# Reminiscences of the early days of fission

**News of the discovery of fission and the onset of World War II affected the author's activities, taking him from Princeton to Los Alamos by way of Lawrence, Kansas.**

Henry H. Barschall

Roger Stuewer has described how Léon Rosenfeld brought the news that uranium undergoes fission to Princeton's Physics Journal Club on the evening of 16 January 1939—the day on which Niels Bohr and Rosenfeld had arrived in New York from Denmark. (See Stuewer's article in PHYSICS TODAY, October 1985, page 48). Bohr had first learned of the discovery of fission from Otto Frisch on 3 January 1939, and Rosenfeld's report was the first information received by physicists in the United States. I was in the audience and the news had an immediate impact on my own activities, and it continued to affect my work for the next six years. The following are some of my recollections of that period.

At the time I was a graduate student at Princeton working under Rudolf Ladenburg in fast-neutron physics. After leaving Germany, I had arrived at Princeton in the fall of 1937. Ladenburg, who is best remembered for his contributions to atomic spectroscopy, had left the Kaiser Wilhelm Institute in Berlin in 1931 to become the Brackett Professor of Physics at Princeton. A relative of mine, Otto Meyerhof, who had been a colleague of Ladenburg's in Berlin, persuaded him to arrange for my acceptance as a physics graduate student even though I had neither an undergraduate degree nor any documented qualifications. It was therefore natural that I should work under Ladenburg.

Soon after the discovery of the neutron, Ladenburg initiated a research program to study the interaction of fast neutrons with nuclei. For this purpose he had acquired in 1934 a 400-kV power supply, which was used to accelerate deuterons. The deuterons impinged on a $D_2O$ ice target and produced fast neutrons. It was an impressive installation that required a lot of attention to operate properly. Fortunately, a more experienced and exceptionally able graduate student, Morton Kanner, taught me patiently all the tricks one had to know to produce and detect neutrons. Almost all the vacuum equipment and all the electronics were built in the department, because there were no commercial suppliers.

Finding and fixing leaks in the vacuum system of the accelerator was an almost daily task. The leak detector was a gas discharge that changed color when alcohol was sprayed on the leak. The proof that the vacuum system was tight enough was the absence of strong x rays coming from the acceleration tube. The x rays were observed on a fluorescent screen at the end of a dark box. Holding one's hand between the fluorescent screen and the acceleration tube allowed one to see the bones of the hand when the accelerator was operated with a poor vacuum.

Sealing the leaks was an art. The expert knew which kind of wax (black or red, hard or soft) was most effective for what kind of leak. Painting with glyptal, a then widely used alkyd resin, was our last resort. Red glyptal was used when other researchers needed to be warned of the prior use of paint, clear glyptal when the use of paint was not to be advertised.

Our home-built electronics required constant attention. Because the amplifiers were very slow, they amplified 60-Hz pickup as well as microphonic noise. Although we could avoid talking or walking around while taking data, we had a noisy mechanical vacuum pump that occasionally caused problems even though we had placed it in a soundproof box. There were no electronic pulse height analyzers. Instead we used a mechanical galvanometer and recorded the deflections of a light beam on photographic film. After the film was developed, we spent day after day measuring the deflections of the light beam with a magnifying glass to obtain pulse height distributions. Another experimental difficulty was the high background count of our detectors, which was due to an accident that Ladenburg had had several years earlier. Suspecting that a radium–beryllium neutron source had a small leak, he had attempted to touch up the soldered joint that was intended to provide a gas-tight seal for the capsule enclosing the radium–beryllium mixture. In the process, the source had blown apart and not only was he sprayed with radium, but, as he was careful to work under a chemical hood, the radium was blown into the entire ventilating system of the building. Radon from the decay of radium spread into our counters, and alpha particles from radon and its daughter products pro-

