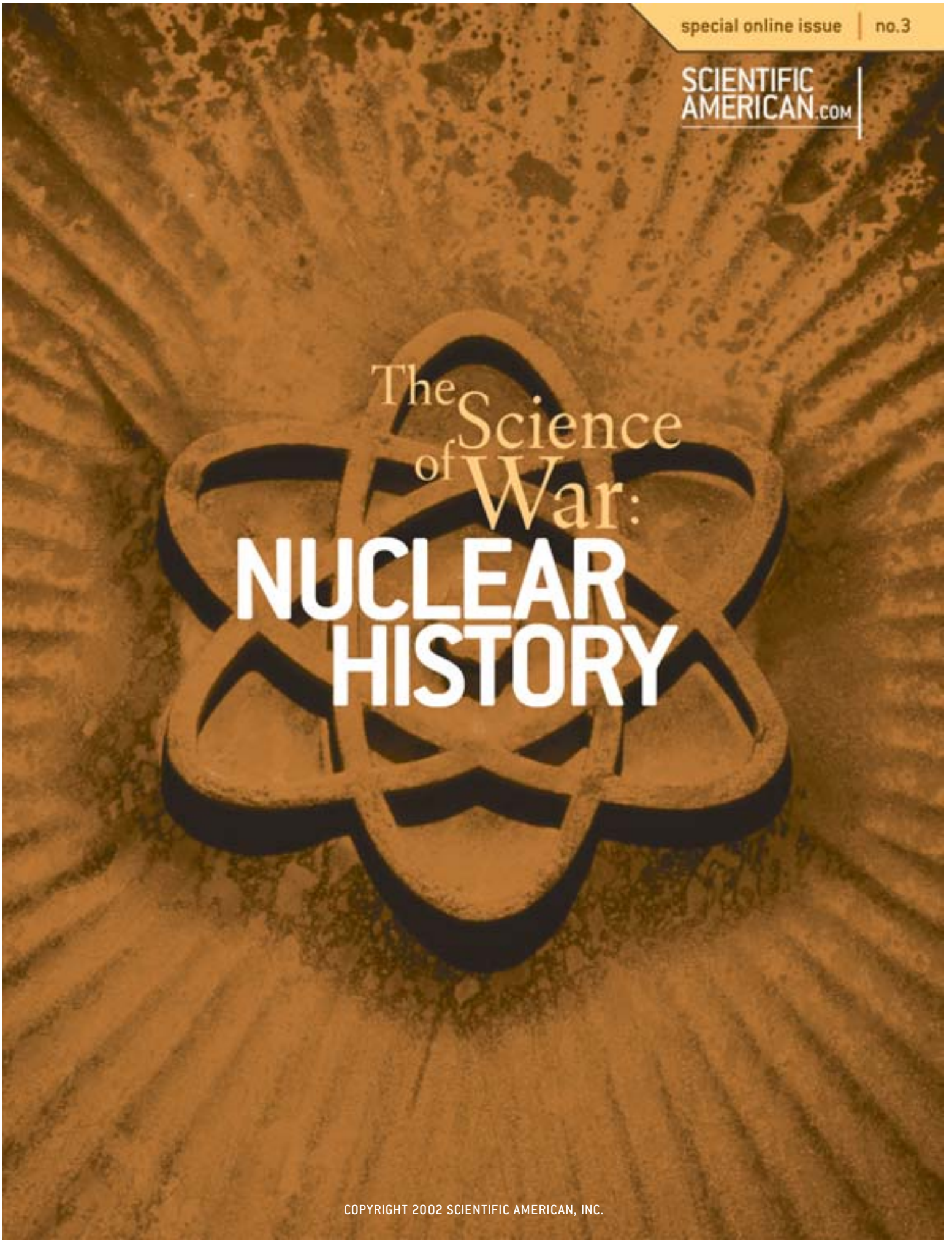
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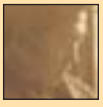
The Science of War: NUCLEAR HISTORY

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The unleashed power of the atom has changed everything save our modes of thinking, and thus we drift toward unparalleled catastrophes," Albert Einstein wrote in 1946. Indeed, the development of nuclear weapons utterly transformed human warfare, as the mass destruction wreaked by bombs dropped on Japan a year earlier made chillingly clear. Yet devastating though the outcomes often were, this was a time of extraordinary discoveries in the field of physics. Scientific American has long covered the science of war. Our first special online issue housed a collection of articles about weapons. Now part two of our war anthology brings together recent contributions from experts on nuclear history.

In this issue, leading authorities discuss the science—and the scientists—that delivered us into the nuclear age, from Lise Meitner's long-overlooked contributions to the discovery of nuclear fission to Manhattan Project member Philip Morrison's reflections on the first nuclear war and how a second must be avoided. Other articles probe such topics as the contentious relationship between atomic bomb collaborators Enrico Fermi and Leo Szilard, a mysterious meeting between Werner Heisenberg and Niels Bohr, and the unlikely achievements of physicists in wartime Japan.
–The Editors

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Physicists in Wartime Japan

During the most trying years of Japan's history, two brilliant schools of theoretical physics flourished

by Laurie M. Brown and Yoichiro Nambu

"The last seminar, given at a gorgeous house left unburned near Riken, was dedicated to [electron] shower theories.... It was difficult to continue the seminars, because Minakawa's house was burnt in April and the laboratory was badly destroyed in May. The laboratory moved to a village near Komoro in July; four physics students including myself lived there. Tatuoki Miyazima also moved to the same village, and we continued our studies there towards the end of 1945."

—Satio Hayakawa, astrophysicist

Between 1935 and 1955 a handful of Japanese men turned their minds to the unsolved problems of theoretical physics. They taught themselves quantum mechanics, constructed the quantum theory of electromagnetism and postulated the existence of new particles. Much of the time their lives were in turmoil, their homes demolished and their bellies empty. But the worst of times for the scientists was the best of times for the science. After the war, as a numbed Japan surveyed the devastation, its physicists brought home two Nobel Prizes.

Their achievements were all the more remarkable in a society that had encountered the methods of science only decades earlier. In 1854 Commodore Matthew Perry's warships forced the country open to international trade, ending two centuries of isolation. Japan realized that without modern technology it was militarily weak. A group of educated samurai forced the ruling shogun to step down in 1868 and reinstated the emperor, who had until then been only a figurehead. The new regime sent young men to Germany, France, England and America to study languages, science, engineering and medicine and founded Western-style universities in Tokyo, Kyoto and elsewhere.

Hantaro Nagaoka was one of Japan's first physicists. His father, a former samurai, initially taught his son calligraphy and Chinese. But after a trip abroad, he returned with loads of English textbooks and apologized for having taught him all the wrong subjects. At university, Nagaoka hesitated to take up science; he was uncertain if Asians could master the craft. But after a year of perusing the history of Chinese science, he decided the Japanese, too, might have a chance.

In 1903 Nagaoka proposed a model of the atom that contained a small nucleus surrounded by a ring of electrons. This "Saturnian" model was the first to contain a nucleus, discovered in 1911 by Ernest Rutherford at the Cavendish Laboratory in Cambridge, England.

As measured by victories against China (1895), Russia (1905) and in World War I, Japan's pursuit of technology was a success. Its larger companies established research laboratories, and in 1917 a quasigovernmental institute called Riken (the Institute of Physical and Chemical Research) came into being in Tokyo. Though designed to provide technical support to industry, Riken also conducted basic research.

A young scientist at Riken, Yoshio Nishina, was sent abroad in 1919, traveling in England and Germany and spending six years at Niels Bohr's institute in Copenhagen. Together with Oskar Klein, Nishina calculated the probability of a photon, a quantum of light, bouncing off an electron. This interaction was fundamental to the emerging quantum theory of electromagnetism, now known as quantum electrodynamics.

When he returned to Japan in 1928, Nishina brought with him the "spirit of Copenhagen"—a democratic style of research in which anyone could speak his mind, contrasting with the authoritarian norm at Japanese universities—as well as knowledge of modern problems and methods. Luminaries from the West, such as Werner K. Heisenberg and Paul A. M. Dirac, came to visit, lecturing to awed ranks of students and faculty.

Hiding near the back of the hall, Shinichiro Tomonaga was one of the few to understand Heisenberg's lectures. He had just spent a year and a half as an undergraduate teaching himself quantum mechanics from all the original papers. On the last day of lectures, Nagaoka scolded that Heisenberg and Dirac had discovered a new theory in their 20s, whereas Japanese students were still pathetically copying lecture notes. "Nagaoka's pep talk really did not get me anywhere," Tomonaga later confessed.

Sons of Samurai

He was, however, destined to go places, along with his high school and college classmate Hideki Yukawa. Both men's fathers had traveled abroad and were academics: Tomonaga's a professor of Western philosophy, Yukawa's

IN JANUARY 1942 author Yoichiro Nambu reads in laboratory room 305 of the physics department at the University of Tokyo. Soon after, he was drafted. When the war ended, Nambu lived in this room for three years; neighboring laboratories were similarly occupied by homeless and hungry scientists.

COURTESY OF YOICHIRO NAMBU

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a professor of geology. Both were of samurai lineage. Even before going to school, the younger Yukawa had learned the Confucian classics from his maternal grandfather, a former samurai. Later he encountered the works of Taoist sages, whose questioning attitude he would liken to the scientific pursuit. Tomonaga was inspired to study physics by hearing Albert Einstein lecture in Kyoto in 1922, as well as by reading popular science books written in Japanese.

The two men obtained their bachelor's degrees in 1929 from Kyoto University, at the start of the worldwide depression. Lacking jobs, they stayed on as unpaid assistants at the university. They taught each other the new physics and went on to tackle research projects independently. "The depression made scholars of us," Yukawa later joked.

In 1932 Tomonaga joined Nishina's lively group at Riken. Yukawa moved to Osaka University and, to Tomonaga's annoyance, confidently focused on the deepest questions of the day. (Yukawa's first-grade teacher had written of him: "Has a strong ego and is firm of mind.") One was a severe pathology of quantum electrodynamics, known as the problem of infinite self-energy. The results of many calculations were turning out to be infinity: the electron, for instance, would interact with the photons of its own electromagnetic field so that its mass—or energy—increased indefinitely. Yukawa made little progress on this question, which was to occupy some of the world's brilliant minds for two more decades. "Each day I would destroy the ideas that I had created that day. By the time I crossed the Kamo River on my way home in the evening, I was in a state of desperation," he later recalled.

Eventually, he resolved to tackle a seemingly easier problem: the nature of the force between a proton and a neutron. Heisenberg had proposed that this force was transmitted by the exchange of an electron. Because the electron has an intrinsic angular momentum, or spin, of one half, his idea violated the conservation of angular momentum, a basic principle of quantum mechanics. But having just replaced classical rules with quantum ones for the behavior of electrons and photons, Heisenberg, Bohr and others were all too willing to throw out quantum physics and assume that protons and neutrons obeyed radical new rules of their own. Unfortunately, Heisenberg's model also predicted the range of the nuclear force to be 200 times too long.

Yukawa discovered that the range of a force depends inversely on the mass of the particle that transmits it. The electromagnetic force, for instance, has infinite range because it is carried by the massless photon. The nuclear force, on the other hand, is confined within the nucleus and should be communicated by a particle of mass 200 times that of the electron. He also found that the nuclear particle required a spin of zero or one to conserve angular momentum.

Yukawa published these observations in his first original paper in 1935 in *Proceedings of the PMSJ* (Physico-Mathematical Society of Japan). Although it was written in English, the paper was ignored for two years. Yukawa had been bold in predicting a new particle—thereby defying Occam's razor, the principle that explanatory entities should not proliferate unnecessarily. In 1937 Carl D. Anderson and Seth H. Neddermeyer of the California Institute of Technology discovered, in traces left by cosmic rays, charged particles that had about the right mass to meet the requirements of Yukawa's theory. But the cosmic-ray particle appeared at sea level instead of being absorbed high up in the atmosphere, so it lived 100 times longer than Yukawa had predicted.

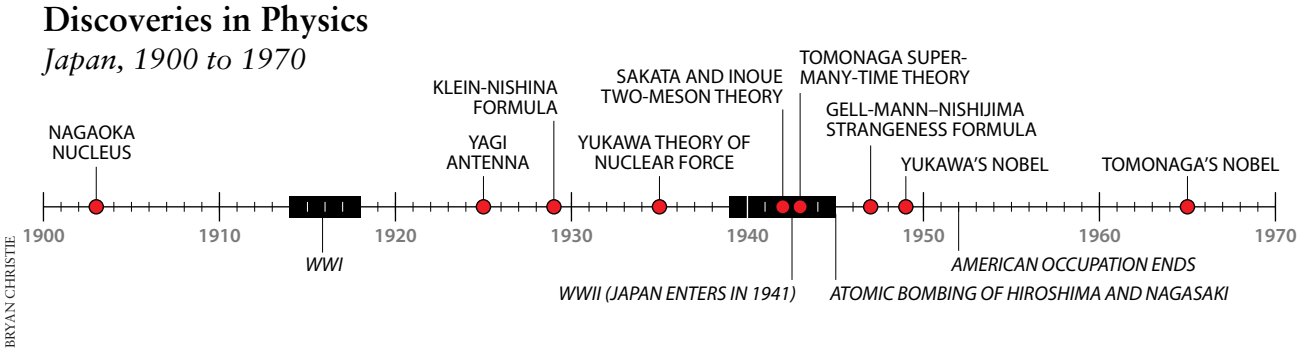
Tomonaga, meanwhile, was working with Nishina on quantum electrodynamics. In 1937 he visited Heisenberg at Leipzig University, collaborating with him for two years on theories of nuclear forces. Yukawa also arrived, en route to the prestigious Solvay Congress in Brussels. But the conference was canceled, and the two men had to leave Europe hurriedly.

War brought the golden age of quantum physics to an abrupt end. The founders of the new physics, until then concentrated in European centers such as Göttingen in Germany, scattered, ending up mainly in the U.S. Heisenberg, left virtually alone in Germany, continued at least initially to work on field theory—a generalization of quantum electrodynamics—and to correspond with Tomonaga.

A War Like No Other

By 1941, when Japan entered the world war, Yukawa had become a professor at Kyoto. His students and collaborators included two radicals, Shoichi Sakata and Mitsuo Taketani. At the time, Marxist philosophy was influential among intellectuals, who saw it as an antidote to the militarism of the imperial government. Unfortunately, Taketani's writings for the Marxist journal *Sekai Bunka* (*World Culture*) had drawn the attention of the thought police. He had been jailed for six months in 1938, then released into Yukawa's custody thanks to the intervention of Nishina. Although Yukawa remained totally wrapped up in physics and expressed no political views at all, he continued to shelter the radicals in his lab.

Sakata and Taketani developed a Marxist philosophy of science called the three-stages theory. Suppose a researcher discovers a new, inexplicable phenomenon. First he or she learns the details and tries to discern regularities. Next the sci-



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One wonders why the worst decades of the century for Japan were the most creative ones for its theoretical physicists.

entist comes up with a qualitative model to explain the patterns and finally develops a precise mathematical theory that subsumes the model. But another discovery soon forces the process to repeat. As a result, the history of science resembles a spiral, going around in circles yet always advancing. This philosophy came to influence many of the younger physicists, including one of us (Nambu).

Meanwhile, as war raged in the Pacific, the researchers continued to work on physics. In 1942 Sakata and Takeshi Inoue suggested that Anderson and Neddermeyer had not seen Yukawa's particle but instead had seen a lighter object, now called a muon, which came from the decay of the true Yukawa particle, the pion. They described their theory to the Meson Club, an informal group that met regularly to discuss physics, and published it in a Japanese journal.

Yukawa was doing war work one day a week; he never said what this entailed. (He did say that he would read the *Tale of Genji* while commuting to the military lab.) Tomonaga, who had become a professor at the Tokyo Bunrika University (now called the University of Tsukuba), was more involved in the war effort. Together with Masao Kotani of the University of Tokyo, he developed a theory of magnetrons—devices used in radar systems for generating electromagnetic waves—for the navy. Through the hands of a submarine captain he knew, Heisenberg sent Tomonaga a paper on a technique he had invented for describing the interactions of quantum particles. It was in essence a theory of waves, which Tomonaga soon applied to designing radar waveguides.

At the same time, Tomonaga was tackling the problem of infinite self-energy that Yukawa had given up. To this end, he developed a means of describing the behavior of several interacting quantum particles, such as electrons, moving at near the speed of light. Generalizing an idea due to Dirac, he assigned to each particle not just space coordinates but also its own time coordinate and called the formulation "super-many-time theory." This work, which became a powerful framework for quantum electrodynamics, was published in 1943 in Riken's science journal.

By this time most students had been mobilized for war. Nambu was among those assigned to radar research for the army. (Intense rivalry between the army and the navy led each to duplicate the other's efforts). Resources were short and the technology often very primitive: the army could not develop mobile radar systems to pinpoint enemy targets. Nambu was once handed a piece of Permalloy magnet, about three by three inches, and told to do what he could with it for aerial submarine detection. He was also told to steal from the navy Tomonaga's paper on waveguides, labeled "Secret," which he accomplished by visiting an unsuspecting professor [see "Strings and Gluons—The Seer Saw Them All," by Madhusree Mukerjee, News and Analysis; Scientific American, February 1995].

(Curiously, Japan's past technical contributions included excellent magnetrons designed by Kinjiro Okabe and an antenna; the latter, invented by Hidetsugu Yagi and Shintaro Uda in 1925, still projects from many rooftops. The Japanese armed forces learned about the importance of the "Yagi array" from a captured British manual.)

Younger physicists around the Tokyo area continued their studies when they could; professors from the University of Tokyo, as well as Tomonaga, held special courses for them on Sundays. In 1944 a few students (including Satio Hayakawa, whose quote begins this article) were freed from war research and returned to the university campus. Even so, times were difficult. One student's house was burned down, another was drafted, and a third had his house burned down just before he was drafted. The venue for the seminars shifted several times. Tomonaga, who had always been physically weak, would sometimes instruct his students while lying sick in bed.

