-235 fission cross section, and we can measure it to an accuracy of about 2 per cent. Theory won't predict it that well. Another example is the DT cross section, which is a simple problem from the theoretical point of view, but its absolute value still cannot be calculated as well as it can be measured.

MANLEY: Dick and Ben are giving answers to the question in which the word "theory" relates to models of a nucleus that help us understand or predict results of experiments on particular nuclei. There is also "theory" that predicts the behavior of a system of interacting nuclei, such as in a nuclear reactor or bomb. With enough experimental information on cross sections, etc., one can do quite well in making "theoretical" calculations of system behavior without "nuclear theory." Examples are critical masses, bomb efficiencies, reactor neutronics, and the like. These calculations more than nuclear theory occupied efforts here for many years and were the major reason for the important developments at Los Alamos in computers that have resulted in very sophisticated nuclear weapon design calculations.

TASCHEK: One experiment we have not talked about yet and might be good to end on was the Trinity experiment.

MANLEY: Yes. One of the most valuable pieces of data from the war years was the generation time measured at Trinity—the alpha experiment. Alpha is a measure of how the neutron population increases with time. It is closely related to bomb efficiency.

DIVEN: The number of neutrons produced as a function of time is $e^{\alpha t}$, where α is a constant if the density and size of the energy-producing region don't change significantly during the explosion. Alpha is still one of the most important diagnostics for all of our tests. If you want the simplest possible test, you measure nothing but the yield—the total bomb energy—and alpha; these parameters will tell you the most about how well or how poorly the bomb worked.

TASCHEK: Many other nuclear experiments were set up at Trinity to do diagnostics, that is, to diagnose the causes if the yield was not anywhere near the theoretical expectation.

MANLEY: That was the purpose of the Trinity experiment after all. We didn't know what the yield was going to be, so we had to prepare for everything from zero to twenty kilotons and to give the answers for why it was any one of those figures from zero to what it was.

TASCHEK: We measured many things that had not really been looked at adequately. Prompt gamma rays were measured in a uniquely definitive way for the first time at Trinity.

MANLEY: In terms of comprehensive data collection, the Trinity experiment was one of the most amazing field experiments ever. Every measurement, as far as I know, was significant in one way or another. It was probably the only field experiment where you had only one shot at it. And that is still one of the problems at Nevada. There is a lot riding on each individual shot. You can't go back the next day and tweak things up and try again like you can in the laboratory. It is too expensive.

It must be intriguing to listen to us talk with such obvious enjoyment about these things that were really a hell of a lot of work. ■