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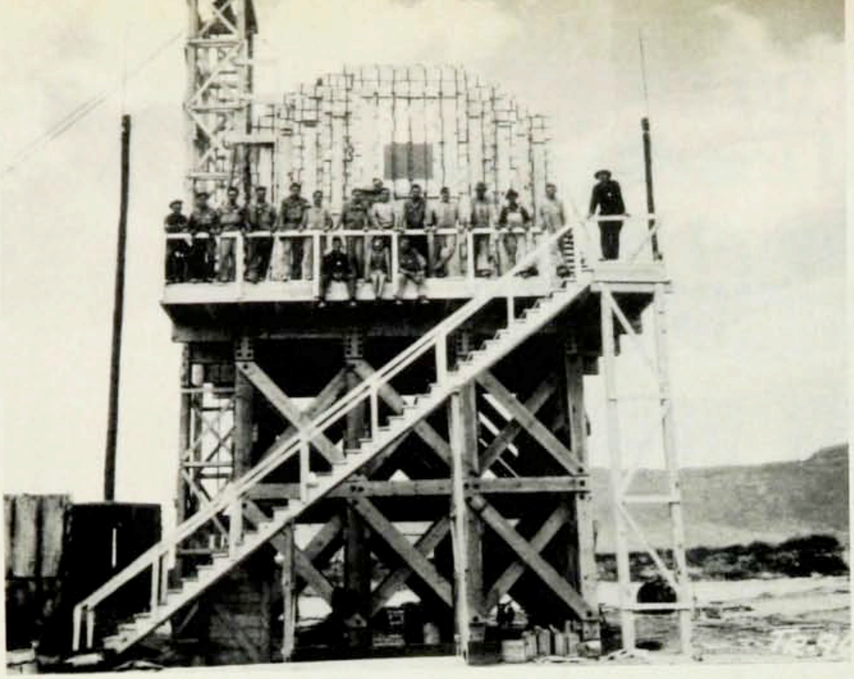


**One hundred tons** of high explosives placed on a 25-ft tower were fired at the Trinity test site on 7 May 1945.

duced background counts. We were more concerned with this background than with possible health effects. In fact, we had no survey instruments to determine the radiation dose either due to radioactivity or due to x rays or neutrons. There was no shielding between the neutron source and the experimenters, although there was some lead between the experimenters and the acceleration tube.

### Demonstrating fission

After hearing Rosenfeld's report on 16 January, Kanner and I discussed, as we walked back to the Graduate College, where we were living, whether we should follow up on the exciting news we had just heard. We were both anxious to work on our PhD theses and concluded it was unwise to divert our activities in another direction. But our decision to continue our research was soon overruled when Ladenburg brought Bohr to the laboratory. Bohr wanted us to demonstrate the fission process by observing neutron-induced fission in an ionization chamber. C. C. Van Voorhis had a vacuum system in which he could sputter uranium metal, and he quickly prepared a thin uranium target. When we put it into an ionization chamber, the neutron-induced fission events could be easily observed. Actually this was the first observation of fast-neutron-induced fission. In his first experiments, Frisch had used thermal neutrons to observe fission in an ionization chamber.[1]

During the next few months we had frequent discussions with Bohr and John Wheeler, who were working on the theory of fission. They suggested many more experiments than we could do to test various aspects of their theory. In his article Stuewer quotes Bohr in a letter written to Frisch on 24 January 1939:

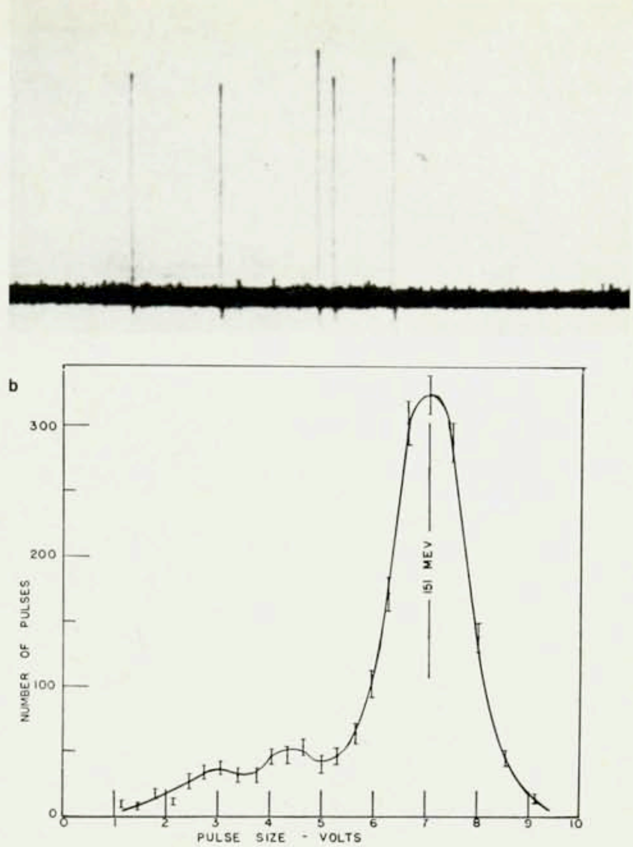
> The physicists here at the institute are very caught up in the whole question, and I already have seen preparations for experiments to detect radioactive matter of very short halflife, the appearance of which should be an immediate result of the new type of splitting of the nucleus.

This refers to our activity—initiated by Bohr—during the week following his arrival at Princeton. Louis Turner, a molecular spectroscopist, had become very interested in the new discovery and joined our effort to look for short-lived activities. We soon observed the activities Bohr was looking for, and we sent a note[2] to the *Physical Review* on 26 April 1939; it appeared on 15 May 1939—that is, in the remarkably short time of 19 days. When I became an editor of the *Physical Review* in 1972, I wished I could achieve such short publication times, but in spite of all the modern facilities, it takes almost as many weeks to publish a paper now as it took days 50 years ago. The observation of the short-lived activities resulted in my first publication.

Our next project was motivated by Bohr's surprising prediction that fission induced by slow neutrons occurs in the rare isotope $U^{235}$, while fission induced by fast neutrons occurs primarily in $U^{238}$. Although others could easily measure the fission cross section for slow neutrons, we were in a good position to measure the fission cross section for fast neutrons. Bohr and Wheeler urged us to perform such a measurement. Even today the measurement of a reaction cross section for fast neutrons is a difficult task, and often measurements performed at different laboratories differ by large factors. The principal difficulty is the determination of the neutron fluence. Ladenburg and his coworkers had previously measured the neutron output of the source, so that we could estimate the fission cross section, but we were not really sure that our results would give more than the order of magnitude. At a neutron energy of 2.5 MeV we found[3] a fission cross section for thorium of $0.1\times10^{-24}$ cm$^2$, and for uranium a fission cross section of $0.5\times10^{-24}$ cm$^2$, with an uncertainty of $\pm25\%$. Recent measurements of these cross sections give 0.12 barn for thorium and 0.54 barn for uranium, values that agree better with the 1939 experiments than I would have expected.

The measurement of the fast-neutron fission cross section was quoted by Bohr and Wheeler[4] in their famous article on the mechanism of nuclear fission as proof that $U^{238}$ is responsible for fast-neutron fission. They used the fact that the cross sections did not vary appreciably for neutron energies between 2.1 and 3.1 MeV to estimate the energy that had to be supplied to induce fission in $Th^{232}$ and $U^{238}$ nuclei.

Our last effort in studying fission was a measurement of the total kinetic energy of the fission fragments. Others had measured the energy of individual fission fragments; we wanted to measure the total energy by observing both fragments simultaneously. This required a uranium foil thin enough that the fragments would lose only a small amount of energy in passing through it. Van Voorhis prepared such a foil for us, and we measured the energy distribution of the fragments in an ionization chamber. The results[5] were consistent with the calculations Bohr and Wheeler had carried out and agree with currently accepted values.