Meanwhile Nishina had been instructed by the army to investigate the possibility of making an atomic bomb. In 1943 he concluded that it was feasible, given enough time and money. He assigned a young cosmic-ray physicist, Masa Takeuchi, to build a device for isolating the lighter form of uranium required for a bomb. Apparently Nishina thought the project would help keep physics research alive for when the war ended. Taketani, back in prison, was also forced to work on the problem. He did not mind, knowing it had no chance of success.

Across the Pacific, the Manhattan Project was employing some 150,000 men and women, not to mention a constellation of geniuses and $2 billion. In contrast, when the Japanese students realized they would need sugar to make uranium hexafluoride (from which they could extract the uranium) they had to bring in their own meager rations. A separate effort, started by the navy in 1943, was also far too little, too late. By the end of the war, all that the projects had produced was a piece of uranium metal the size of a postage stamp, still unenriched with its light form.

And two atom bombs had exploded in Japan. Luis W. Alvarez of the University of California at Berkeley was in the aircraft that dropped the second bomb over Nagasaki, deploying three microphones to measure the intensity of the blast. Around these instruments he wrapped a letter (with two photocopies) drafted by himself and two Berkeley colleagues, Philip Morrison and Robert Serber. They were addressed to Riokichi Sagane, Nagaoka's son and a physicist in Tomonaga's group. An experimenter, Sagane had spent two years at Berkeley learning about cyclotrons, enormous machines for conducting studies in particle physics. He had become acquainted with the three Americans who now sought to inform him of the nature of the bomb. Although the letter was

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recovered by the military police, Sagane learned of it only after the war. After the Japanese surrender in August 1945, the country was effectively under American occupation for seven years. General Douglas MacArthur's administration reformed, liberalized and expanded the university system. But experimental research in nuclear and related fields was essentially prohibited. All cyclotrons in Japan were dismantled and thrown into the sea, for fear that they might be used to research an atomic bomb.

In any case, the miserable economy did not allow the luxury of experimental research. Tomonaga was living with his family in a laboratory, half of which had been bombed to bits. Nambu arrived at the University of Tokyo as a research assistant and lived for three years in a laboratory, sleeping on a straw mattress spread over his desk (and always dressed in military uniform for lack of other clothes). Neighboring offices were similarly occupied, one by a professor and his family.

A Hungry Peace

Getting food was everyone's preoccupation. Nambu would sometimes find sardines at Tokyo's fish market, which rapidly produced a stench because he had no refrigerator. On weekends he would venture to the countryside, asking farmers for whatever they could offer.

Several other physicists also used the room. One, Ziro Koba, was working with Tomonaga's group at Bunrika on the self-energy problem. Some of the officemates specialized in the study of solids and liquids (now called condensed-matter physics) under the guidance of Kotani and his assistant Ryogo Kubo, who was later to attain fame for his theorems in statistical mechanics. The young men taught each other what they knew of physics and regularly visited a library set up by MacArthur, perusing whatever journals had arrived.

At a meeting in 1946 Sakata, then at Nagoya University—whose physics department had moved to a suburban primary school—proposed a means of dealing with the infinite self-energy of the electron by balancing the electromagnetic force against an unknown force. At the end of the calculation, the latter could be induced to vanish. (At about the same time, Abraham Pais of the Institute for Advanced Study in Princeton, N.J., proposed a similar solution.) Although the method had its flaws, it eventually led Tomonaga's group to figure out how to dispose of the infinities, by a method now known as renormalization.

This time the results were published in *Progress of Theoretical Physics,* an English-language journal founded by Yukawa in 1946. In September 1947 Tomonaga read in *Newsweek* about a striking experimental result obtained by Willis E. Lamb and Robert C. Retherford of Columbia University. The electron in a hydrogen atom can occupy one of several quantum states; two of these states, previously thought to have identical energies, actually turned out to have slightly different energies.

Right after the finding was reported, Hans Bethe of Cornell University had offered a quick, nonrelativistic calculation of the "Lamb shift," as the energy difference came to be known. The effect is a finite change in the infinite self-energy of the electron as it moves inside an atom. With his students, Tomonaga soon obtained a relativistic result by correctly accounting for the infinities.

Their work strongly resembled that being done, almost at the same time, by Julian S. Schwinger of Harvard University. Years later Tomonaga and Schwinger were to note astonishing parallels in their careers: both had worked on radar, wave propagation and magnetrons as part of their respective war efforts, and both used Heisenberg's theory to solve the same problem. The two shared a Nobel Prize with Richard Feynman in 1965 for the development of quantum electrodynamics. (Feynman had his own idiosyncratic take—involving electrons that moved backward in time—which Freeman Dyson of the Institute for Advanced Study later showed was equivalent to the approach of Tomonaga and Schwinger.) And both Tomonaga's and Schwinger's names mean "oscillator," a system fundamental to much of physics.

At about the time the Lamb shift was reported, a group in England discovered the decay of the pion to the muon in photographic plates exposed to cosmic rays at high altitude. The finding proved Inoue, Sakata and Yukawa to have been spectacularly correct. After the dust settled, it became clear that Yukawa had discovered a deep rule about forces: they are transmitted by particles whose spin is always an integer and whose mass determines their range. Moreover, his tactic of postulating a new particle turned out to be astoundingly successful. The 20th century saw the discovery of an abundance of subatomic particles, many of which were predicted years before.

In 1947 new particles began to show up that were so puzzling that they were dubbed "strange." Although they appeared rarely, they often did so in pairs and, moreover, lived anomalously long. Eventually Murray Gell-Mann of the California Institute of Technology and, independently, Kazuhiko Nishijima of Osaka City University and other Japanese researchers discovered a regularity behind their properties, described by a quantum characteristic called "strangeness." (Discerning this pattern was the first step in the three-stages theory.)

In subsequent years Sakata and his associates became active in sorting through the abundance of particles that were turning up and postulated a mathematical framework, or triad, that became the forerunner of the quark model. (This framework formed the second stage. At present, high-energy physics, with its precise theory of particles and forces known as the Standard Model, is in the third and final stage.)

Meanwhile physicists in Japan were renewing ties with those in the U.S. who had made the atomic bomb. Their feelings toward the Americans were ambiguous. The carpet bombings of Tokyo and the holocausts in Hiroshima and Nagasaki had been shocking even for those Japanese who had opposed the war. On the other hand, the occupation, with its program of liberalization, was relatively benevolent. Perhaps the deciding factor was their shared fascination for science.

Reconciliation

Dyson has described how, in 1948, Bethe received the first two issues of *Progress of Theoretical Physics*, printed on rough, brownish paper. An article in the second issue by Tomonaga contained the central idea of Schwinger's theory. "Somehow or other, amid the ruin and turmoil of the war, Tomonaga had maintained in Japan a school of research in theoretical physics that was in some respects ahead of anything existing elsewhere at that time," Dyson wrote. "He had pushed on alone and laid the foundations of the new quantum electrodynamics, five years before Schwinger and without any help from the Columbia experiments. It came to us as a voice out of the deep." J. Robert Oppenheimer, then director of the Insti-

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YOICHIRO NAMBU

GROUP SNAPSHOT taken in Rochester, N.Y., around 1953 features Japanese researchers with physicist Richard Feynman. Masatoshi Koshiba (*back row, left*) went on to design the Kamiokande facility; the others became prominent theorists. The picture was taken by Nambu (*front row, center*), whose skills lay in areas other than photography.

tute for Advanced Study, invited Yukawa to visit. He spent a year there, another at Columbia, and received the Nobel Prize in 1949. Tomonaga also visited the institute and found it extremely stimulating. But he was homesick. "I feel as if I am exiled in paradise," he wrote to his former students. He returned after a year to Japan, having worked on a theory of particles moving in one dimension that is currently proving useful to string theorists.

From the early 1950s, younger physicists also began to visit the U.S. Some, such as Nambu, stayed on. To an extent mitigating this brain drain, the expatriates retained ties with their colleagues in Japan. One means was to send letters to an informal newsletter, *Soryushiron Kenkyu,* which was often read aloud during meetings of a research group that succeeded the Meson Club. In 1953 Yukawa became the director of a new research institute at Kyoto, now known as the Yukawa Institute for Theoretical Physics.

In the same year he and Tomonaga hosted an international conference on theoretical physics in Tokyo and Kyoto. Fifty-five foreign physicists attended, including Oppenheimer. It is said that Oppenheimer wished to visit the beautiful Inland Sea but that Yukawa discouraged him, feeling that Oppenheimer would find it too upsetting to see Hiroshima, which was nearby. Despite their lifelong immersion in abstractions, Yukawa and Tomonaga became active in the antinuclear movement and signed several petitions calling for the destruction of nuclear weapons. In 1959 Leo Esaki, a doctoral student at the University of Tokyo, submitted a thesis on the quantum behavior of semiconductors, work that eventually led to the development of transistors. He would bring home a third Japanese Nobel in physics, shared with Ivar Giaever and Brian D. Josephson, in 1973.

One wonders why the worst decades of the century for Japan were the most creative ones for its theoretical physicists. Perhaps the troubled mind sought escape from the horrors of war in the pure contemplation of theory. Perhaps the war enhanced an isolation that served to prod originality. Certainly the traditional style of feudal allegiance to professors and administrators broke down for a while. Perhaps for once the physicists were free to follow their ideas.

Or perhaps the period is just too extraordinary to allow explanation.

The Authors

LAURIE M. BROWN and YOICHIRO NAMBU often collaborate on projects involving the history of Japanese physics. Brown is professor emeritus of physics at Northwestern University and has turned his interests in the past two decades to the history of physics. He is the author or co-author of eight books on the subject. Nambu is professor emeritus at the University of Chicago. He is responsible for several key ideas in particle theory and has received the Wolf Prize, the Dirac Medal, the National Medal of Science, the Order of Culture (from the Japanese government) and numerous other awards. Nambu last wrote for *Scientific American* in November 1976, on the confinement of quarks.

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Recollections of a Nuclear War

Two nuclear bombs were dropped on Japan 50 years ago this month. The author, a member of the Manhattan Project, reflects on how the nuclear age began and what the post–cold war future might hold

by Philip Morrison

Rarely do anniversaries mark the very beginning of an event. The roots of my own recollections of the Manhattan Project and the first nuclear bomb go back well before August 1945. One thick taproot extends down to 1938, when I was a graduate student in physics and a serious campus activist at the University of California at Berkeley. One night that spring, my friends and I stayed up into the chilly small hours just to catch the gravelly voice of the Führer speaking at his mass rally under the midday sun in Nuremberg. His tone was boastful, his helmeted armies on the march across national borders. His harangue, though delivered across the ocean and nine hours to the east, sounded all too nearby. It was clear that a terrible war against the Third Reich and its Axis was not far off. The concessions to Hitler made at Munich that autumn confirmed our deepest anxieties. World war was close.

A fateful coincidence in nuclear physics soon linked university laboratories to the course of war and peace. By early 1939 it became certain that an unprecedented release of energy accompanies the absorption of slow neutrons by the element uranium. I can recall the January day when I first watched in awe the green spikes on the oscilloscope screen that displayed the huge amplified pulses of electrons set free by one of the two fast-moving fragments of each divided uranium nucleus.

The first evidence for this phenomenon had been published only weeks earlier. It was indirect, even enigmatic. The radiochemists in Otto Hahn's laboratory at the Kaiser Wilhelm Institute of Chemistry in Berlin—there were none better—had found strong residual radioactivity in barium, which formed as a reaction product when uranium absorbed neutrons. Notably, a barium atom is only a little more than half the weight of an atom of uranium, the heaviest element then known. No such profound fragmentation after neutron capture had ever been seen. The identification was compelling, but its implications were obscure.

Almost at once two refugee physicists from Nazi Germany, Otto R. Frisch and Lise Meitner (Frisch's celebrated aunt), meeting in Sweden, grasped that the nucleus of uranium must have been split into two roughly equal parts, releasing along the way more energy than any nuclear reaction seen before. Soon this news was out, first carried to the U.S. by the Danish physicist Niels Bohr.

Furthermore, the division process, known as fission, seemed intrinsically likely to set free at least two neutrons each time. Two neutrons would follow the first fission, and if conditions were right, they would induce two more fission events that would in turn release four additional neutrons. Fission resulting from these four neutrons would produce eight neutrons, and so on. A geometrically growing chain of reactions (an idea Leo Szilard, a refugee from Europe newly come to New York City, alone had presciently held for some years) was now expected. The long-doubted, large-scale release of nuclear energy was finally at hand. We all knew that the energy released by the fission of uranium would be a million-fold greater pound for pound than that from any possible chemical fuel or explosive.

The World at War

Relevance to the looming war was inevitable. After hearing the news from Europe, my graduate student friends and I, somewhat naive about neutron physics but with a crudely correct vision, worked out a sketch—perhaps it would be better dubbed a cartoon—on the chalkboards of our shared office, showing an arrangement we imagined efficacious for a bomb. Although our understanding was incomplete, we knew that this device, if it could be made, would be terrible. I have no documentation of our casual drawings, but there are telling letters sent by our theorist mentor J. Robert Oppenheimer, whose own office adjoined ours. On February 2, 1939, he wrote his old friend in Ann Arbor, physicist George E. Uhlenbeck. Oppenheimer summarized the few but startling facts and closed: "So I think it really not too improbable that a ten centimeter cube of uranium deuteride…might very well blow itself to hell."

In time, just that would happen, although the process was more complicated than anyone first imagined. I am quite confident that similar gropings took place during those first weeks of 1939 throughout the small world of nuclear physics and surely in Germany, where fission was first found. By the autumn of 1939 Bohr and John A. Wheeler had published from Princeton the first full analysis of fission physics. Gallant Madrid had fallen, and the great war itself had opened. It is a matter of record that by the spring of 1940 several groups of experts had been charged to study the topic in no fewer than six countries: Germany, France (as a nation, soon to become a prisoner of war), Britain, the Soviet Union, the U.S. and Japan. It was certainly not statesmen or military leaders who first promoted the wartime potential of the fission process, but physicists in all these countries. In the U.S., for example, Albert Einstein signed the famous letter to President Franklin D. Roosevelt, just as the war began, encouraging him to pursue the

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development of nuclear weapons.

By the end of 1941 all those powers, and Italy, too, were immersed in war, as China and Japan long had been. Physics, of course, was fully caught up in the sudden, sweeping American mobilization. By then I was a physics instructor at the University of Illinois at Urbana-Champaign, where I had moved in 1941 to fill an opening left by two of my Berkeley physicist friends, as first one and then his replacement had come and gone again, both bound for some undisclosed war work. In 1942 most male students marched singing to their classes in military formations, students at the pleasure of the draft authorities. The college year was extended to a full 12 months; we faculty members taught full tilt and embarked as well on war-directed investigations with generous federal support.

Another fateful voice now informs my memories. Every Thanksgiving the physicists of the Midwest met in Chicago. I went to their sessions in 1942. A fellow graduate of our small Berkeley group charged me by telephone to come without fail to visit him at the University of Chicago lab where he worked at the time. I entered that Gothic physics building, my appointment verified by unforeseen and incongruous armed guards, to find my friend Bob Christy sitting quietly at his desk. "Do you know what we're doing here?" he asked. I admitted that it was easy to guess: this must be the hidden uranium project to which so many others had gone. "Yes," he said, in his familiar style of calm speech, "we are making bombs."

I was startled, even hushed, by the ambitious plan with so final and fearful a goal. Christy and I talked, and a question arose: How else could our side lose the war unless it was the Germans who first made nuclear weapons? The task was indeed vital; every physicist with relevant competence—they were few enough—had to take part. I was persuaded; my wife concurred. Within weeks I was in the very same Chicago lab, learning how to assist Enrico Fermi, who was in the office next door. I had enlisted, so to speak, for the duration, like many a young soldier before me.

During the bitter war year of 1943, I became an adept neutron engineer, testing again and again detailed mock-ups of the huge reactors to be built in Hanford, Wash., along the Columbia River. I recall other lines of thought, too, within the busy circle of theorists and engineers around Eugene P. Wigner. I recognized almost as a revelation that even the small concentration of uranium found in abundantly available granite could provide enough fission fuel to power its own extraction from the massive rock and yield a large energy surplus besides. In principle only—practice does not even today support this dream—an energy source that could use as fuel the mountains themselves would far outlast all fossil fuels. I was also to propose (not alone) a detailed plan to ferret out what the Germans were in fact up to, and soon I became a technical adviser to General Leslie R. Groves's new intelligence organization in Europe—a dramatic and, in the end, worrisome sideline for a young physicist.

Building the Bomb

Here in the States, two giant industrial sites were being swiftly built to produce sufficiently large quantities of two distinct nuclear explosives, uranium and a newly discovered element, plutonium. And we all knew that somewhere—at a hidden "Site Y"—work was under way to develop a bomb mechanism that could detonate these nuclear explosives. But in mid-1944, even as the reactors along the Columbia that would produce plutonium were being completed by 40,000 construction workers, Site Y encountered an unforeseen technical crisis. The favored bomb design had been simple and gunlike: a subcritical enriched uranium bullet was fired into a matching hole in a subcritical enriched uranium target, detonating them both. Yet measurements on early samples proved that this design could not be used with plutonium, and the bulk of the bomb material the U.S. was prepared to make during the next years would be plutonium. A complex and uncertain means of assembly, known as the implosion design, examined earlier but set aside as extremely difficult, now seemed the only way open: you had to squeeze solid plutonium metal to a momentary high density with a well-focused implosion of plenty of ordinary high explosive.

By summer's end of 1944, I was living and working in Site Y amid the beautiful high mesas and deep canyons of Los Alamos, N.M., along with many other scientists and engineers. We had been urgently gathered from the whole of the wide Manhattan Project to multiply and strengthen the original Los Alamos staff, star-studded but too few to realize the novel engineering of the implosion design.