**Fragments** from uranium fission induced by fast neutrons are indicated by sharp (150-MeV) pulses in the photographic record of galvanometer deflections (above). The graph below shows the pulse-height distribution. The large peak is due to pairs of fragments, the two smaller peaks are due to single fragments.[4]

The discovery of fission attracted the interest of the press, where speculations about the possible military applications of the new discovery made headlines. Hence our experiments brought us in contact with newspaper reporters, an experience that appeared glamorous at the time.

### Wartime research

Kanner and I had originally been reluctant to get involved in experiments on fission, because we wanted to work on our theses. In view of the results we had obtained, Ladenburg agreed to let Kanner use the fission experiments as his thesis. He in turn agreed to stay on for another year to help me with my thesis, as it was almost impossible for one person alone to operate the accelerator and take data. I completed my thesis research—measurements of the angular distributions of neutrons scattered by hydrogen, deuterium and helium—during my third year at Princeton.[6] Although Ladenburg was my major professor, the ideas and the understanding were largely provided by Wheeler.

In 1940, when I got my degree, some members of the Princeton faculty had moved to the MIT Radiation Laboratory. I was invited to stay on at Princeton for a year as an instructor, an opportunity I welcomed because there were few jobs for physicists available. During the 1940–41 academic year I continued experiments with fast neutrons and measured elastic and inelastic scattering by several intermediate and heavy elements. By 1941 questions were raised about whether, because of the war, my measurements should be published and whether as a recent immigrant from Germany I should be allowed to perform such measurements. Ladenburg finally informed me reluctantly that I could no longer work in his laboratory. In the meantime I was trying to find another job. One of the few openings was at the University of Kansas. James D. Stranathan, the chairman of the Kansas physics department, interviewed many applicants during the 1941 Washington APS meeting. I did not consider my chances good of being offered what appeared to be one of the very few open positions. Fortunately Stranathan was just writing a textbook on modern physics[7] in which he quoted my work on fission, and I think that was the reason he offered me the position. I moved to Lawrence, Kansas, in the fall of 1941. After the attack at Pearl Harbor I found myself under the restrictions applicable to enemy aliens, the most unpleasant of which was that I was not allowed to leave the city limits of Lawrence without special permission from the US Attorney.

Although hardly any experimental equipment was available in the Kansas physics department, I was able to do some work on charged-particle detectors with a couple of graduate students and a colleague.

At the end of October 1942 I received an unexpected letter from Wheeler on University of Chicago—Metallurgical Laboratory stationery. He wrote:

> I am writing to ask if there is enough possibility of your being able to join this laboratory to make it worthwhile for us to make you a formal offer. . . . There might be a little delay in getting your clearance through, but we have done such things before.

I responded:

> Your letter came as a great surprise to me, since I did not know that I was eligible to do defense work. As you can realize, I am most anxious to do anything which might be of direct value to the war effort.

On 6 November Wheeler responded that he had referred my letter to John Manley and that further communications would come from Manley or Arthur Compton. Shortly thereafter Manley wrote that he had asked the Internal Security District to send me an Alien Questionnaire, which I returned to Manley on 16 November 1942. Although various friends told me that War Department investigators were asking them about me, there was no indication that my clearance would be approved. In fact, the director of personnel at the Metallurgical Laboratory considered approval unlikely. In the meantime Henry Smyth, the chairman of the Princeton physics department, as well as Ladenburg tried to persuade me to return to Princeton to teach in a Navy program.

In view of my problems as an enemy alien I suggested that the University of Chicago might ask the War Department to help me become a US citizen. Naturalization was difficult for an enemy alien in wartime, but Sam Allison at the Metallurgical Laboratory immediately agreed to help. A letter to the Immigration and Naturalization

**Building Z** at Los Alamos, which housed a Cockcroft–Walton accelerator that had been moved from the University of Illinois, is shown in this 1944 photo. The accelerator produced neutrons of energies around 2.5 MeV, not far from the energies of neutrons produced in fission.

Service was written on University of California stationery and signed by the University Representative, University War Council. On 25 May 1943 Manley wrote that he had made contact with the War Department to expedite both my clearance and my naturalization. A confusing development was that Manley's letter came from Santa Fe, New Mexico, the supporting letter came from Berkeley and the rest of the correspondence was from Chicago. In any event, the intervention by the War Department resulted in immediate action by the Immigration and Naturalization Service. They agreed to hold a special hearing in Lawrence if I could persuade the judge of the District Court to interrupt his summer vacation. After a colleague who knew the judge arranged this, I was admitted to citizenship on 3 July 1943. The following three months were chaotic. By that time the students at the university were mostly soldiers and sailors, all of whom had to take physics, so that I had to give lectures several times a day. The university had to provide this instruction and was unwilling to let me leave. In the meantime research teams at both Chicago and Los Alamos wanted me. I wrote to Wheeler for advice. His answer, dated 9 July 1943, came from Wilmington, Delaware, on Du Pont stationery with the notation Explosives Department—TNX, another confusing development. He wrote:

> You ask whether I would recommend that you go to Chicago or Santa Fe. This is a very difficult question to answer.... The experimental instruments available for use are of a quite new type at Chicago. If you went there, you would naturally work with Fermi.... It is an easy transition from Chicago to Santa Fe, but once one is at Sante Fe it is difficult to be released to go back to Chicago.

I interpreted Wheeler's letter as advising me to go to Chicago. On the other hand, he had originally suggested that I work with Manley, who had moved to Los Alamos. Manley had taken the initiative that had resulted in my naturalization; hence I was inclined to want to work with him even though the prospect of working with Enrico Fermi was difficult to ignore.

The first attempt to get me released from Kansas was made on 3 July, the date of my naturalization, by Arthur L. Hughes, who was then personnel director at Los Alamos and a well-known physicist. He wrote again on 13 July:

> We can ask the highest official in Washington connected with our project to apply pressure to your chancellor. We seldom use this procedure.

On 19 July Hughes wrote once again. This time his stationery bore the heading "US Engineer Project 'Y,' " and he now signed using the title "Assistant Director." He wrote:

> We have written to Dr. James B. Conant in Washington asking for his assistance in getting an early release for you.

On 31 July Hughes wrote:

> Dr. Conant is going to talk to General [Leslie] Groves who in turn would get the office of the Secretary of War to act.

On 17 August Hughes wrote (this time the letter was classified "Restricted"):

> Dr. Conant tried to get your Chancellor to release you but was completely unsuccessful. He suggested as a last resort that we call on General Groves to request the Secretary of War to ask for your release.... We have adopted this extreme approach in the case of only one other man.

Groves told me later that this person was Norman Ramsey.

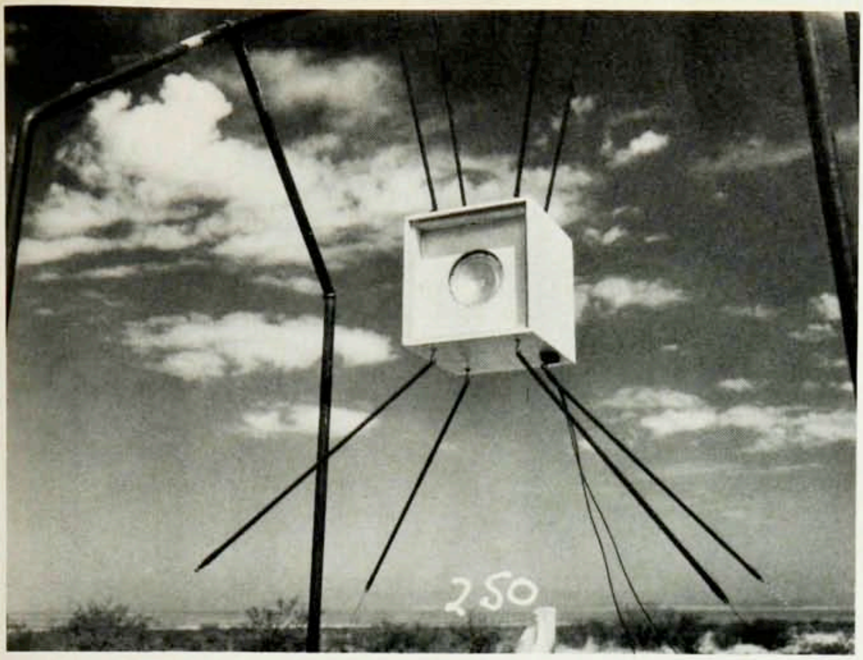
On 30 August Hughes wired me, "After discussing your case with General Groves' office I am at liberty to ask you to come here as soon as possible," but on 6 September he wired, "On General Groves' instruction do not move from Kansas until you get signals from his office." On 12 September Groves wrote:

> I have had several telephone conversations with Chancellor [Deane] Malott, and in the course of these have promised him that no action would be taken by me until after he had received a letter from the Secretary of War.

My reaction to this letter was that surely in the middle of the war the Secretary of War had more important tasks than to write a letter on behalf of a recently naturalized instructor at the University of Kansas and that the chancellor had succeeded in talking the general into accepting a condition that he could not meet, but I was wrong. The letter from the Secretary of War really arrived, and a few days later I headed west.

## Los Alamos

I had received another Restricted document ("Within the meaning of the Espionage Act, the contents of this document...") that explained living and employment conditions at Los Alamos: "Rent for furnished, equipped single rooms including utilities is $13 a month. Room service is $2 extra a month." The salary of a PhD with three years' experience was $355 a month, which was far more than my salary in Kansas. My instructions were to report to 109 East Palace Avenue in Santa Fe. This turned out to be a small office in the back of a one-story adobe building. Here I received a pass to Los Alamos and instructions on how to get there. I found the drive to the mesa on which Los Alamos is




**Loudspeaker** used to record the arrival of the sound wave and of the shock wave from the first nuclear explosion. Ten such loudspeakers were used to measure the shock wave's velocity.

located frightening. The road was narrow, with sharp switchbacks. I encountered large graders that were being used to widen the road, and I had to back down around the switchbacks. In addition, it was a hot day and my car's engine vapor-locked at the high altitude so that I had trouble maneuvering. I finally reached the top of the mesa, where armed military police inspected my pass.

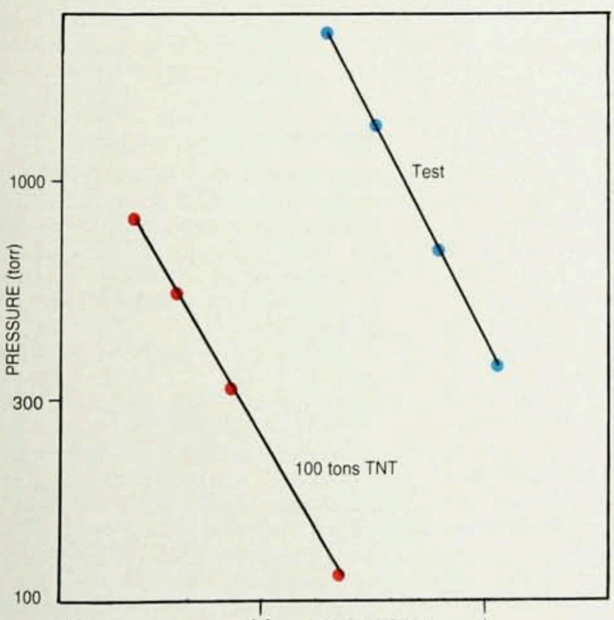
At Los Alamos I was assigned, as I had expected, to Manley's group. He had brought to Los Alamos from the University of Illinois an accelerator of the same type as the one I had used at Princeton, and the measurements in progress[8] were very similar to those I had performed during my last year at Princeton. The group's goal was to find a suitable material to serve as a neutron reflector for a nuclear explosive. I had vaguely suspected that something like this was the reason I could not continue my work at Princeton, but I had not guessed the full story. I soon learned what the new type of instrument at Chicago was that Wheeler had written about: Fermi had observed the first chain reaction ten months before I arrived at Los Alamos.