Information from German labs convinced us by the close of 1944 that the Nazis would not beat us to the bomb. In January 1945, I was working in Frisch's group, which had become skilled in assembling subcritical masses of nuclear material that could be brought together to form the supercritical mass needed for energy release. Indeed, we had the temerity to "tickle the dragon's tail" by forming a supercritical mass of uranium. We made a much subdued and diluted little uranium bomb that we allowed to go barely supercritical for a few milliseconds. Its neutron bursts were fierce, the first direct evidence for an explosive chain reaction.

By spring the lab had fixed on a design for a real plutonium implosion bomb, one worked out by Christy, and scheduled its full-scale test. Two of us from the Frisch group (I was one, physicist Marshall G. Holloway the other) had been appointed as G-engineers, the "G" short for gadget—the code name for the implosion bomb. We were fully responsible for the first two cores of plutonium metal produced. We had to specify their design in great detail; once enough plutonium compound arrived, we were charged to procure the cores from Los Alamos resources, prepare their handling and by July be ready to assemble the first test core amid the other systems of the complex weapon. By June, though, the battle with Germany was over, but the war with Japan burned more terribly than ever. We kept on toward the still uncertain bomb, in loyal duty to our country and the leaders we trusted—perhaps too much?

The Trinity Test, the first test of a nuclear bomb, went off as planned, on July 16, 1945, leaving lifelong indelible memories. None is as vivid for me as that brief flash of heat on my face, sharp as noonday for a watcher 10 miles away in the cold desert predawn, while our own false sun rose on the earth and set again. For most of the 2,000 technical people at Los Alamos—civilians, military and student-soldiers—that test was the climax of our actions. The terrifying deployment less than a month later appeared as anticlimax, out of our hands, far away. The explicit warning I had hoped for never came; the nuclear transformation of warfare was kept secret from the world until disclosed by the fires of Hiroshima.

Nuclear War in Embryo

All three bombs of 1945—the test bomb and the two bombs dropped on Japan—were more nearly improvised pieces of complex laboratory equipment than they were reliable weaponry. Very soon after the July test, some 60 of us flew from Los Alamos to the North Pacific to assist in the assembly of these

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complex bombs, adding our unique skills to those of scores of thousands of airmen on Tinian, where unending shiploads of gasoline and firebombs were entering the harbor.

The Hiroshima bomb, first to be readied, was first to be used, on August 6, 1945. That city was turned to rust-red ruin by the uranium bomb nicknamed Little Boy. The design had never been tested before it was dropped, as the gun design was so simple, though much costlier in nuclear fuel. Then the second version of the just tested plutonium implosion bomb Fat Man brought disaster to Nagasaki. The war soon ended.

With the sense that I was completing my long witness to the entire tragedy, I accepted the assignment to join the preliminary American party hurriedly sent from our Pacific base to enter Japan on the first day of U.S. occupation. Joined by two other young Americans in uniform, I traveled by train for a couple of weeks across Japan, the rails crowded with demobilizing troops. The Japanese were disastrously impoverished and hungry, yet still orderly. Along the tracks, we saw cities large and small, ruined by 100 wildfires set with jelly gasoline by raids of up to 1,000 B-29 bombers, devastation that was the very mark of the old war. The damage in these other cities resembled the destruction visited on Hiroshima by one single nuclear explosion and its aftermath of fire.

We had loosed our new kind of war, nuclear war in embryo, with only two bombs. A single bomber was now able to destroy a good-size city, leaving hundreds of thousands dead. Yet there on the ground, among all those who cruelly suffered and died, there was not all that much difference between old fire and new. Both ways brought unimagined inferno. True, we saw hundreds of people lying along the railway platform at Hiroshima; most of them would die from burns or from the new epidemic of radiation sickness that we had sowed. But many other cities, including firebombed Tokyo, where 100,000 or more had died in the first fire raid, also counted hosts of burned and scarred survivors. Radiation is no minor matter, but the difference between the all-out raids made on the cities of Japan and those two nuclear attacks remains less in the nature or the scale of the human tragedy than in the chilling fact that now it was much easier to destroy the populous cities of humankind. Two nuclear bombs had perhaps doubled the death count brought by air power to Japan.

Fission and then fusion offered havoc wholesale, on the cheap. It was not World War II that the atom's nucleus would most transform but the next great war. The past 50 years have been ruled by one nuclear truth. In 1945 the U.S. deployed about 1,000 long-range B-29s. By the 1960s we had about 2,000 jet bombers, and by the 1980s maybe 1,500 missiles. For more than four decades we kept a striking force comparable with the one General Curtis E. LeMay commanded in 1945, each year becoming faster, more reliable, and so on. But now every single payload was not chemical explosive but nuclear fire, bringing tens or even hundreds of times greater death and destruction. The statesmen on both sides chose to arm and even threaten war with these weapons, a war that would be orders of magnitude more violent than all before it. Yet the statesmen did not follow through on their threats; large-scale nuclear conflict is now recognized for what it is, wholly intolerable.

I returned from Japan at the end of September 1945 to learn that one young man within our small group was gone, killed in the lab by a runaway radiation burst. (He would not be the last, either.) Our temerity about the nuclear dragon had left its legacy in New Mexico as well. America was at peace but clamorous, the new atomic bomb, in all its terror, the center of interest. By the end of the year many scientists, including myself, made clear, concerted, even dramatic public statements about the future of nuclear war. What we said then was this: Secrecy will not defend us, for atoms and skills are everywhere. No defenses are likely to make up for the enormous energy release; it will never be practical to intercept every bomb, and even a few can bring grave disaster. Passive shelter is little use, for the deeper the costly shelter, the bigger the inexpensive bomb. No likely working margin of technical superiority will defend us either, for even a smaller nuclear force can wreak its intolerable damage.

Legacy of the Bomb

I think these views are as right today as they were in 1945. Only one way remains: comprehensive international agreement for putting an end to nuclear war, worked out in rich detail. It is striking that the laboratory leaders of the Manhattan Project said much the same thing as early as August 17, 1945, three days after the peace was made with Japan. But they wrote in secret to the U.S. secretary of war, and their first views remained hidden for many years.

The 1990s have given us an unexpected historical opportunity, as unexpected as was fission itself. The U.S. and the former Soviet Union are right now dismantling some eight or 10 nuclear warheads every day, yet both have a long way to go. We have never had so promising and so concrete an omen of peace, but it is still mainly promise. We need resolute and widespread action. The task is not simple, but was any international goal more important than securing the future against nuclear war? How could we ever have planned war with tens of thousands of nuclear warheads? Did we not know that America would lie in ruin as well? With nuclear weapons, war achieves a final, futile symmetry of mutual destruction.

In 1963 Oppenheimer recalled that when Bohr first came to Los Alamos during the war, the visitor asked his friend and host very seriously: "Is it big enough?" Oppenheimer knew just what Bohr meant: Was this new scale of warfare big enough to challenge the institution of war itself? "I don't know if it was then," Oppenheimer wrote, "but finally it did become big enough." Then it became frighteningly too big, and it is still far too big, but at least no longer is it luxuriantly growing. We can, if we persist, end its unparalleled threat.

The Author

PHILIP MORRISON was born in Somerville, N.J., in 1915 and spent the years from late 1942 until mid-1946 working on the Manhattan Project. He taught physics at Cornell University from 1946 to 1965. He then moved to the Massachusetts Institute of Technology, where he is now professor emeritus. Since 1945 Morrison has talked and written, at last rather hopefully, about avoiding a second nuclear war. He has enjoyed reviewing books for this magazine in nearly every issue of the past 350 months.

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What Did Heisenberg Tell Bohr about the Bomb?

In 1941 Werner Heisenberg and Niels Bohr met privately in Copenhagen. Almost two years later at Los Alamos, Bohr showed a sketch of what he believed was Heisenberg's design for a nuclear weapon

by Jeremy Bernstein

In September 1943 Niels Bohr learned that the gestapo in Copenhagen intended to arrest him. A few weeks later, on the 29th, he, his wife and several others hoping to escape from Denmark crawled in complete darkness to a beach outside Carlsberg. There they boarded a boat and crossed the Øresund in secret to Sweden. On October 6 the British flew Bohr alone from Sweden to Scotland. Later that same day he traveled to London and in the evening met with Sir John Anderson, the physical chemist in charge of the nascent British atomic bomb project. Anderson gave the Danish physicist a briefing on the Anglo-American program. According to Bohr's son Aage, who followed his father to England a week later and was his assistant throughout the war, Bohr was deeply surprised—shocked may be a better description—by how far the Anglo-American program had already progressed.

Bohr's alarm very likely had two sources. First, during the 1930s, when nuclear physics was developing, Bohr had said on several occasions that he thought any practical use of nuclear energy was all but impossible. That view was reinforced in the spring of 1939, when he realized an important detail concerning the fission of uranium. In December 1938 the German physical chemists Otto Hahn and Fritz Strassmann had discovered that uranium could be fissioned if it was bombarded with neutrons. (Hahn's former assistant Lise Meitner and her nephew Otto Frisch conjectured that the uranium nucleus had actually been split in the experiments and so coined the name "fission" for the process.) The experiments used natural uranium, 99 percent of which is in the isotope uranium 238. About seven tenths of a percent is in the isotope uranium 235, whose nucleus contains three fewer neutrons.

Chemically, the isotopes are indistinguishable. What Bohr realized was that because of their structural differences, only the very rare isotope uranium 235 had fissioned in the Hahn-Strassmann experiments. He concluded, then, that making a nuclear weapon would be almost impossible because it would require separating these isotopes—a daunting task. In December 1939 he said in a lecture, "With present technical means it is, however, impossible to purify the rare uranium isotope in su - cient quantity to realize the chain reaction." One can therefore well understand why Bohr was shocked to learn four years later that that was just what the Allies intended to do.

The second reason for Bohr's alarm can be traced back to a meeting he had had with the German physicist Werner Heisenberg in mid-September 1941, almost two years before his escape to Britain. By 1941 the Germans had occupied Denmark for more than a year. During that period, they established a so-called German Cultural Institute in Copenhagen to generate German cultural propaganda. Among its activities, the institute organized scientific meetings. Heisenberg was one of several German scientists who came under its auspices to Copenhagen, in this case to a meeting of astronomers. He had known Bohr since 1922 and had spent a good deal of time at Bohr's institute in Copenhagen, where Bohr had acted as a kind of muse for the creation of quantum theory. Now Heisenberg had returned as a representative of a despised occupying power, touting the certainty of its victory, according to some accounts.

Heisenberg's Visit

Heisenberg spent a week in Copenhagen and visited Bohr's institute on several occasions. During one of these visits, he and Bohr talked privately. Neither man seems to have made any notes, so one cannot be entirely sure what was said. Also, Bohr was a poor listener, so the two may well have talked past each other. Nevertheless, Bohr came away from the discussion with the distinct impression that Heisenberg was working on nuclear weapons. As Aage Bohr later recalled, "Heisenberg brought up the question of the military applications of atomic energy. My father was very reticent and expressed his skepticism because of the great technical di culties that had to be overcome, but he had the impression that Heisenberg thought that the new possibilities could decide the outcome of the war if the war dragged on." Now, two years later, Bohr was learning for the first time of the Allied nuclear weapons program. What had the Germans done during those two years? No wonder Bohr was alarmed.

It would be fascinating to know in detail what was meant by "new possibilities," but one can make an educated guess. By the mid-1940s physicists on both sides of the conflict realized that aside from fissioning uranium, there

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was an entirely separate route to making a nuclear weapon—the use of what later came to be known as plutonium. That element is somewhat heavier than uranium and has a di›erent chemistry, but given its nuclear structure, it is at least as fissionable. Unlike uranium, though, plutonium does not exist naturally and must be manufactured in a nuclear reactor by bombarding the reactor's uranium fuel rods with neutrons. Once made, the plutonium can be separated from its uranium matrix by chemical means.

From the moment this process was understood, any reactor became, in a certain sense, a component of a nuclear weapon. There is no doubt whatsoever that Heisenberg knew this fact well when he visited Bohr. He even gave lectures, whose texts have been preserved, describing such a possibility to highly placed German officials. Is this what he was trying to tell Bohr and, if so, why? There was such a lack of agreement between the two men as to what exactly was said that we will probably never know for sure.

As a corollary to this larger puzzle there is a smaller one. There is evidence that during the course of the Copenhagen meeting, Heisenberg gave Bohr a drawing. It is not clear whether Heisenberg made the drawing at the meeting or beforehand. Being familiar with how theoretical physicists communicate, I would imagine he drew the sketch on the spot to help convey an idea. In any case, under circumstances I will shortly describe, this drawing, or a replica, found its way to Los Alamos Laboratory in December 1943, where it created a considerable stir: it appeared to contain direct information about how the Germans were planning to make nuclear weapons. Before I describe how the drawing got to Los Alamos, let me tell how I learned of its existence. There is a relation.

The Mysterious Sketch

Beginning in November 1977, I conducted a series of interviews with the physicist Hans Bethe. Those sessions lasted on and off for two years and resulted in a three-part profile for the New Yorker magazine and a subsequent book. The interviews, which I taped, followed the chronology of Bethe's life. Bethe, who was born in Strasbourg in 1906, emigrated to the U.S. in 1935 and has been at Cornell University ever since. He became an American citizen in 1941, by which time, as he recalled, he was "desperate to do something—to make some contribution to the war effort." He, like Bohr, was at first certain that nuclear weapons were entirely impractical and went to work on the development of radar at the Massachusetts Institute of Technology.

In the summer of 1942 J. Robert Oppenheimer convened a study group at the University of California at Berkeley to investigate nuclear weapons. By this time Bethe was acknowledged as one of the leading nuclear theorists in the world, so Oppenheimer naturally asked him to participate. On the way to California by train, Bethe stopped in Chicago to pick up Edward Teller. There Bethe got the chance to see Enrico Fermi's developing nuclear reactor and, in his words, "became convinced that the atomic bomb project was real, and that it would probably work." He spent that summer working on the theory of nuclear weapons and in April 1943 went to Los Alamos, which had just opened as a laboratory. Eventually he became head of its theory division.

Now to the drawing. On November 29, 1943, Bohr and his son Aage sailed from Glasgow on the Aquitania for New York City. They arrived on December 6. Bohr was assigned the code name of Nicholas Baker, and Aage became James Baker; they were also given bodyguards. On December 28, after having had high-level meetings in Washington, D.C., with many offcials—including Major General Leslie R. Groves, the commanding offcer in charge of the Manhattan Project—Bohr departed for Los Alamos. On the 31st, presumably just after arriving at the laboratory, he met with a select group of physicists. The principal purpose of this meeting was for Bohr to tell the attending physicists what he knew about the German e›ort to make a nuclear weapon—in particular what he had learned from Heisenberg.

During one of my interviews with Bethe, he described this meeting, though not in any detail, and told me about the drawing. This is what he said to me (I have it on my tapes): "Heisenberg gave Bohr a drawing. This drawing was transmitted by Bohr later on to us at Los Alamos. This drawing was clearly the drawing of a reactor. But our conclusion was, when seeing it, these Germans are totally crazy. Do they want to throw a reactor down on London?" Only after the war did the Los Alamos scientists learn that the Germans knew perfectly well, at least in principle, what to do with a reactor—use it to make plutonium. But Bohr was concerned that one could actually use this reactor as some sort of weapon.

As far as I know, until I described this matter in the New Yorker, no one had ever mentioned such a drawing in print. In fact, my article on Bethe was frequently cited as the source for this odd sidelight on the Bohr-Heisenberg relationship. Hence, I found myself as a kind of a footnote to a footnote to history. My authority was shaken, though, at the start of 1994, during one of my periodic visits to the Rockefeller University in New York City, where I am an adjunct professor. Abraham Pais, a biographer of both Einstein and Bohr and a professor of physics emeritus at the university, called me into his offce. I have known Pais for 40 years but had not seen him in a while. This visit, then, was his first opportunity to tell me about a call he had received several months earlier.

It was from Thomas Powers, who at that time was writing Heisenberg's War. Powers had learned about the drawing from my book on Bethe. He was struck by the fact that at first glance it seemed as if Heisenberg had given to Bohr, in the middle of a war, a drawing of a highly classified German military project. That was such an extraordinary thing for Heisenberg to have done, if he did do it, that Powers wanted to check the matter out. He therefore got in touch with Aage Bohr in Copenhagen (his father had died in 1962). In a letter dated November 16, 1989, Aage Bohr wrote, "Heisenberg certainly drew no sketch of a reactor during his visit in 1941. The operation of a reactor was not discussed at all."

Stunned, Powers next contacted Bethe, who repeated to him exactly what he had told me 10 years earlier. In a quandary, Powers had called Pais, and now Pais was asking me. But Pais had done his own investigation. He had spoken with Aage Bohr, who once again insisted that there had never been any such drawing. Pais had also checked the archives in Copenhagen where all Bohr's private papers and journals are stored. Nowhere, he told me, did he find any mention of this drawing. Now it was my turn to be stunned. It is one thing to be a footnote to a footnote to history, but it is quite another to be a footnote to a footnote to incorrect history.

I promised Pais I would look into the matter myself, although, in truth, when I left his offce I did not have the foggiest idea of how I would go about it. Obviously, contacting Bethe again would not get me much further. Nothing could be more direct than what he had told me and repeated to Powers. I would need witnesses independent of Bethe and Aage Bohr. That much was clear. But who? Oppenheimer was dead. Niels

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Bohr was dead. Groves was dead. Who else could have seen that drawing?

The Investigation

I began, in fact, with less information than I have so far given the reader. All Bethe had told me was that Bohr had "transmitted" a drawing to Los Alamos. He had not related any specific details about the December 31 meeting, so initially I had no idea who might have been there. Indeed, I did not even have the specific date. All that I learned subsequently. But I did know physicists who were at Los Alamos at the time and who might have seen or heard about the drawing. Two came to mind. One was Victor Weisskopf, an old friend, who had been close to Oppenheimer.

The other was Rudolf Peierls. Peierls and Otto Frisch had in March 1940 made the first correct calculation—in principle—to determine the amount of uranium 235, or the critical mass, needed to make a bomb. (The fact that this mass turned out to be pounds rather than tons is what really prompted the Allied e›ort.) Peierls, along with Frisch, was at Los Alamos as of early 1944. I have also known Peierls for many years and have frequently discussed with him the history of nuclear weapons. So it was quite natural for me to write him as well. This I did in early February, and soon after, both men answered.

Peierls replied that he had never seen the "famous sketch" yet did not think that either Bethe or Aage Bohr had deliberately lied. He proposed that perhaps Niels Bohr had kept knowledge of the sensitive document from his family or that perhaps Heisenberg had only shown the sketch to Bohr, who might then have redrawn it. He suggested I contact Bethe about this possibility. Weisskopf also wrote proposing I contact Bethe once more, because he, too, had never seen or heard about the drawing.

Neither of these letters was what I had hoped to receive. Clearly, I had to write Bethe to tell him what I had learned and to see if he could shed any further light on the situation. But then I had an inspiration. I would call Robert Serber. Serber, a professor of physics emeritus at Columbia University who lives in New York City, is also an old friend. After receiving his Ph.D. in 1934 from the University of Wisconsin, he had won one of five National Research Council Fellowships in physics and chose to go work with Oppenheimer at Berkeley. During the next few years, he had become very close to Oppenheimer.

After a brief interlude at the University of Illinois from 1938 until 1942, Serber returned to Berkeley to work on the bomb with Oppenheimer. He was there in the summer of 1942 when Bethe and Teller arrived. By March 1943 he had moved, with the first batch of scientists, to Los Alamos. One of his early tasks was to give a series of introductory lectures on bomb physics to the arriving scientists. These lessons were collected into what came to be called The Los Alamos Primer, declassified in 1965 and first published in its entirety in 1992. If anyone knew about the drawing, it would be Serber because he was in constant contact with Oppenheimer throughout this period.

I called Serber, and immediately I knew I had struck a gold mine. Not only did he remember the drawing vividly, but he also remembered the precise circumstances under which he had seen it. He had been summoned to Oppenheimer's offce on December 31, where a meeting was already in progress. Oppenheimer showed him a drawing with no explanation and asked him to identify it. This was the kind of intellectual game Oppenheimer liked to play. Serber looked at it and said it was clearly the drawing of a reactor. Oppenheimer replied that in fact it was a drawing of Heisenberg's reactor and had been given to the assembled group by Bohr. Bohr, who was, as Serber recalled, standing next to Oppenheimer, did not disagree.

That is what Serber told me. But he also said he had some written material related to this meeting. A few days later copies of two documents arrived: a letter from Oppenheimer to General Groves sent the day after the meeting and a two-page memorandum written by Bethe and Teller on the explosive possibilities of the reactor. Unfortunately, although these documents were very suggestive, they did not, at least when I first read them, settle the issue completely. The Bethe-Teller memorandum did hold significant clues, but I will return to them later. Oppenheimer's letter made no mention of the drawing or of Heisenberg or of the Germans. But the last sentence clearly implied that Bohr had spoken to Groves in Washington about these matters. Perhaps something in Groves's own archives might prove enlightening.

Meanwhile I had at last written to Bethe, and on March 2, I received his answer. It begins, "I am quite positive there was a drawing. Niels Bohr presented it to us, and both Teller and I immediately said, 'This is a drawing of a reactor, not of a bomb.'… Whether the drawing was actually due Heisenberg, or was made by Bohr from memory, I cannot tell. But the meeting on 31 December 1943 was especially called to show us what Niels Bohr knew about the Germans' idea of a bomb."

Bethe o›ered a theory to explain the mystery: "Heisenberg thought that the main step to a bomb was to get a reactor and to make plutonium. A reactor, however, could also be used as a power source. Niels Bohr was very ignorant about the whole subject. Heisenberg probably wanted to show Bohr that the Germans were not making a bomb but merely a reactor. Bohr misunderstood completely, and only on 31 December 1943 was it finally explained to him that this was not a bomb. That drawing made a great impression on me. Again, I am surprised that Viki [Weisskopf] and Aage have forgotten about it. What does Serber say?"

I was able to write Bethe and tell him what Serber had said. I also wrote Teller to ask for his recollections of the meeting. I was not sure I would get an answer and never have. But I had also written again to Weisskopf, sending him copies of the memorandums from Serber. On February 23, I received a typically gracious Weisskopf letter, acknowledging that he had indeed seen the sketch but later forgotten about it.

I now had, I thought, enough material to return to Pais. I played for him my Bethe tape and gave him copies of all the documents. He was about to return to Copenhagen, where he spends about half the year with his Danish wife. He promised me that he would speak to Aage Bohr at an opportune moment. That happened late in June. By the 30th Pais had written to tell me what had happened. He and Aage Bohr had met, discussed the letters and reviewed the tapes. Still, Aage Bohr felt certain that Heisenberg never gave any such drawing to his father. So I wrote to Aage Bohr directly. In February of this year his assistant, Finn Aaserud, wrote, "Aage Bohr maintains that it is entirely impossible that Bohr brought with him to the U.S. a drawing from the 1941 meeting with Heisenberg and indeed that the discussion at Los Alamos you refer to had anything to do with the 1941 encounter at all."

Where does this leave us? I have asked myself this question many times since receiving Pais's letter last June. I was at a loss until recently, when I took another look at the memorandum that Bethe and Teller prepared for Oppenheimer and Bohr and eventually for Groves. It suddenly struck me that in the first sentence of the second paragraph of this report Heisenberg's imprint stands out like a sore thumb. It reads, "The proposed pile [reactor] con-

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sists of uranium sheets immersed into heavy water." In other words, Bethe and Teller were not considering any old reactor design but rather a very particular one that Bohr had described to them. This design is actually the faulty reactor Heisenberg invented in late 1939 and early 1940, which he clung to until nearly the end of the war!

It is almost unthinkable that in the few short weeks from when Bohr learned about the Allied project to when he arrived at Los Alamos he would have produced his own design possessing the same flaws as did Heisenberg's. He must have gotten this idea from Heisenberg, either verbally or in the form of a drawing. Where else could it have come from?

The Evidence

Let me explain. Any reactor requires fuel elements, the uranium, and what is known as a moderator, a device that slows the speed of neutrons hitting the fuel. Neutrons traveling near the speed of sound are vastly more e›ective in causing fission than are the rapidly moving neutrons produced by the fissioning itself. So the fuel elements in a reactor are embedded in the moderator. But a designer must carefully choose from which material the moderator should be made and also how the fuel elements should be placed in it. The latter involves both art and science.

The trick is that the uranium itself can absorb neutrons without producing fission. This absorption becomes stronger as the neutrons are slowing down. If the geometry of the fuel elements is not well thought out, the uranium will absorb so many neutrons that a self-sustaining chain reaction will never take place. In fact, the most effcient design involves separated lumps of uranium embedded in a lattice within the moderator. How big these lumps should be, and how they should be arranged, involves art. But the worst possible solution is placing the uranium in sheets, or layers.

To return to the matter at hand, note that Bethe and Teller wrote, "The proposed pile consists of uranium sheets." Heisenberg chose just such a design because it involved easier calculations than did other schemes. Then there is the question of the moderator. Bethe and Teller stated that the sheets were to be "immersed into heavy water." This specification, once explained, also has Heisenberg written all over it. The role of the moderator is, as I have mentioned, to slow down the fissioned neutrons. The best materials for this purpose are the lightest because a collision between a neutron and an object having a similar mass results in the greatest energy loss. If the neutron collides with a heavier object it will bounce o› and change its direction but not its speed.

If mass were the only consideration, the ideal moderator would be hydrogen, whose nucleus is a single proton having a mass sensibly the same as the neutron's. But, in reality, ordinary hydrogen fails as a moderator because it absorbs neutrons. In contrast, "heavy hydrogen," which has an extra neutron in its nucleus, does not absorb neutrons. Heavy hydrogen is found in "heavy water." But in seawater, say, this heavy water is only about one part in 5,000. So to use it as a moderator, it must be separated from ordinary water—an expensive and diffcult process.

Carbon, on the other hand, is abundant and cheap, although somewhat less e›ective as a moderator. By late 1940 Heisenberg had concluded that only carbon and heavy hydrogen should be used as moderators. But in January 1941, Walther Bothe, who was the leading experimental nuclear physicist left in Germany, began working with graphite, the form of carbon commonly used in pencils. His experiments seemed to show that graphite absorbed neutrons too strongly to serve as an e›ective moderator. What Bothe did not realize was that unless the graphite is purified far beyond any ordinary industrial requirement, it will contain boron impurities. Boron soaks up neutrons like a sponge. One part boron in 500,000 of graphite can ruin that graphite as a moderator. All the same, because of Bothe's experiment, Heisenberg and other German physicists decided that heavy water was the only practical choice.

Needless to say, physicists who were responsible for the successful reactor program here made the same kinds of calculations. Like Heisenberg, they decided that a carbon reactor would need more natural uranium than a heavy-water reactor. Fermi and his colleague Leo Szilard had also done experiments on neutron absorption by carbon. But Szilard was a fanatic about the purity of the graphite, and so their graphite, unlike Bothe's, worked well as a moderator. Because carbon was so cheap compared with heavy water, they decided that it was the best moderator. Fermi's reactor, which first operated on December 2, 1942, had a lattice of uranium lumps embedded in carbon. All the German experimental reactors—none of which ever operated—used heavy-water moderators. Look again at the sentence in the Bethe-Teller memorandum: "The proposed pile consists of uranium sheets immersed into heavy water." It is as if someone had written "Made in Germany" on this design.

Putting everything together, there seems to be little doubt that Heisenberg attempted to describe a nuclear device to Bohr. It seems that this device was his version of a reactor. He may, or may not, have given Bohr a drawing, but Bohr clearly retained a visual memory of the design. Bohr, however, did not understand the difference between a reactor and a bomb at the time and assumed that Heisenberg was describing a bomb.

So Aage Bohr may be quite right when he says, as far as his father was concerned, there was no discussion of a reactor. He may also be right that Heisenberg never gave Bohr a drawing. None of the individuals I have contacted are sure that the drawing they saw was in Heisenberg's hand—only that it was a drawing of Heisenberg's reactor. This I think solves the puzzle, but it does not solve the mystery. What was the purpose of Heisenberg's visit in the first place? Those who admire Heisenberg have argued that it was to show Bohr that the Germans were working only on a "peaceful" reactor.

It also must be noted that when Heisenberg visited Bohr, he clearly knew that reactors could be used to manufacture plutonium and that plutonium could fuel a nuclear weapon. Why, then, did he visit Bohr? What message was he trying to convey? What was he trying to persuade Bohr to do, or not to do? What was he trying to learn? That is the real mystery, one we may never solve.

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Lise Meitner and the Discovery of Nuclear Fission

by Ruth Lewin Sime

One of the discoverers of fission in 1938, Meitner was at the time overlooked by the Nobel judges. Racial persecution, fear and opportunism combined to obscure her contributions

When scientists first recognized, in late 1938, that a neutron could split an atom's core, the discovery came as a complete surprise. Indeed, no physical theory had predicted nuclear fission, and its discoverers had not the slightest foreknowledge of its eventual use in atomic bombs and power plants. That much of the story is undisputed.

The question of who deserved credit for the breakthrough, however, has long been debated. Physicist Lise Meitner and two chemists, Otto Hahn and Fritz Strassmann, conducted a four-year-long investigation that resulted in the discovery of fission in their laboratory in Berlin. Meitner fled Nazi Germany in 1938 to escape the persecution of Jews, and soon after, Hahn and Strassmann reported the discovery. Meitner and her nephew, Otto R. Frisch, published the correct theoretical interpretation of fission a few weeks later. But the 1944 Nobel Prize in Chemistry was awarded to Hahn alone.

That Strassmann did not get the Nobel with Hahn is probably because he was the junior investigator on the team, and Nobel committees tend to favor senior scientists. But Meitner and Hahn held equal professional standing. Why was she excluded? Hahn offered what became the standard account, which was uncritically accepted for many years. According to him, the discovery had relied solely on chemical experiments that were done after Meitner left Berlin. She and physics, he maintained, had nothing to do with his success, except perhaps to delay it.

Strassmann, who was very much in Hahn's shadow, disagreed. He insisted that Meitner had been their intellectual leader and that she remained one of them through her correspondence with Hahn, even after she left Berlin. The available documents support Strassmann's view. Scientific publications show that the investigation that led to the discovery of fission was intensely interdisciplinary. Questions from nuclear physics initiated the work. Data and assumptions from both chemistry and physics guided and misguided their progress. And private letters reveal that Meitner made essential contributions until the very end.

By any normal standards of scientific attribution, the Nobel committees should have recognized her influence. But in Germany the conditions were anything but normal. The country's anti-Jewish policies forced Meitner to emigrate, separated her from her laboratory and prohibited her from being a co-author with Hahn and Strassmann in reporting the fission result. Because of political oppression and fear, Hahn distanced himself and fission from Meitner and physics soon after the discovery took place. In time, the Nobel awards sealed these injustices into scientific history. Recently released documents show that the Nobel committees did not grasp the extent to which the result relied on both physics and chemistry, and they did not recognize that Hahn had distanced himself from Meitner not on scientific grounds but because of political oppression, fear and opportunism.

Other factors also served to marginalize Meitner, including her outsider status as a refugee in Sweden, a postwar unwillingness in Germany to confront Nazi crimes, and a general perception—held much more strongly then than it is now—that women scientists were unimportant, subordinate or wrong. Publicly, Meitner said little at the time. Privately, she described Hahn's behavior as "simply suppressing the past," a past in which they had been the closest of colleagues and friends. She must have believed that history would be on her side. Fifty years later, it is.

Born and educated in Vienna, Lise Meitner moved to Berlin in 1907 at the age of 28. There she teamed up with Otto Hahn, a chemist just her age, to study radioactivity, the process by which one nucleus is transformed into another by the emission of alpha or beta particles. Their collaboration was capped by their discovery in 1918 of protactinium, a particularly heavy radioactive element. As their careers progressed, they remained equals scientifically and professionally: both were professors at the Kaiser Wilhelm Institute for Chemistry,

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and each maintained an independent section in the institute—his for radiochemistry, hers for physics. During the 1920s, Hahn continued developing radiochemical techniques, whereas Meitner entered the new field of nuclear physics. Hahn later described this period as a time when her work, more than his, brought international recognition to the institute. Her prominence, and her Austrian citizenship, shielded Meitner when Hitler came to power in 1933; unlike most others of Jewish origin, she was not dismissed from her position. And although many of her students and assistants were Nazi enthusiasts, Meitner found the physics too exciting to leave. She was particularly intrigued by the experiments of Enrico Fermi and his co-workers in Rome, who began using neutrons to bombard elements throughout the periodic table.

Fermi observed that when a neutron reaction occurred, the targeted nucleus did not change dramatically: the incoming neutron would most often cause the target nucleus to emit a proton or an alpha particle, nothing more. Heavy elements, he found, favored neutron capture. That is, a heavy nucleus would gain an extra neutron; if radioactive, the heavier nucleus would invariably decay by emitting beta rays, which transformed it into the next higher element. When Fermi irradiated the heaviest known element, uranium, with neutrons, he observed several new beta emitters, none with the chemical properties of uranium or the elements near it. Thus, he cautiously suggested that he had synthesized new elements beyond uranium. All over the world, scientists were fascinated.

Meitner had been verifying Fermi's results up to this point. The work perfectly suited her interests and expertise, and she was then in her prime: one of the first women to enter the upper ranks of German science, she was a leading nuclear physicist of her day. To study these new "transuranics" in detail, however, Meitner needed an outstanding radiochemist. Hahn, though reluctant at first, agreed to help, and Fritz Strassmann, an analytical chemist from the institute, also joined the collaboration. The three were politically compatible: Meitner was "non-Aryan," Hahn was anti-Nazi, and Strassmann had refused to join the National Socialist–associated German Chemical Society, making him unemployable outside the institute. By the end of 1934, the team reported that the beta emitters Fermi observed could not be attributed to any other known element and that they behaved in a manner expected for transuranics: they could be separated out of the reaction mixture along with transition metals, such as platinum and rhenium sulfides. Thus, like Fermi, the Berlin scientists tentatively suggested that these activities were new elements beyond uranium. As it turned out, the interpretation was incorrect: it rested on two assumptions—one from physics and one from chemistry—that would prove false only several years later.

From physics, it had until then been observed that only small changes could take place during nuclear reactions, leaving an event such as fission unimaginable. And from chemistry it appeared that transuranic elements would be transition elements. It was a simple mistake: the chemistry of thorium and uranium is quite similar to that of transition elements, so chemists in the 1930s also expected that the elements beyond uranium would be transitionlike, resembling rhenium, osmium, iridium and platinum.

Untangling Decay Chains

The two false assumptions reinforced each other, misleading the investigation for several years. Later Hahn blamed physicists and their mistaken faith in small nuclear changes for obstructing the discovery. If anything, however, the scientific publications indicate that the chemists were complacent and the physicists were more skeptical. Physics did not predict fission, to be sure, but it detected discrepancies that chemistry could not.

The Berlin scientists had tried to separate the presumed transuranics, which had extremely weak activities, from uranium and its decay products, which had much stronger, natural radioactivity. After irradiating a uranium sample with neutrons, they would dissolve the sample and then separate from the solution just those activities with the chemistry of transition metals, generally by using transition-metal compounds as carriers. The precipitate itself was a mixture of several beta emitters, which the Berlin team painstakingly began to disentangle.