Manley's group was hardworking and congenial. Some group members whom I met on the day of my arrival have remained my friends ever since. We initially used neutrons of the same energy that I had used at Princeton, but we soon extended the measurements to lower energies. For these experiments we used electrostatic accelerators that had been built under Ray Herb's direction at the University of Wisconsin and moved to Los Alamos. I had heard about these machines, and the possibility of varying the energy easily over a wide range made them unique tools.

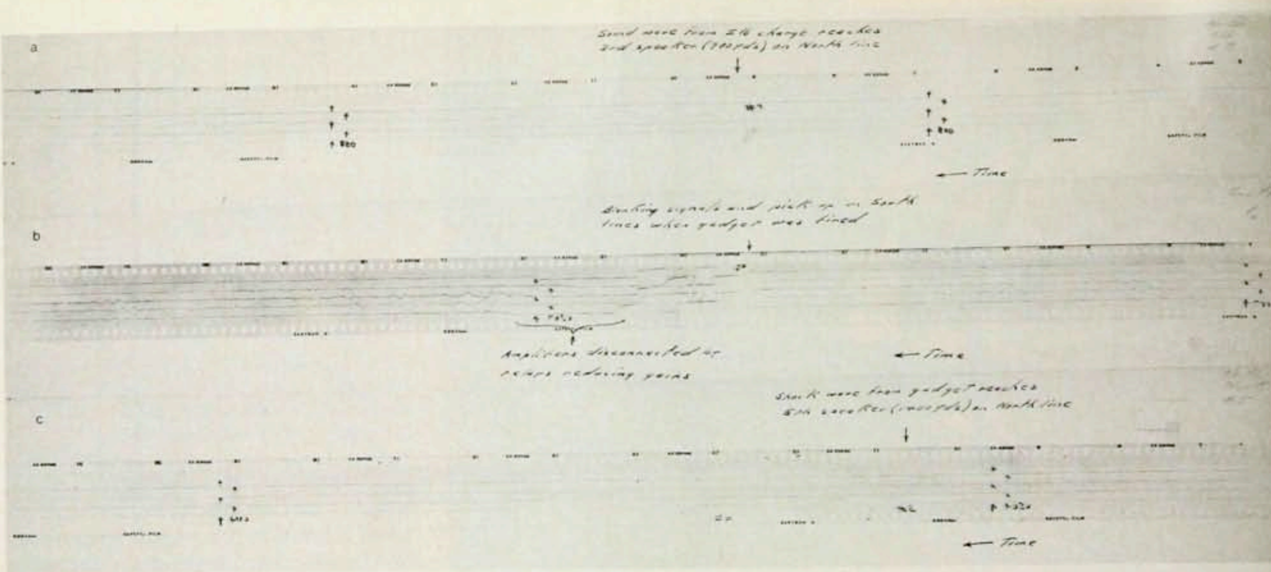
The cooperative spirit at Los Alamos made for an unusually effective operation. Data were analyzed by the theoretical group as soon as they were obtained. Viki Weisskopf did most of the analysis, but other theorists were also involved. The electronic equipment had improved enormously, largely because of advances made at the MIT Radiation Laboratory. There was always a staff of electronics technicians available to take care of all problems. Many of these technicians were young draftees of exceptional ability, and some of them became distinguished physicists.

When samples of $U^{235}$ and $Pu^{239}$ became available, we were assigned to study the properties of fissile nuclides. In particular, we measured fast-neutron multiplications of subcritical amounts of these materials.