Over two years, they identified two parallel beta-decay chains, which they referred to as processes one and two [*see box at right*]. The sequence of these decays corresponded to the properties expected for the elements following uranium: they resembled the transition elements rhenium, osmium and so on. The fit between the sequences and the predicted chemistry seemed too good not to be true. Publishing in *Chemische Berichte* in 1936 and 1937, with Hahn as the senior author, the elated group repeatedly referred to these transuranics as "unquestionable," there being "no doubt" about their existence and "no need for further discussion."

All the while, the data were stretching physical theories thin. Meitner struggled to integrate the results from chemistry, radiochemistry and her own physical measurements into a cogent model of the nuclear processes involved. She established that thermal—exceedingly slow—neutrons enhanced the yield of processes one and two, evidence that these events involved neutron capture. But fast neutrons generated the same results. Thus, she concluded that both processes originated with the most abundant uranium isotope, uranium 238. She also identified a third process—involving the capture of moderately slow neutrons—for which there was no long beta chain.

Meitner regarded it as odd that three different neutron-capture processes all originated from the same uranium 238 isotope. She suspected that something was very wrong with processes one and two. From theoretical considerations, she could not understand how the capture of a single neutron could produce such great instability that it would take four or five beta emissions to relieve it. And it was even harder to understand that the two long beta-decay chains paralleled each other for several steps. Theory offered no explanation. In a 1937 report to *Zeitschrift für Physik,* Meitner concluded that the results were "difficult to reconcile with current concepts of nuclear structure."

Once fission was recognized, researchers understood that processes one and two were fission processes: the uranium splits into fragments that are highly radioactive and form a long sequence of beta decays. (There can be many such decay chains because uranium can split in many ways.) Meitner regarded process three as the most normal, and later this was shown to be correct: the uranium 239 isotope formed in this neutron-capture reaction decays by beta emission to element 93. In 1940 it was identified by Edwin McMillan and Philip Abelson and later named neptunium. Had the Berlin scientists been

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able to detect neptunium, they would have found that it is a rare-earth element, and they would have realized that the activities in processes one and two are not transuranics. But they did not detect it; their neutron sources were too weak.The most serious error the Berlin team made was that the investigators separated out and studied only those activities with transition-metal chemistry, ignoring all others. In 1938 in Paris, Irène Curie and Pavel Savitch used a different technique to examine the entire mixture of uranium products and found a new, strong activity whose chemistry they could not ascertain. Like the presumed transuranics, its yield was enhanced by thermal neutrons. By the time the Berlin team looked into it in October 1938, however, Meitner had been forced to flee Germany for Stockholm. Hahn and Strassmann analyzed the Curie activity alone and, finding that it followed a barium carrier, identified it as an isotope of radium.

Identifying Barium

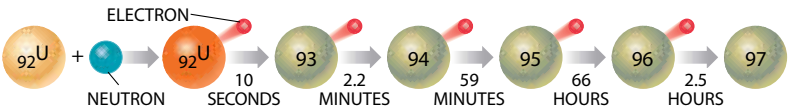
Meitner and Hahn corresponded constantly, and mail between Stockholm and Berlin was delivered overnight. She could scarcely believe the radium result. To form radium, the uranium nucleus would have to emit two alpha particles. Meitner was convinced that it was energetically impossible for a thermal neutron to knock out even one alpha particle—and certainly not two. In November 1938 Meitner visited Niels Bohr's Institute for Theoretical Physics in Copenhagen, and Hahn met her there on November 13. Outside the city their meeting was kept secret to avoid political difficulties for Hahn, and he never mentioned it later in his memoirs. But we know from Hahn's own pocket diary that they met, and we know that Meitner objected strenuously to the radium result. That was the message Hahn brought back to Strassmann in Berlin.

According to Strassmann, Hahn told him that Meitner "urgently pleaded" that they verify the radium one more time. "Fortunately, her opinion and judgment carried so much weight with us that we immediately began the necessary control experiments," Strassmann remembered. With these experiments, they intended to verify the presence of radium by partially separating it from its barium carrier. But no separation occurred, and they were forced to con-

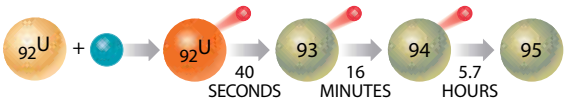
Discovering Fission

The Berlin group found that a large number of beta emitters (radioactive nuclei that emit electrons) were formed when neutrons hit uranium nuclei. The researchers proposed two chains, which they believed consisted of elements beyond uranium, each with its own rate of beta decay:

Process 1

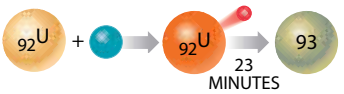


Process 2

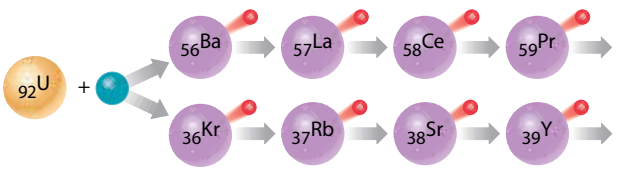


In addition, they identified a simpler reaction:

Process 3



Meitner regarded process three as the most understandable, and later it was shown to be correct. But she was puzzled by processes one and two because the decay chains were so long and paralleled each other. Ultimately, when Hahn and Strassmann identified one of the reaction products as barium, Meitner and Frisch realized that the uranium nucleus had split into nuclei of barium and krypton, which began a series of beta emissions:



These nuclei and other fission fragments account for the decay chains of processes one and two. Meitner and Frisch proposed the name "nuclear fission," published the first theoretical explanation of the process and predicted the enormous energy released. —R.L.S.

JARED SCHNEIDMAN DESIGN

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clude that their "radium" was in fact an isotope of barium, an element much lighter than uranium.

In December 1938, just before Christmas, Hahn told Meitner about the barium. It was a "frightful result," he wrote. "We know uranium cannot really break up into barium!" He hoped she could propose "some fantastic explanation." Meitner answered by return mail. Although she found it difficult to think of a "thorough-going breakup," she assured him that "one cannot unconditionally say: it is impossible." Her letter must have been the best Christmas present he ever received. She had vehemently objected to the radium result, but she was ready to consider the barium result as expanding, rather than contradicting, existing theory.

Later, Hahn was known to say that if Meitner had still been in Berlin, she might have talked him out of the barium result and might have "forbidden" him from making the discovery. But Meitner's letter, which Hahn always had in his possession, demonstrates that the opposite is true. And at the time, Hahn clearly found her letter reassuring, because only after he received it did he add a paragraph to the galley proofs of his barium publication, suggesting that the uranium nucleus had split in two. Meitner was bitterly disappointed that she could not share in this "beautiful discovery," as she called it, but they all knew that it was impossible to include a "non-Aryan" in the publication.

Revising Nuclear Theory

For Christmas, Meitner visited a friend in western Sweden, and her nephew, Otto Frisch, a physicist at Bohr's institute, joined her. When Meitner and Frisch came together, so, too, did the various strands of nuclear theory. Both were accustomed to thinking of the nucleus as a liquid drop, but now they visualized it as a wobbly, oscillating drop that was ready to split in two. Frisch realized that the surface tension of a nucleus as large as uranium might be vanishingly small. Meitner did the mass defect calculation in her head and estimated the lost mass that was converted to enormous energy when the nucleus split. Everything fell into place: the theoretical interpretation itself was a beautiful discovery—and it was recognized as such. The physics community immediately adopted the term "fission" that Meitner and Frisch proposed, and Bohr used their work as a starting point for a more extensive theory.

Hahn and Strassmann's barium finding appeared in *Naturwissenschaften* in January 1939; Meitner and Frisch published their interpretation in *Nature* a few weeks later. On the surface, the discovery of fission was now completely divided—chemistry from physics, experiment from theory, Germans from refugees. To those who did not understand the science or who did not care to understand the politics, it might appear that chemists had discovered fission, whereas physicists had only interpreted it.

In the weeks following the discovery, Hahn exploited that artificial division. He knew Meitner's forced emigration was unjust. He knew she had fully participated in the discovery. But he could not say so. He was afraid for himself and for his position and terribly afraid that others would find out that he and Strassmann had continued to collaborate with Meitner after she left Berlin.

He decided that the discovery of fission consisted of just those chemical experiments that he and Strassmann had done in December. In February 1939 he wrote to Meitner, "We absolutely never touched on physics, but instead we did chemical separations over and over again." He described fission as a "gift from heaven," a miracle that would protect him and his institute. As it turned out, it may not have been necessary for Hahn to divorce himself from Meitner and physics to make the "miracle" come true. That spring the German military took an active interest in the potential uses of the new discovery, and by the summer of 1939 Hahn and his institute were secure. Later he recalled that "fission saved that whole situation."

After the atomic bomb, fission was more sensational than ever, and Hahn was a very famous man. In postwar Germany, he was a major public figure for a generation, lionized as a Nobel laureate and a decent German who never gave in to the Nazis, a scientist who did not build a bomb. His treatment of Meitner, however, was anything but decent. Not once in his numerous articles, interviews, memoirs or autobiographies did he mention her initiative for the uranium project, her leadership of their team in Berlin or their collaboration after she left. He died in Göttingen in 1968 at the age of 89.

In Sweden during the war, Meitner's professional status was poor. Her friends believed that she almost surely would have been awarded a Nobel Prize had she emigrated anywhere else. In 1943 she was invited to Los Alamos to work on the atomic bomb, but she refused. For a brief period after the war ended, she was a celebrity in the U.S. and Britain, miscast as the Jewish refugee who escaped the Nazis with the secret of the bomb. But Meitner was a private person who detested publicity. She never wrote an autobiography or authorized a biography. She left Stockholm for Cambridge, England, in 1960 and died there in 1968, a few days before her 90th birthday. Sadly, she died some 30 years before she received proper recognition for her work.

The Author

RUTH LEWIN SIME was born in New York City in 1939. She received a bachelor's degree in mathematics from Barnard College in 1960 and obtained a doctorate in chemistry from Harvard University in 1964. Since 1968, she has taught chemistry at Sacramento City College. Her interest in Lise Meitner began some 25 years ago, when she taught a class on women in science and discovered that little scholarly attention had been paid to Meitner's life and work. Her biography *Lise Meitner: A Life in Physics* was published in 1996 by the University of California Press.

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The Odd Couple and the Bomb

Like a story by Victor Hugo as told to Neil Simon, the events leading up to the first controlled nuclear chain reaction involved accidental encounters among larger-than-life figures, especially two who did not exactly get along—but had to

by William Lanouette

On the eve of World War II, European physicists Enrico Fermi and Leo Szilard both moved into the King's Crown Hotel, near Columbia University in New York City. Although they had previously exchanged letters, they met by chance at the hotel in January 1939. The encounter led to one of the more colorful—and contentious—partnerships in the history of science.

Each man was a refugee from European fascism, and each possessed essential pieces to the puzzle that would ultimately release the energy of the atom. They quickly realized, however, that a joint effort would require them to overcome deep differences in their worldviews, work styles and basic personalities. Had Fermi and Szilard failed to persevere in their often uncomfortable collaboration, the world's first controlled nuclear chain reaction would not have been developed by 1942, and the Manhattan Project would not have built the first atomic bombs by 1945. As Szilard later reflected, "If the nation owes us gratitude—and it may not—it does so for having stuck it out together as long as it was necessary."

Crossed Paths

The 38-year-old Enrico Fermi had just arrived in New York from Rome. The trip included a stop in Stockholm to receive the 1938 Nobel Prize in Physics, for work in which he had bombarded the element uranium with neutrons, which created new transuranic (heavier-than-uranium) elements. Fearing new racial laws in fascist Italy, Fermi and his Jewish wife decided against returning home. Instead he accepted one of four American offers and took a job at Columbia.

Leo Szilard, a 40-year-old Hungarian Jew, came to New York by a more circuitous route. He left his native Budapest in 1919 for Berlin, where he studied and worked with Albert Einstein. Initially, the two shared some ideas and several patents for an electromagnetic refrigerator pump [see "The Einstein-Szilard Refrigerators," by Gene Dannen; Scientific American, January 1997]; two decades later their relationship would take on vast historical significance.

When Adolf Hitler took power in 1933, the wary Szilard fled to London. That same year, he conceived the idea for a nuclear "chain reaction" that, according to his 1934 patent application, might produce "electrical energy" and possibly "an explosion." Such chain reactions would eventually take place in nuclear power plants and in nuclear weapons. First, however, an element that could foster a chain reaction would have to be discovered. After four years of failed experiments at the University of Oxford and then at the universities of Rochester and Illinois in the U.S., Szilard, too, came to Columbia.

Fermi was a rigorous academic whose life centered on a brilliant physics career; he had little interest in politics. A homebody, he soon moved his family from the King's Crown to a house in suburban New Jersey. He awoke at 5:30 each morning and spent the two hours before breakfast polishing his theories and planning the day's experiments. Rare among 20th-century scientists, Fermi was a gifted theoretical physicist who also enjoyed working with his hands. When not lecturing, he toiled in the laboratory with his dedicated assistants, making and manipulating equipment.

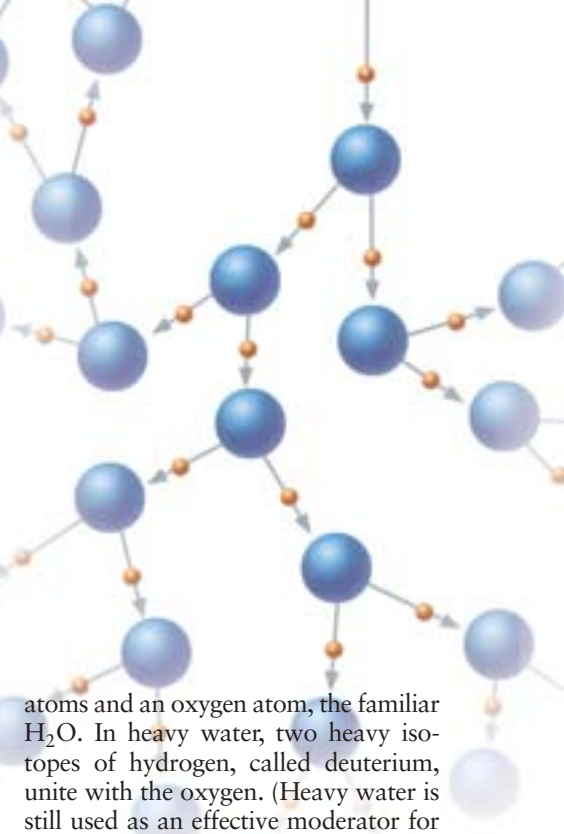
An unemployed "guest scholar" with no classes or lab of his own, the bachelor Szilard rarely taught, published infrequently and dabbled in economics and biology. He lived in hotels and faculty clubs and enjoyed soaking for hours in the bathtub to dream up fresh ideas. (One later inspiration was that the National Science Foundation should pay second-rate scientists *not* to conduct research.) Szilard read newspapers avidly, speculated constantly about financial, political and military affairs, and always kept two bags packed for hasty escapes from any new eruptions of fascism.

A late sleeper, he often appeared at Columbia only in time for lunch, after which he would drop in on colleagues, posing insightful questions and suggesting experiments *they* should try. "You have too many ideas," future physics Nobel laureate Isidor Isaac Rabi finally said to him. "Please go away."

The late Massachusetts Institute of Technology physicist Bernard Feld worked with Fermi and Szilard as the latter's research assistant at Columbia. He summed up the two men: "Fermi would not go from point A to point B until he knew all that he could about A and had reasonable assurances about B. Szilard would jump from point A to point D, then wonder why you were wasting your time with B and C."

Within days of the chance meeting between Fermi and Szilard at the King's Crown Hotel, Danish physicist Niels Bohr landed in New York with important word from Europe: physicist Lise Meitner, a Jew who had fled from Germany to Stockholm, had determined that Berlin chemists Otto Hahn and Fritz Strassmann had caused uranium to undergo "fis-

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sion" via neutron bombardment. They had split the atom. (In 1966 the three would win the Enrico Fermi Award for this work.) Bohr's report helped Fermi come to a more complete understanding of his own 1934 uranium experiments; in addition to creating transuranic elements, he had unknowingly split atoms.

To Szilard, the news was more ominous. He realized that uranium was the element that could fuel the chain reaction described in his 1934 patent application. Betting on his political insight, he had assigned that patent to the British Admiralty in secret, lest he alert German scientists to the possibility of atomic explosives. The discovery of fission confirmed Szilard's fears that an atom bomb could soon be a decisive reality.

The notion of the nuclear chain reaction had first come to Szilard while he was standing on a London street corner in 1933. The neutron had been discovered only the previous year, and physicists now thought of the atom as resembling a solar system, with negatively charged electrons orbiting a nucleus of positively charged protons and neutral neutrons. Having no charge, a neutron hurled at an atom might stealthily penetrate the nucleus without being repelled. Szilard imagined that if a neutron hit a nucleus and split the atom, the breakup might release the binding energy that holds the atom together. Some of that atom's neutrons might in turn be released, which could hit and split other atoms. If more than one neutron was released from each split atom, the process could exponentially expand, with millions of atoms splitting in a fraction of a second and freeing vast amounts of energy. (Szilard would later learn that Bohr's news enabled Fermi likewise to envision a chain reaction, although he considered one extremely unlikely.)

While Szilard was filing his patent in 1934, Fermi was in Rome, becoming the world's expert on neutron bombardment of atoms. He found that by passing the neutrons through paraffin wax he could slow them down, increasing the chance that they would be absorbed by the target nucleus. His work with uranium was puzzling. Sometimes the nucleus absorbed neutrons. (Because atomic identity is governed by the number of protons, the neutron absorption produced only heavier variants, or isotopes, of uranium.) But sometimes neutron bombardment created entirely new elements. German chemist Ida Noddack, following Fermi's experiments in journal reports, suggested a chemical analysis of the new species to see if they were the fragments of split atoms. But Fermi, concentrating on the physics of bombardment and absorption, did not pursue the implications of those new elements. Had he done so, he might have recognized nuclear fission years before Meitner.

At Columbia in the spring of 1939, Fermi and Szilard each tried experiments aimed at a better understanding of fission. Szilard offered Canadian physicist Walter Zinn a radium-beryllium neutron source he had just ordered from England. With it, Zinn and Szilard showed that more than two neutrons escaped during fission. Fermi and his assistant Herbert Anderson tried a similar experiment using a more powerful radon-beryllium source, with inconclusive results. Szilard guessed that the source was too strong, enabling some neutrons to pass right through the nucleus and making it hard to know if they were counting neutrons from fission events or merely the original neutrons. Szilard loaned Fermi his English neutron source, which gave much clearer results.

The two men then attempted to work together—with a resounding clash of individual styles. Szilard shunned manual labor in favor of brainstorming, but Fermi expected all his team members to participate in hands-on experiments. Although the men respected the other's abilities, they bristled in the other's company. Recognizing their mutual need, however, they reached out to Columbia's physics department chairman, George Pegram, who agreed to coordinate their separate work. Pegram's shuttle diplomacy harnessed Fermi's precision and Szilard's prescience. With Anderson, the combative colleagues succeeded in determining that by using slow neutrons "a nuclear chain reaction could be maintained."

Building the Chain

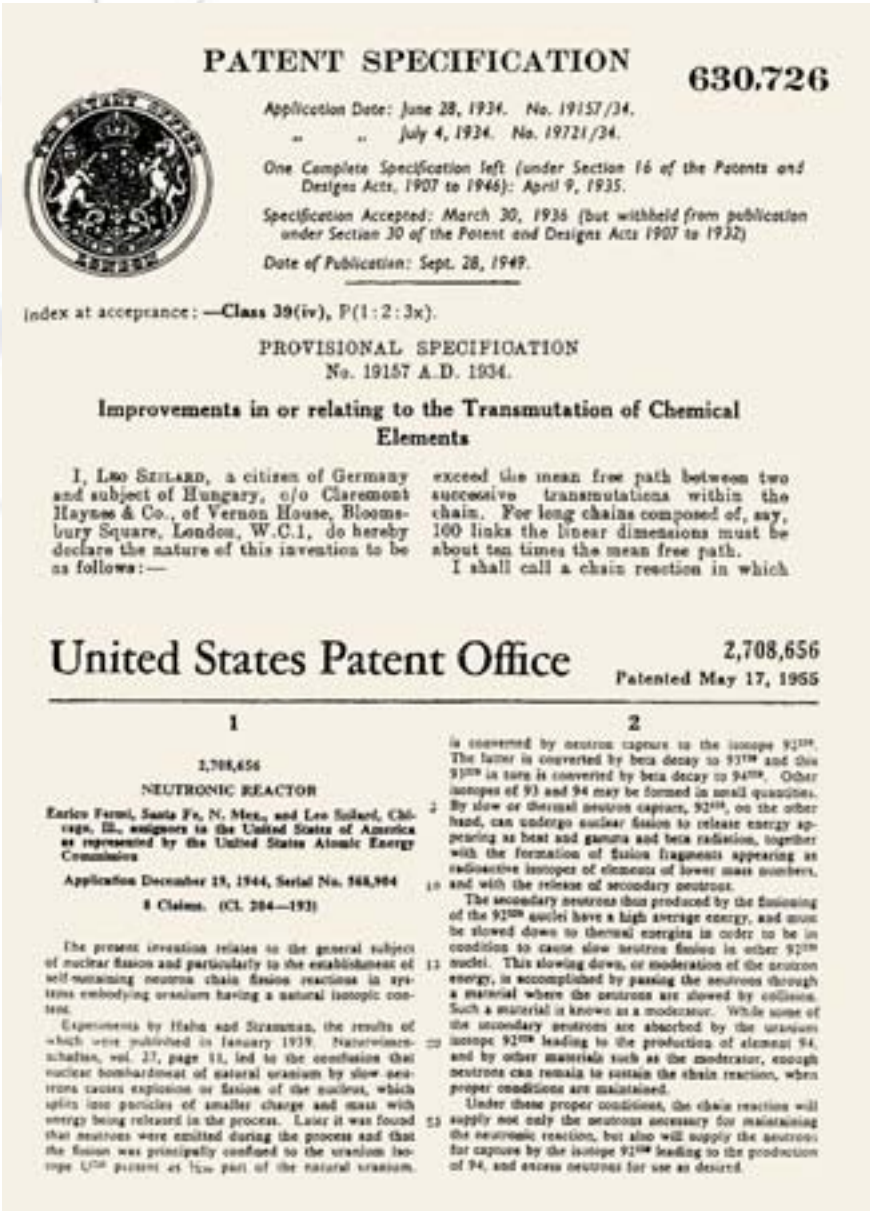
Although collisions between Fermi and Szilard were all too common, collisions between neutrons and nuclei were at first too rare. Passing the neutrons through so-called moderators, such as Fermi's paraffin, helped to slow them, making their collision with an atom's nucleus more likely. By 1939 physicists also knew that "heavy water" was an efficient moderator. Ordinary, or "light water," consists of two hydrogen atoms and an oxygen atom, the familiar H_2O. In heavy water, two heavy isotopes of hydrogen, called deuterium, unite with the oxygen. (Heavy water is still used as an effective moderator for natural uranium fuel in today's nuclear reactors, whereas light water is used for enriched uranium fuel.) But heavy water was expensive and scarce. The large-scale experiments that Szilard had in mind would require a more common and affordable moderator. He would discover one that his German counterparts had overlooked.

As Szilard had feared, German atom-bomb research was well under way by the spring of 1939. Both German and American physicists also recognized that graphite—the soft form of carbon that is used as pencil lead—could be a moderator. But German scientists gave up on it because it absorbed too many neutrons; they instead concentrated on heavy water, always in short supply. Szilard, who often personally took trains to Boston or Buffalo to procure raw materials for Fermi's experiments, realized that commercial graphite also contained small amounts of boron—a voracious absorber of neutrons. He ordered custom-made, boron-free graphite, which eventually led to one of the most caustic Fermi/Szilard confrontations.

Anderson measured neutron absorption in the pure graphite and found that it would indeed make a good moderator. Szilard recommended that the test results remain secret. Fermi, ever the professional scientist, objected to the breach of the long-standing academic tradition of peer-reviewed journal publication. "Fermi really lost his temper," Szilard would later recall. "He really

JENNIFER JOHANSEN

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PATENT SPECIFICATION

630,726

Application Date: June 28, 1934. No. 19157/34.
" " July 4, 1934. No. 19721/34.

One Complete Specification left (under Section 16 of the Patents and Designs Acts, 1907 to 1946): April 9, 1935.

Specification Accepted: March 30, 1936 (but withheld from publication under Section 30 of the Patent and Designs Acts 1907 to 1932)

Date of Publication: Sept. 28, 1949.

Index at acceptance:—Class 39(iv), P(1:2:3x).

PROVISIONAL SPECIFICATION
No. 19157 A.D. 1934.

Improvements in or relating to the Transmutation of Chemical Elements

I, LEO SZILARD, a citizen of Germany and subject of Hungary, c/o Claremont Haynes & Co., of Vernon House, Bloomsbury Square, London, W.C.1, do hereby declare the nature of this invention to be as follows:—

exceed the mean free path between two successive transmutations within the chain. For long chains composed of, say, 100 links the linear dimensions must be about ten times the mean free path.

I shall call a chain reaction in which

United States Patent Office

2,708,656

Patented May 17, 1955

1

2,708,656

NEUTRONIC REACTOR

Enrico Fermi, Santa Fe, N. Mex., and Leo Szilard, Chicago, Ill., assignors to the United States of America as represented by the United States Atomic Energy Commission

Application December 19, 1944, Serial No. 568,904

8 Claims. (Cl. 204—193)

The present invention relates to the general subject of nuclear fission and particularly to the establishment of self-sustaining neutronic chain fission reactions in systems embodying uranium having a natural isotopic content.

Experiments by Hahn and Strassman, the results of which were published in January 1939. Naturwissenschaften, vol. 27, page 11, led to the conclusion that nuclear bombardment of natural uranium by slow neutrons causes explosion or fission of the nucleus, which splits into particles of smaller charge and mass with energy being released in the process. Later it was found that neutrons were emitted during the process and that the fission was principally confined to the uranium isotope U^{235} present as low part of the natural uranium.

2

is converted by neutron capture to the isotope 93^{239}. The latter is converted by beta decay to 93^{239} and this 93^{239} in turn is converted by beta decay to 94^{239}. Other isotopes of 93 and 94 may be formed in small quantities. By slow or thermal neutron capture, 92^{235}, on the other hand, can undergo nuclear fission to release energy appearing as heat and gamma and beta radiation, together with the formation of fission fragments appearing as radioactive isotopes of elements of lower mass numbers, and with the release of secondary neutrons.

The secondary neutrons thus produced by the fissioning of the 92^{235} nuclei have a high average energy, and must be slowed down to thermal energies in order to be in condition to cause slow neutron fission in other 92^{235} nuclei. This slowing down, or moderation of the neutron energy, is accomplished by passing the neutrons through a material where the neutrons are slowed by collision. Such a material is known as a moderator. While some of the secondary neutrons are absorbed by the uranium isotope 92^{238} leading to the production of element 94, and by other materials such as the moderator, enough neutrons can remain to sustain the chain reaction, when proper conditions are maintained.

Under these proper conditions, the chain reaction will supply not only the neutrons necessary for maintaining the neutronic reaction, but also will supply the neutrons for capture by the isotope 92^{238} leading to the production of 94, and excess neutrons for use as desired.

PATENT awarded to Szilard in England for the chain reaction idea was assigned to the British Admiralty and remained secret until after the war. A U.S. patent for the actual reactor was awarded jointly to Fermi and Szilard.

thought this was absurd." Pegram once again interceded, however, and Fermi reluctantly agreed to self-censorship under these special circumstances.

With the graphite moderator, Fermi thought there might now be at least a ray of hope for a self-sustaining chain reaction. On the question of how realistic that hope was, Fermi and Szilard had also shown distinctly different modes of thinking. Szilard fretted that the Germans were ahead in a nuclear arms race; in the American vernacular that Fermi enjoyed trying out, he reacted to Szilard's speculation with "Nuts!" Fermi thought that any atom bombs were perhaps 25 to 50 years away and told colleagues that actually creating the self-sustaining chain reaction was "a remote possibility" with perhaps a 10 percent chance.

"Ten percent is *not* a remote possibility if it means that we may die of it," Isidor Rabi replied. Szilard noted how differently he and Fermi interpreted the same information. "We both wanted to be conservative," Szilard later recalled, "but Fermi thought that the conservative thing was to play down the possibility that this may happen, and I thought the conservative thing was to assume that it would happen and take the necessary precautions."

These precautions included Szilard borrowing $2,000 to support Fermi's research. Nevertheless, in the summer of 1939 Fermi showed his relative lack of concern over the implications of nuclear research by leaving for the University of Michigan to study cosmic rays. The world's first successful design for a nuclear reactor was thus created neither in a lab nor a library but in letters.

Szilard, typically, urged starting "large scale" experiments "right away." Fermi, typically, remained skeptical. Szilard proposed stacking alternating layers of graphite and uranium in a lattice, the geometry of which would define neutron scattering and subsequent fission events. Fermi countered with a homogeneous design in which the uranium and graphite would be mixed like gravel. The suggestion angered Szilard, who concluded that Fermi preferred it only because it was an easier configuration about which to make calculations. Fermi responded that further reflection had convinced him of Szilard's lattice idea. Once sold, Fermi applied his substantial ingenuity to determining the lattice's physical properties and coordinating the personnel necessary to make a reactor.

Friends in High Places

Szilard recognized that despite his and Fermi's brainpower, they would still need help from important allies for their collaboration to succeed. They would get it from an unlikely trio: Franklin D. Roosevelt, J. Edgar Hoover and Albert Einstein.

During the summer, Szilard learned that Germany was restricting uranium supplies. He assumed that this indicated fission research and wanted to alert the federal government. With the instincts of a public relations expert, he turned to his mentor and friend Einstein, who was living at a summer cottage on Long Island, about 70 miles east of New York City. Szilard told the renowned physicist about the chain reaction. "I haven't thought of that at all," Einstein replied, seeing at last a mechanism that might make real the mass-energy conversion of his famous equation.

Szilard made two visits to Einstein, the second to discuss a letter for him to sign. "Szilard could do anything, except he could not drive a car," recalls his second-trip chauffeur, a fellow Hungarian refugee scientist. "And I could drive a car. And, therefore, I drove Szilard to the summer place.... Einstein was a democrat in that he invited not only Szi-

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lard for a cup of coffee but also his driver." Edward Teller was thus present when Einstein, wearing an old robe and slippers, read and agreed to sign the now well known letter to President Roosevelt. The letter, dated August 2, 1939, began, "Some recent work by E. Fermi and L. Szilard. . . ." It proceeded to warn of German atomic weapons research and urged the U.S. to do its own.

Szilard passed the letter to investment banker Alexander Sachs, who was a New Deal adviser and had access to the president. World War II began on September 1, and in October, when Roosevelt finally received the letter, he agreed that some action was needed "to see that the Nazis don't blow us up." To that end, he created a federal Uranium Committee, with Szilard and other émigré scientists as members. Within weeks they had gained a commitment of $6,000 for research at Columbia.

After the war, Einstein said he had "really only acted as a mailbox" for Szilard. In 1940, however, Einstein was once again forced to play a decisive role when the U.S. Army almost denied Fermi and Szilard security clearance. Investigators, basing their conclusions on information from "highly reliable sources," came to the paradoxical conclusions that Fermi, a refugee from fascism, was "undoubtedly a Fascist" and that Szilard, in terror of the Nazis, was "very pro-German." Perhaps Szilard's cries that Germany could win the war accounted for the latter misinterpretation. (The report also spelled Szilard's name in two different ways, both of which were wrong.) The army decided of each man that "employment of this person on secret work is not recommended," despite the fact that the only secret work in question in the U.S. at the time was taking place in the minds of Fermi and Szilard.

Had the army been heeded, of course, funds would have run out, and all the embryonic federal atomic research by Fermi and Szilard would have ceased. This mistake was averted when the Federal Bureau of Investigation, under pressure from the White House, was ordered to "verify their loyalty to the United States." FBI director J. Edgar Hoover sent agents to interview Einstein (whose pacifist views would later cause his own loyalty to be questioned). With Einstein's good word, federal money flowed in to Columbia in November 1940, although suspicions of Fermi and Szilard would abate only years after they became U.S. citizens.

Funding in place, Fermi's team now worked systematically to construct "piles" (Szilard's lattice) of uranium and graphite, to test for the ratio and geometry that would optimize a chain reaction. The day before the Japanese attack on Pearl Harbor, President Roosevelt approved an all-out federal commitment to research the A-bomb. In the spring of 1942 Fermi, Szilard and the rest of the Columbia team moved to the University of Chicago, where they established a top-secret "metallurgical laboratory" for chain-reaction research. The army's Manhattan Project took over control of the effort in June. Ironically, at this same moment in history, Germany scaled down its own A-bomb work, convinced that the undertaking was impractical for the current war.

In the fall, a pile was constructed, with uranium spheres embedded in graphite blocks. On December 2, 1942, in a squash court under Stagg Field, the university's football stadium, Fermi directed the experiment that initiated the world's first controlled, self-sustaining nuclear chain reaction. After the historic experiment, Fermi and Szilard found themselves alone with their reactor. They shook hands, Szilard remembered, "and I said I thought this day would go down as a black day in the history of mankind."

Later Conflicts and Harmony

Near the war's end in 1945, Fermi and Szilard differed once again. Szilard had hastened the A-bomb's development as a weapon of defense against Germany. With Hitler's defeat, Szilard argued that the bomb should not be used offensively against Japan but instead be demonstrated to encourage surrender. Fermi, as scientific adviser to the administration's high-level committee on options for bomb use, argued that a demonstration would be impractical. The administration agreed, with the subsequent August devastation of the cities of Hiroshima and Nagasaki.

After the war, Fermi favored continuing army control of atomic research, while Szilard successfully lobbied Congress for a new, civilian Atomic Energy Commission. The two men found common ground in opposition to Szilard's old friend Teller in 1950, when both objected to U.S. development of the hydrogen bomb. Fermi called the H-bomb "a weapon which in practical effect is almost one of genocide."

A joint patent for the Fermi-Szilard "neutronic reactor" was first published in 1955, a year after Fermi's death. Szilard pursued molecular biology and nuclear arms control until his death in 1964. Fermi summed up Szilard by calling him "extremely brilliant" but someone who "seems to enjoy startling people." Szilard reflected on Fermi by writing, "I liked him best on the rare occasions when he got mad (except of course when he got mad at me)."

The Author

WILLIAM LANOUETTE received a doctorate in politics from the London School of Economics in 1973. His thesis, comparing the use and abuse of scientific information by U.S. and U.K. legislators and government officials, prepared him well for his current work as an energy/science policy analyst at the U.S. General Accounting Office. He has written about atomic energy and science policy for more than 30 years, in such publications as the *Atlantic Monthly,* the *Bulletin of the Atomic Scientists* and the *Economist.* The author of a biography of Leo Szilard, Lanouette has lectured widely about the politics and personalities of the Manhattan Project. He is an avid oarsman, and his next book will be about the lucrative rise and scandalous end of professional rowing in 19th-century America. Lanouette thanks Nina Byers, professor of physics at the University of California, Los Angeles, and independent scholar Gene Dannen for helpful additions to this article.

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J. Robert Oppenheimer: Before the War

Although Oppenheimer is now best remembered for his influence during World War II, he made many important contributions to theoretical physics in the 1930s

by John S. Rigden

Fifty years ago this month, on July 16, 1945, an unearthly blast of light seared the predawn sky over the desert in New Mexico. The witnesses of this event included many of this century's most distinguished physicists. As they watched the boiling glare through their welding goggles, a sober reality bore into them: the nuclear age had begun. The chief witness—the person who had directed the atomic bomb project from its inception—was J. Robert Oppenheimer.