The design of the nuclear explosive was completed toward the end of 1944. Early in 1945 Manley was given the task of performing blast measurements at the first bomb test, which was scheduled for the middle of the year. None of us had any experience with such measurements, and we spent much time learning about the new field—especially from British experts who had come to help us. While techniques for blast measurements of conventional explosions were well developed, the measurements we were asked to perform had special difficul-




**Peak gauge pressure** as a function of distance from the explosive, measured for the firing of 100 tons of high explosives and for the nuclear explosion. Comparison of the two yields the TNT explosive equivalent of the nuclear device.




**Oscilloscope record** of the arrival of sound and shock waves from the Trinity explosion. Each line records signals from a different loudspeaker. **a:** Arrival of the sound wave from a 5-lb calibration explosion, set off just before the nuclear explosion. **b:** The electromagnetic pulse from the nuclear explosion is picked up as noise on the cables from the speakers and a mechanical relay reduces the gain of the amplifiers (arrows). **c:** The shock wave from the nuclear explosion arrives at the fifth north detector (arrow).

ties. To provide data in the event of a very low explosive yield, our instruments had to be capable of giving results for energy releases differing by a factor of at least 100. Furthermore, the nuclear explosion was expected to produce very strong electromagnetic signals that would disable most electronic equipment, and intense thermal radiation that was likely to burn flammable materials; of course, these radiations would arrive at the blast detectors long before the shock wave. Another problem was that previous blast measurements had usually been performed near sea level, while our test site was at high altitude.

My assignment was to measure the velocity of propagation of the shock wave as a function of distance from the explosion. The ratio of the velocity of the shock wave to the sound velocity is related to the peak pressure of the shock wave by the Rankine–Hugoniot equation. We used loudspeakers as detectors for the arrival of the shock wave. The motions of the speaker coil in the magnetic field would induce a large signal that could be transmitted to a shelter, where a movie camera would photograph the signal as displayed on an oscilloscope screen. Five loudspeakers were placed in a line at different distances from ground zero, so that four average velocities could be measured. Two such strings of speakers were arranged in opposite directions from the tower on which the explosive would be detonated.

An accurate knowledge of the velocity of sound at the time of the explosion was necessary to deduce the peak pressure. Because the sound velocity depends on temperature and wind velocity, which vary with time, we wanted to measure the sound velocity immediately before the nuclear explosion. To accomplish this we decided to set off a small (5-lb, conventional) explosion near the nuclear explosion, a request that was approved only reluctantly. As soon as the signal from the small charge was recorded, the sensitivity of the system would be reduced.

We tested our detection and analysis equipment on 7 May 1945 by setting off 100 tons of high explosives on a platform with the center of the explosive about 10 meters above the ground. All the equipment worked satisfactorily. The nuclear test, originally scheduled for 4 July 1945, had to be postponed until the 15th. After various rehearsals and dry runs on the preceding days, everybody was ready on the evening of 15 July. For the benefit of optical measurements the test was planned to be done during the night, but storms on the evening of 15 July and in the early hours of 16 July led to a long wait. The weather cleared up around 5 am, and the explosion occurred at 5:30 am. I was watching the oscilloscope in a shelter 10 000 yards from the tower, and observed first the arrival of the signals from the explosion of the 5-lb charge. Then the shelter suddenly lit up as the nuclear explosion occurred. The arrival of the shock wave at the loudspeakers could be seen on the oscilloscope. About 20 seconds later the shock wave hit our shelter.

We sent the film back to Los Alamos for developing and analysis. The data yielded an energy release equivalent to 10 000 ± 1000 tons of TNT, a value close to expectations.[9]

With the completion of this measurement my activities initiated by the news of the discovery of fission ended, and I returned to research in fast-neutron physics. After the war the "Long Tank," the electrostatic accelerator that we used at Los Alamos, was returned to Wisconsin. It was the ideal tool for extending my research in neutron physics to other neutron energies. Hence I was delighted to be invited to join the Wisconsin faculty and to be able to continue my research there.

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