Oppenheimer was a rare individual. His intellectual acuity, diverse interests, frail physique and ethereal personality made him a man of legendary proportions. After World War II Oppenheimer became a public figure, known for leading the physicists who built the atomic bomb at Los Alamos Laboratory. His success as the director of the Manhattan Project provided him with a base of influence, and, for a time, he enjoyed the authority and power that were his.

Then, in June 1954, amid the anticommunism paranoia of McCarthyism, the U.S. Atomic Energy Commission (AEC) concluded that Oppenheimer had defects in his character and deemed him a national security risk. Albert Einstein and others at the Institute for Advanced Study in Princeton, N.J., where Oppenheimer was then director, declared their support for him. In October the trustees of the institute reelected him to another term as director, a position he then held until a year before his death in February 1967. Still, after the AEC's actions, Oppenheimer's slight frame became the depiction of a broken man.

Few historians have written about the Oppenheimer who invigorated American theoretical physics a decade before the war, which is unfortunate for two reasons. First, Oppenheimer became a physicist at the rarest of times, when the theories of quantum mechanics and nuclear physics were being formed, revising a great deal of traditional thought in the field. Second, although he is sometimes characterized as an underachiever, Oppenheimer had in fact made many significant contributions to several major areas of physical research before taking his post at Los Alamos.

Oppenheimer built the foundation for contemporary studies of molecular physics. He was the first to recognize quantum-mechanical tunneling, which is the basis of the scanning tunneling microscope, used to reveal the structure of surfaces atom by atom. He fell just short of predicting the existence of the positron, the electron's antiparticle. He raised several crucial difficulties in the theory of quantum electrodynamics. He developed the theory of cosmic-ray showers. And long before neutron stars and black holes were part of our celestial landscape, Oppenheimer showed that massive stars can collapse under the influence of gravitational forces.

To Physics from Chemistry

Like many physicists of his era, Oppenheimer studied chemistry first. "Compared to physics," he said, "[chemistry] starts right in the heart of things." As a freshman at Harvard University he realized that "what I liked in chemistry was very close to physics." So that spring, he submitted a reading list to the physics department and was granted graduate standing. He enrolled in many physics classes, but because his interests and coursework were very diverse, he claimed later to have received only "a very quick, superficial, eager familiarization with some parts of physics." He wrote: "Although I liked to work, I spread myself very thin and got by with murder; I got A's in all these courses which I don't think I should have." Whether that was true or not, Oppenheimer did gain valuable experience working in Percy W. Bridgman's laboratory—a privilege granted to him by virtue of his advanced standing. In the 1920s American physics was dominated by experimentalists such as Bridgman, who was among the first to investigate the properties of matter under high pressure and built much of the apparatus needed to do so. Thus, from his student experiences, Oppenheimer did not distinguish between experimental and theoretical physics, the latter being largely a European activity. "I didn't know you could earn your living that way [as a theoretical physicist]," he once said, looking back on his undergraduate days.

For this reason, as his graduation in 1925 grew near, he aspired to work under Ernest Rutherford, one of the greatest experimentalists of the century, at the Cavendish Laboratory in Cambridge, England. Rutherford had conducted the first trials to reveal that atoms contained extremely small, heavy cores, or nuclei. He was, however, unimpressed with Oppenheimer's credentials and rejected his application. Oppenheimer next wrote to Joseph John Thomson, another renowned experimentalist at the Cavendish. Thomson accepted Oppenheimer as a research student and put him to work in a corner of the laboratory, depositing thin films on a base of

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collodion. "I am having a pretty bad time," he wrote to a high school friend on November 1, 1925. "The lab work is a terrible bore, and I am so bad at it that it is impossible to feel that I am learning anything."

The ensuing winter was a dark time for Oppenheimer, but with the coming of spring, new possibilities became apparent. Rutherford, who took to Oppenheimer in person, introduced him to Niels Bohr when Bohr visited the Cavendish; through Patrick M. S. Blackett, a physicist at the Cavendish, he met Paul Ehrenfest of the University of Leiden. He also became friends with the influential Cambridge physicists Paul A. M. Dirac and Ralph H. Fowler. All these men were theoreticians and helped to broaden Oppenheimer's view of the field. Fowler was particularly perceptive. He advised Oppenheimer to learn Dirac's new quantum-mechanical formalism and apply it to band spectra, a melding of old and new knowledge as yet untackled.

Oppenheimer became absorbed in the problem and over the next few years developed the modern theory of continuous spectra. This work not only led to his first paper, it also marked the beginning of his career as a theoretical physicist. When Max Born visited the Cavendish in the summer of 1926 and suggested that Oppenheimer pursue graduate studies at the University of Göttingen, a center for theoretical physics, Oppenheimer readily accepted the plan. "I felt completely relieved of the responsibility to go back into the laboratory," he said to the philosopher Thomas S. Kuhn in a 1963 interview.

It was at Göttingen that Oppenheimer first became aware of the problems perplexing European physicists. "The science is much better [here]," he wrote to his friend Francis Furgusson in November 1926. At that time, Born, Werner Heisenberg and Pascual Jordan were all in Göttingen, formulating the theory of quantum mechanics. Born, a distinguished teacher, made Göttingen as good a place as any to learn the intricacies of the new theory. Oppenheimer learned fast. In December 1926, only four short months after he had applied to Göttingen, he sent an article, "On the Quantum Theory of Continuous Spectra," to the leading German physics journal Zeitschrift für Physik. This paper was in fact an abridged version of what would be his dissertation. After receiving his doctorate from Göttingen in March 1927, he spent the next two years, one in the U.S. and one in Europe, as a National Research Council Fellow.

During this period, Oppenheimer profited a great deal from his association with prominent European physicists of the day. "They gave me some sense and…some taste in physics," he told Kuhn. Still, the theoretical problems he investigated were primarily of his own choosing. Later, in the 1930s, perhaps because of his own laboratory experience, Oppenheimer worked closely with experimentalists, many of whom acknowledged that he understood their data better than they did.

Atoms and Molecules

The atom, once found to emit discrete spectra during transitions between energy states, gave the first indication that the physics of preceding centuries was inadequate. Thus, atoms and molecules provided a natural testing ground for the new theory of quantum mechanics and for Oppenheimer in 1927. His first major contribution was finding a way to simplify the analysis of molecular spectra. By interpreting spectra, physicists determine the structure and properties of molecules. But an exact quantum-mechanical description of even a simple molecule is complicated by the fact that the electrons and nuclei of the atoms making up that molecule all interact with one another.

Oppenheimer recognized that because of the great disparity between the nuclear and electronic masses, these interactions could be largely ignored. The massive nuclei respond so slowly to mutual interactions that the electrons complete several cycles of their motion as the nuclei complete a small fraction of their own. While on a vacation, Oppenheimer wrote up a short paper on the topic and sent it to Born. Born was aghast at the brevity of Oppenheimer's draft and churned out a 30-page paper, showing in detail that the vibration and rotation of the nuclei could be treated separately from the motion of the electrons. Today the Born-Oppenheimer approximation is the starting point for physicists and chemists engaged in molecular analysis. Later on, Oppenheimer determined the probability that one atom captures the electron of another atom. In keeping with the Born-Oppenheimer approximation, he showed that the probability is independent of the internuclear potential between the two atoms.

Oppenheimer in fact discovered another quantum-mechanical behavior, called tunneling, in 1928. Tunneling occurs under many theoretical conditions. An electron, for example, can escape from confines that normally sequester it if it behaves like an infinitesimal billiard ball. The time-honored example of tunneling is that which takes place when a nucleus expels an alpha particle during radioactive decay. Inside a uranium nucleus, both nuclear and electrostatic forces will restrict the motion of an alpha particle. Classically, it has no way to leave the nucleus. Quantum-mechanically, though, the alpha particle can tunnel through the surrounding barrier and slip away.

During the summer of 1928 physicists George Gamow and, independently, Edward U. Condon and Ronald W. Gurney first explained radioactive disintegration by means of tunneling. Textbook writers of today acknowledge this fact, but they also imply that these scientists actually discovered the phenomenon, which is not true. Several months earlier, in March, Oppenheimer had submitted a paper to the Proceedings of the National Academy of Sciences that considered the e›ect an electric field has on an atom. Classically, an atom can be dissociated only by an intense electric field. In the quantum view, however, a weak field can separate an electron from its parent atom because the electron can tunnel through the barrier that binds it. Oppenheimer showed that a weak electric field could dislodge electrons from the surface of a metal. Gerd Binnig and Heinrich Rohrer of the IBM Zurich Research Laboratory developed the scanning tunneling microscope based on this principle in 1982, 54 years after Oppenheimer had discovered it [see "The Scanning Tunneling Microscope," by Gerd Binnig and Heinrich Rohrer; SCIENTIFIC AMERICAN, August 1985].

Particles and Fields

Oppenheimer spent his final months in Europe, from January to June 1929, with Wolfgang Pauli at the Swiss Federal Institute of Technology in Zurich. After this apprenticeship, Oppenheimer's interests turned away from applications of quantum mechanics to more basic questions of physics. The timing for such a shift was perfect. That spring he received o›ers from the California Institute of Technology and the University of California at Berkeley; in both places, physical research was aimed at the forefront of basic questions. Robert A. Millikan, who coined the term "cosmic rays" in 1925, was at Caltech, and Ernest O. Lawrence, who invented the cyclotron in 1930, was investigating nuclear physics at Berkeley.

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Oppenheimer became a physicist at the rarest of times, when the theories of quantum mechanics and nuclear physics were being formed, revising a great deal of traditional thought in the field.

Oppenheimer accepted both positions, typically spending the fall term at Berkeley and the spring semester at Caltech. At both schools he attracted outstanding students who helped to bring American physics into the ranks of the world's best.

One of the most heated controversies of the early 1930s was over a theory proposed by Dirac. On January 2, 1928, the editor of the Proceedings of the Royal Society received a manuscript from Dirac entitled "The Quantum Theory of the Electron." This paper, along with a second part published a month later, was probably Dirac's most significant accomplishment. The relativistic wave equation he devised to describe the electron thrilled physicists in that it yielded the particle's spin and correct magnetic moment. Yet this paper also raised vexing issues. Heisenberg wrote to Pauli in July 1928 that the "saddest chapter of modern physics is and remains the Dirac theory." The principal problem with Dirac's wave equation was that it gave solutions corresponding both to positive energy states and to an infinite number of negative energy states. In such a situation, quantum mechanics predicts that electrons can jump into these negative energy states, and so all electrons could end up there. Accordingly, ordinary electrons should not exist.

To avoid this difficulty, Dirac imagined that these negative energy states were occupied by an infinite number of electrons. If a few of these states were unoccupied, however, they would appear as positive holes in the negative sea of charge. In March 1930 Dirac published a paper asserting that these positive holes were protons. But Oppenheimer, who read Dirac's paper before publication, argued in a letter to Physical Review, printed the same month, that they were not. He pointed out that if the positive holes in Dirac's theory were protons, then electrons and protons would annihilate one another, meaning that ordinary matter would have a lifetime of approximately 10^{-10} second. He further made note that the positive particles posited by Dirac's theory needed to have the same mass as an electron. In fact, these positive holes were positrons, the electron's antiparticle, but in 1930 this particle was unknown and unanticipated. In contesting Dirac, though, Oppenheimer fell just short of predicting its existence.

Even after the Caltech physicist Carl Anderson's discovery of the positron in 1932, positron theory resulting from Dirac's work was plagued with problems. Oppenheimer and other physicists working on quantum electrodynamics (QED) had many doubts about the basic theory. In 1930, for example, Oppenheimer showed that when the QED theory published that same year by Heisenberg and Pauli was applied to the interactions between electrons, protons and an electromagnetic field, the displacement of spectral lines was infinite. Oppenheimer's skepticism about QED was kept alive throughout the 1930s by anomalies in his cosmic-ray work caused by the muon and other high-energy particles unknown at the time. Had Oppenheimer had an experimental result on the hydrogen atom obtained by his student Willis E. Lamb only after the war, it is conceivable that he would have resolved the troubling problem of infinities.

In 1931 Oppenheimer attempted to find an equation for the photon that would be an analogue to Dirac's equation for the electron. He failed in this effort but in the process demonstrated the basic difference between particles of half-integral and integral spins, which later constituted the basis for Pauli's formal proof of the connection between spin and statistics. According to quantum mechanics, both the annihilation and the creation of matter—subject to the conservation laws of energy and momentum—are possible. A gamma ray, for example, can give rise to an electron and a positron in a process called pair production. Oddly, Oppenheimer did not originate the idea of pair production, but along with his student Milton S. Plesset, he did provide the first correct description of it in 1933. Working with his postdoctoral student Wendell H. Furry a year later, Oppenheimer developed electron-positron theory essentially in its modern form. They showed that the observed charge of the electron is not the true charge and, in doing so, anticipated the phenomenon called charge renormalization, which helped to explain some of the earlier difficulties surrounding infinities in QED.

Creation and Destruction of Matter

In the 1930s most of the high-energy physics experimentation was happening in the earth's atmosphere. There energetic particles (in the billion-electron-volt range) having cosmic origins bombarded atmospheric atoms. It was during a cloud-chamber study of such cosmic radiation in 1932 that Anderson first discovered the positron. If a metal plate of, say, lead is placed in a cloud chamber, a single cosmic-ray track incident on the plate from above the surface can give rise to a number of tracks emanating from a point on the plate's lower surface. Oppenheimer and his student J. Franklin Carlson showed that these cosmic-ray "showers," commonly consisting of photons, electrons and positrons, are produced by a cascade of electron-positron pair productions. The thickness of the lead plate can, of course, be varied. If the primary cosmic ray was either a photon or an electron, Oppenheimer and Carlson noted that a lead plate 20 centimeters thick absorbed all the resulting radiation for the energy

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ranges experimentally observed.

Additional data revealed, however, that penetration exceeded depths that could be attributed to either photons or electrons. They concluded that "there is another cosmic-ray component." A few months later groups at Caltech and at Harvard simultaneously discovered a new particle. Oppenheimer and his Berkeley colleague Robert Serber immediately equated this particle with one the Japanese physicist Hideki Yukawa had predicted to explain nuclear forces. The newly discovered particle in fact turned out to be the muon. The pion—Yukawa's prediction—came later.

Away from Caltech at Berkeley, Oppenheimer's research revolved around the accelerator. When James Chadwick discovered the neutron in 1932, the proton-electron theory of the nucleus was abandoned, and the modern proton-neutron model took its place. During the spring of 1933 Lawrence first began accelerating deuterons, consisting of a single neutron and proton, and using them to bombard heavy nuclei. Deuterons, he found, disintegrated nuclei more effectively than did protons. In no time at all, Lawrence and his coworkers observed alpha particles coming out of target nuclei.

Then they came on a puzzling result: when high-energy deuterons hit any nucleus whatsoever, the target would give o› protons within a narrow energy range. In fact, deuterons contaminating Lawrence's apparatus accounted for the mystery: the protons he witnessed all resulted from deuterium fusion. But before this explanation emerged, the observation stimulated questions about deuterium-induced reactions. At Berkeley, Oppenheimer and his student Melba N. Phillips showed that when a deuteron collides with a heavy nucleus, that nucleus can capture the neutron in the deuteron, liberating the proton. The theory Oppenheimer and Phillips formulated for this reaction, now named after them, accounted exactly for Lawrence's strange results.

Now accepted as end points in stellar evolution, neutron stars and black holes were both postulated on theoretical grounds during the 1930s. Oppenheimer and two of his students, George M. Volkoff and Hartland S. Snyder, were in the vanguard of this development. Oppenheimer and Volkoff together became interested in another worker's suggestion that once a sufficiently massive star had exhausted its source of thermonuclear energy, a neutron core could be formed. To test whether this scenario was possible, Oppenheimer and Volkoff set out to establish the difference between a gravitational treatment of the process, based on Newton's theory, and one consistent with Einstein's general relativity.

Neutron Stars and Black Holes

The Oppenheimer-Volkoff equation, which gives the pressure gradient within the star, revealed that the pressure increased more rapidly moving deeper into the stellar core than would be expected from a Newton-based calculation. Thus, the Oppenheimer-Volkoff theory, based on general relativity, predicted stronger, and more accurate, gravitational forces than did Newtonian theory. Oppenheimer and Volkoff also performed the first detailed calculations establishing the structure of a neutron star, thereby laying the foundation for the general relativistic theory of stellar structure. Just before Oppenheimer and Volkoff published a paper on this work in 1939, Oppenheimer sent a letter to George E. Uhlenbeck, a theoretical physicist at the University of Michigan, who, with his colleague Samuel A. Goudsmit, discovered the electron's spin. He wrote, "We have been…working on static and nonstatic solutions for very heavy masses…old stars perhaps which collapse to neutron cores. The results have been very odd…."

The results in fact became even stranger. Later that year Oppenheimer and Snyder published a classic paper entitled "On Continued Gravitational Contraction." They noted that when a massive star has exhausted its internal source of nuclear energy, its ultimate fate is determined by how much mass it can shed, either through radiative expulsion or by rapid rotation and flying apart. After all avenues for ejecting mass have been traversed, the core that remains is bound together by the gravitational force. If there is no thermonuclear energy to act as an equilibrating counterforce, the core will continue to collapse. As this collapse takes place, the light radiating from the core becomes increasingly redshifted, meaning its wavelength lengthens; further, the path along which this light can escape into space becomes increasingly narrow until the path closes on itself, leaving behind a source of gravitational attraction shut o› from external observation. In constructing this description, Oppenheimer and Snyder provided the first calculation revealing how a black hole can form. In May 1994 compelling evidence was observed through the eye of the Hubble Space Telescope for the presence of a massive black hole in the center of the galaxy M87, the biggest and brightest in the Virgo cluster. Oppenheimer's contribution to physics throughout the century was broad, deep and lasting. The Born-Oppenheimer approximation, the penetration of electrons through potential barriers, the theory of cosmic-ray showers, neutron stars and black holes are all a vital part of contemporary physics.

Pulsars, now recognized as spinning neutron stars, were first seen in 1967, the year Oppenheimer died of cancer in Princeton. Had he lived longer, Oppenheimer might have enjoyed the recognition this discovery brought to his prewar physics, something that had been overshadowed by his wartime work and postwar fame.

The Author

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The Metamorphosis of Andrei Sakharov

The inventor of the Soviet hydrogen bomb became an advocate of peace and human rights. What led him to his fateful decision?

by Gennady Gorelik

The cloud turned gray, quickly separated from the ground and swirled upward, shimmering with gleams of orange.... The shock wave blasted my ears and struck a sharp blow to my entire body; then there was a prolonged, ominous rumble that slowly died away after thirty seconds or so .... The cloud, which now filled half the sky, turned a sinister blue-black color."

It was August 12, 1953, and Andrei Dmitrievich Sakharov had just become father of the Soviet hydrogen bomb. Along with a few officials, he donned a dustproof jumpsuit and drove into the blast range. The car stopped beside an eagle that was trying to get off the ground; its wings had been badly burned. "I have been told that thousands of birds are destroyed during every test," Sakharov was later to write in his memoirs. "They take wing at the flash, but then fall to earth, burned and blinded."

The innocent victims of nuclear testing were to become a deepening concern, and ultimately an obsession, for this extraordinary man. While he continued to design ever more efficient bombs, he also agonized over how many human lives the fallout from each blast would cost. Sakharov's many fruitless attempts to stop unnecessary tests at last led to his realizing how little control he had over the weapons he had created.

Numerous tales have been invented to account for Sakharov's transformation to an advocate for human rights. After his death in 1989, the Russian state archives released many secret documents relating to his life and work, which are now to be found in the Sakharov Archives in Moscow. These papers, as well as Sakharov's own writings, show that his metamorphosis derived directly from his involvement in the weapons project. For years, Sakharov genuinely believed that nuclear—and thermonuclear—weapons were vital to maintaining military parity with and preventing aggression by the U.S. His transformation came not from a newfound morality but from his rather old-fashioned one, coupled with his accumulating experience with weapons and in the politics of weaponry.

A Sugary Layered Roll

Sakharov was born in 1921 to a family of Moscow intelligentsia. His father was a teacher of physics and a writer of popular science books, as well as a humane and forthright man. After graduating from high school, Andrei enrolled in Moscow University in 1938. When war broke out with Germany, his weak heart prevented him from being drafted. Graduating with honors in 1942, he refused to go on to higher studies: he wanted to contribute to the war effort. Accordingly, he became an engineer in a military ammunition plant in Ulyanovsk, where he invented a magnetic device to test the cores of the bullets that were being manufactured.

At the factory he met Klavdia Vikhireva, whom he married at the age of 22. In those years he also dreamed up and solved some small problems in physics, which found their way through his father to Igor Tamm, the leading theoretical physicist at the P. N. Lebedev Physical Institute in Moscow. In early 1945 Sakharov was officially invited to Moscow to conduct graduate studies under Tamm's supervision.

One morning in August he saw in a newspaper that an atomic bomb had exploded over Hiroshima. He realized that "my fate and the fate of many others, perhaps of the entire world, had changed overnight."

Sakharov was clearly very able as a scientist and soon came up with a theory of sound propagation in a bubbly liquid, of importance in detecting submarines with sonar. He also calculated how fusion, the merging of two nuclei into one, might be catalyzed by a light, electronlike particle known as a muon. (Atoms that contain muons in place of electrons are much smaller and therefore would require less compression to be fused.)

Exhilarated by pure physics, he twice declined invitations from senior officials to join the Soviet atomic

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weapons project. An atomic bomb involves the fission of a heavy nucleus such as uranium 235 into two roughly equal parts, accompanied by the release of energy. But one day in 1948 Tamm announced that he and some selected associates, including Sakharov, had been assigned to investigate the possibility of a hydrogen bomb. This kind of bomb is based on the fusion of light nuclei, most commonly the two forms of hydrogen called deuterium and tritium, emitting greater amounts of energy than a fission bomb does.

Yakov Zel'dovich, a brilliant physicist who headed theoretical research for the nuclear weapons program, handed Tamm a tentative design for the hydrogen bomb. Fusion requires two positively charged nuclei to be brought close enough, despite their mutual repulsion, to touch; such conditions can arise only from the tremendous energy generated by a preceding fission reaction. The idea was to use fission to ignite fusion—otherwise known as a thermonuclear reaction—at one end of a tube of deuterium and somehow make the fusion propagate through the tube. This plan for a "superbomb," devised by American scientists, was given to Soviet intelligence authorities, most likely by physicist and spy Klaus Fuchs in 1945.

Sakharov turned out to be exceedingly adept at the combination of theoretical physics and engineering that was required in making a hydrogen bomb. Despite his junior status, he soon proposed a radically different design, called the *sloika,* or "layered roll": a spherical configuration with an atom bomb in the center, surrounded by shells of deuterium alternating with heavy elements such as natural uranium. The electrons released by the initial atomic explosion generated tremendous pressure within the uranium shell, forcing the fusion of deuterium. The Soviets called the process "sakharization"—literally, "sugaring" (the Russian *sakhar* translates to "sugar"). The fusion in turn released neutrons that enabled the fission of uranium.

The concept, enhanced by an idea from Vitaly Ginzburg—that lithium deuteride replace deuterium as a fuel—allowed the Soviet program to catch up with the American one. It was not until 1950 that American scientists realized that their superbomb design was a dud. But Stanislaw Ulam and Edward Teller of Los Alamos National Laboratory in New Mexico soon invented another design, and the thermonuclear arms race had taken off.

Although Sakharov was fascinated with the physics of fusion, his zeal in pursuing the bomb derived also from patriotism. He believed in concepts such as "strategic parity" and "nuclear deterrence," which suggested that nuclear war was impossible. His emotional investment in the project was immense: "The monstrous destructive force, the scale of our enterprise and the price paid for it by our poor, hungry, war-torn country … all these things inflamed our sense of drama and inspired us to make a maximum effort so that the sacrifices—which we accepted as inevitable—would not be in vain. We were possessed by a true war psychology."

Yet when Sakharov received an invitation to join the Communist Party, he refused because of its past crimes. He had no choice, however, when in March 1950 he and Tamm were assigned exclusively to bomb work at a secret city where weapons designers lived and worked. Sakharov learned that this military facility had been built by prison labor in the old monastery town of Sarov, situated about 500 kilometers from Moscow. The entire city was surrounded by rows of barbed wire and erased from all maps. It was known to insiders by various code names, at the time Arzamas-16.

In a Secret City

Zel'dovich was already at Arzamas-16. The physicists spent much of the day ironing out details of bomb design. Nevertheless, Sakharov found time to conceive an idea for confining a plasma, gas so hot that electrons have been stripped from the atoms, leaving bare nuclei. The plasma would destroy any material walls but could be confined and even induced to fuse by means of magnetic fields. This principle, the basis of the tokamak reactor, is still the most promising design for producing energy from sustained fusion. ("Tokamak" is derived from the Russian phrase for a doughnut-shaped chamber with a magnetic coil.)

In November 1952 the U.S. had detonated a thermonuclear device. And by August 1953 Soviet scientists were ready to test the *sloika.* At the last minute, however, Viktor Gavrilov, a physicist trained as a meteorologist, pointed out that the radioactive fallout from the explosion would spread far beyond the test site and affect neighboring populations. Somehow no one had thought of this problem. Using an American manual on the effects of test explosions, the physicists quickly worked out the fallout pattern and realized that thousands of people would have to be moved. The recommendation was followed (although, as one official informed an anxious Sakharov, such maneuvers typically cause 20 or 30 deaths).

The *sloika* was successfully tested, yielding an energy about 20 times that of the Hiroshima bomb. In a few months Sakharov was elected a member of the Soviet Academy of Sciences—at 32 its youngest physicist ever. He also received the Stalin Prize and was decorated with the title Hero of Socialist Labor. The Soviet leadership had great hopes for Sakharov: not only was he brilliant, he was also non-Jewish (unlike Zel'dovich and Ginzburg) and politically clean (unlike Tamm).

The *sloika* was, however, limited in scope—its yield could not be increased indefinitely—and soon Sakharov and Zel'dovich came up with a new design. The idea was to use the radiation (photons) generated by an initial atomic explosion to compress a tube, thereby igniting fusion within it. The design, similar to the Ulam-Teller one, had potentially unlimited yield because the length of the tube could be increased as required.

Life at Arzamas-16 was unusual in more than one way. The researchers discussed politics quite freely. Moreover, they had access to Western journals, including the *Bulletin of the Atomic Scientists,* which concerned itself mainly with the social dimensions of nuclear energy and demonstrated how scientists on the other side of the Iron Curtain sought to influence public affairs. One inspiring figure was Leo Szilard, who had discovered the "chain reaction" that makes atomic bombs possible but who turned into a vocal critic of nuclear weapons. Sakharov was also aware of the political writings of Albert Einstein, Niels Bohr and Albert Schweitzer, who doubtless influenced him as well.

A memo written by the administrative director of Arzamas-16 in 1955 noted that although Sakharov was an able scientist, he had substantial defects in the realm of politics. He had, for in-

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Sakharov's many fruitless attempts to stop unnecessary tests at last led to his realizing how little control he had over the weapons he had created.

stance, declined an offer to be elected to the Council of People's Deputies, a legislative body at Arzamas. The "defects" were to get worse.

In November 1955 the Soviets tested the unlimited hydrogen bomb. This time the shock wave from the blast collapsed a distant trench, killing a soldier, and crumbled a building, killing a toddler. These events weighed heavily on Sakharov. When asked to propose a toast at the celebratory banquet that night, he announced, "May all our devices explode as successfully as today's, but always over test sites and never over cities." Marshal Mitrofan Nedelin replied with an obscene joke, whose point was that scientists should just make the bombs and let military men decide where they should explode. It was designed to put Sakharov in his place.

As variations of the basic thermonuclear devices continued to be tested, Sakharov became increasingly concerned about the unidentifiable victims of each blast. He taught himself enough genetics to calculate how many persons worldwide would be affected by cancers and other mutations as a result of nuclear testing.

In 1957 the U.S. press reported the development of a "clean bomb," a fusion bomb that used almost no fissionable material and seemingly produced no radioactive fallout. Sakharov found, however, on the basis of available biological data that a one-megaton (equivalent to a million tons of TNT) clean bomb would result in 6,600 deaths worldwide over a period of 8,000 years because of the proliferation of radioactive carbon 14 (produced when neutrons from the explosion interacted with atmospheric nitrogen). He published his results in 1958 in the Soviet journal *Atomic Energy,* concluding that the atmospheric testing of any hydrogen bomb—"clean" or not—is harmful to humans.

The Chips Fly

Soviet premier Nikita S. Khrushchev himself endorsed the publication of this article. It suited his purposes: in March of 1958 he had suddenly announced a unilateral cessation of nuclear tests. Sakharov was not, however, playing political games. His figures revealed, as he saw it, that "to the suffering and death already existing in the world there would be added hundreds of thousands of additional victims, including people living in neutral countries as well as in future generations." He was also troubled that "this crime is committed with complete impunity, since it is impossible to prove that a particular death was caused by radiation."

In the same year Teller published a book, *Our Nuclear Future,* laying out the majority view of both American and Soviet hydrogen-bomb experts—who did not share Sakharov's concern. Teller estimated the radiation dose from testing as roughly 100th of that from other sources (such as cosmic rays and medical x-ray examinations). He also noted that radiation from testing reduced life expectancy by about two days, whereas a pack of cigarettes a day or a sedentary job reduced it by 1,000 times more. "It has been claimed," he concluded, "that it is wrong to endanger any human life. Is it not more realistic and in fact more in keeping with the ideals of humanitarianism to strive toward a better life for all mankind?" To Sakharov, that statement sounded a lot like the Soviet slogan "when you chop wood, chips fly." He felt personally responsible for any deaths from the fallout of testing.

Meanwhile the U.S. and Britain continued testing, and after six months, a furious Khrushchev ordered that testing be resumed. Deeply concerned—because of the deaths he was convinced would ensue—Sakharov persuaded Igor Kurchatov, the scientific head of the atomic project, to visit Khrushchev and explain how computers, limited experiments and other kinds of modeling could make testing unnecessary. Khrushchev did not agree, nor did he welcome the advice. Sakharov repeated his efforts in 1961, when after a de facto moratorium the premier again announced new tests. Khrushchev angrily told him to leave politics to those who understood it.

In 1962 Sakharov learned that tests of two very similar designs of hydrogen bombs were going to be carried out. He tried his best to stop the duplicate test. He pulled all the strings he could, pleaded with Khrushchev, enraged his colleagues and bosses—all to no avail. When the second bomb was exploded, he put his face down on his desk and wept.

To his surprise, however, he was soon able to solve the larger problem. In 1963 his suggestion of a ban on the most harmful—atmospheric—testing was well received by the authorities and resulted in the signing of the Limited Test Ban Treaty in Moscow that same year. Sakharov was justifiably proud of his contribution. After atmospheric testing was stopped, its harmful effects ceased to worry him.

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His concerns, however, had induced him to take two major steps: from science to the sphere of morals and finally to politics. The bomb program did not really need him anymore, but Sakharov was starting to feel that his presence would be essential to his retaining influence over the politics of weapons.

In these years Sakharov also found time to return to his first love, pure science. A problem that continues to plague scientists is the excess of matter over antimatter in the universe [see "The Asymmetry between Matter and Antimatter," by Helen R. Quinn and Michael S. Witherell; SCIENTIFIC AMERICAN, October 1998]. He laid out the conditions that could allow such an imbalance to arise, his most important contribution in theoretical physics. Vladimir Kartsev, a young physicist who asked Sakharov to write a preface for his popular science book, recalls that he looked very happy, full of creative energy and ideas about physics.

In 1966 Sakharov signed a collective letter to Soviet leaders against an ominous tendency to rehabilitate Stalin. Most tellingly, in December of that year he accepted an anonymous invitation to participate in a silent demonstration in support of human rights. But when he wrote to the Soviet government in support of dissidents, his salary was slashed, and he lost one of his administrative positions. The events, however, put him in increasing and ultimately fateful contact with activists in Moscow.

Sakharov's worldview was becoming increasingly radical, and it demanded an outlet. In July 1967 he sent via secret mail a letter to the government. He argued that a moratorium proposed by the U.S. on antiballistic-missile systems was to the benefit of the Soviet Union, because an arms race in this new technology would make a nuclear war much more probable. This nine-page memo, with two technical appendices, is now to be found in the Sakharov Archives. Among other things, the letter sought permission for publishing an accompanying 10-page manuscript in a Soviet newspaper to help "American scientists to curb their hawks." The article's style shows that Sakharov still considered himself a technical expert devoted to the "essential interests of Soviet policy."

Nevertheless, permission was refused. The rejection was yet another confirmation to the physicist that those who mattered were oblivious to the danger to which they were subjecting the world.

Early in 1968 Sakharov started working on a massive essay, entitled "Reflections on Progress, Peaceful Coexistence and Intellectual Freedom." He made no effort to hide this manuscript—the secretary at Arzamas-16 retyped it, automatically handing a copy to the KGB. (This carbon copy is now in the president's archives in Moscow.) The article described the grave danger of thermonuclear war and went on to discuss other issues, such as pollution of the environment, overpopulation and the cold war. It argued that intellectual freedom—and more generally, human rights—is the only true basis for international security and called for the convergence of socialism and capitalism toward a system that combined the best aspects of both.

The Die Is Cast

By the end of April Sakharov had released to the samizdat, or underground press, this radical essay. In June he sent it to Leonid I. Brezhnev (who had already seen it, courtesy of the KGB), and in July its contents were described by the British Broadcasting Corporation and published in the New York Times. Sakharov recalled listening to the BBC broadcast with profound satisfaction: "The die was cast."

Sakharov was ordered to stay in Moscow and restricted from visiting Arzamas-16. He had spent 18 years of his life in the secret city. He was not, however, fired from the bomb project until the next year: deciding the fate of a Hero of Socialist Labor three times over, who, moreover, knows the nation's most sensitive secrets, can be tricky. Shortly after, his wife died of cancer, leaving him with three children, the youngest aged only 11. Grief-stricken, Sakharov donated all his savings to a cancer hospital and the Soviet Red Cross.

For Sakharov, a lifetime had ended, and another was about to begin. He had 20 years of life left. He was to meet Elena Bonner, the friend and love of his life, to be awarded the Nobel Prize in Peace in 1975, to pass seven years in exile at Gorki and, unbelievably, to spend his last seven months as an elected member of the Soviet parliament.

Perhaps the best person to explain Sakharov is Sakharov. "If I feel myself free," he once mused, "it is specifically because I am guided to action by my concrete moral evaluation, and I don't think I am bound by anything else." He always did exactly what he believed in, led by a clear, unwavering inner morality. In the 1970s one of his colleagues, Vladimir Ritus, asked him why he had taken the steps he did, thereby putting himself in such grave danger. Sakharov's reply was, "If not me, who?" It was not that he considered himself chosen in any way. He simply knew that fate, and his work on the hydrogen bomb, had uniquely placed him to make choices. And he felt compelled to make them.

The Author

GENNADY GORELIK has just written a biography of Andrei Sakharov with the aid of grants from the Guggenheim foundation and the MacArthur foundation. It is to be published by W. H. Freeman and Company. He received his Ph.D. in 1979 from the Institute for the History of Science and Technology of the Russian Academy of Sciences. Currently he is a research fellow at the Center for Philosophy and History of Science at Boston University. He also wrote for *Scientific American* in August 1997, on an anti-Stalin manifesto co-authored by physicist Lev Landau.

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