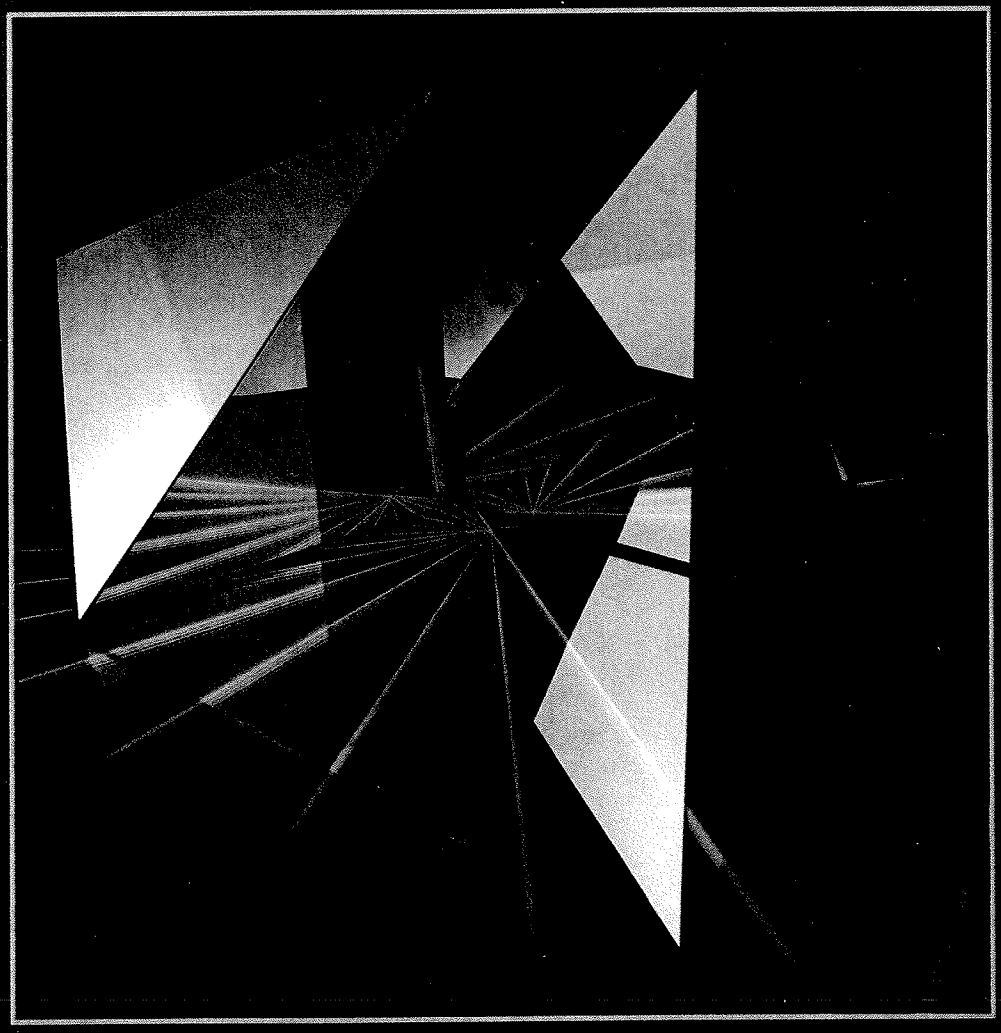
Making Physics

A Biography of Brookhaven National Laboratory, 1946–1972



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CHAPTER 12

The High Flux Beam Reactor

In September 1961, the same month that Brookhaven's second-generation large accelerator, the Alternating Gradient Synchrotron, was dedicated, ground was broken for the lab's second-generation reactor, the High Flux Beam Reactor (HFBR). Its project engineer was Joe Hendrie.

Hendrie was born in Detroit in 1925. As a youth he was good with his hands and had a strong physical intuition. Many of his male ancestors had been mining engineers; young Joe would build reactors. He entered college intending to become an electrical engineer, but soon discovered physics. In 1950, he began graduate studies at Columbia, where his teachers included some of the foremost figures in the field and several Nobel laureates or future laureates, including Polycarp Kusch (his thesis advisor), Willis Lamb, Rabi, James Rainwater, and Hideki Yukawa.

> If I'd gone to some place with a little lower intellectual aspirations [Hendrie recalled] I might have come away thinking I was a hotshot physicist. But one of the benefits of being at Columbia in those days was that you got a chance to measure yourself against the best. At Columbia, if you had any self-critical capability at all you could speedily determine where you fell in the scheme of things.[1]

Deciding that the scheme of things did not include a place for him as a physicist, Hendrie sought a job as an engineer instead. In spring 1955, with his thesis work done but not yet written up, he interviewed for a place in Kouts's group. Though the salary was well below offers he had received from private industries, Hendrie accepted it, liking the atmosphere, the sailing, and the people. Hendrie's design and procurement work impressed Kouts, while Hendrie's draftsmanship impressed the engineering staff, who were astonished to find a card-carrying Ph.D. physicist able to turn in quality engineering sketches.

When it came time to select a technical head for the HFBR project, Hendrie, all of thirty-three, was an obvious choice. He was charismatic, polymathic in interests and abilities, and a fanatic about detail. He was not

only articulate, but liked to express himself with a folksy directness that often shocked: years later, as chairman of the Nuclear Regulatory Commission at the time of the Three Mile Island reactor accident, he brutally and impolitically described the ensuing investigation as "the blind leading the blind," landing him in trouble in many quarters (among them an organization representing the disabled). Nevertheless, he was the J. Robert Oppenheimer of the HFBR; he brought home a technically complex project through a combination of a deep appreciation for engineering, a thorough knowledge of physics, and an ability to motivate and even inspire.

In fall 1958, soon after agreeing to become HFBR's project engineer, Hendrie recalled a remark he'd once heard Lyle Borst make: "Every good engineer has *one* reactor in him." He wondered what that was supposed to mean. He was about to find out.

HFBR Construction

Haworth had been pushing the AEC to allow AUI to handle the contracting; poor communication between the AEC and its contractors had hampered several large construction projects at national labs (including the BGRR and BMRR), and Haworth was eager to avoid a repetition. At first it looked like he might succeed. But Haworth ultimately lost; the AEC decided that, as a matter of policy, its area offices would hold the contracts for significant construction projects though he resolved to increase the lab's diligence in inspection and technical supervision.[2]

Shortly after Labor Day 1958, Hendrie began to make sketches of what the reactor might look like, based on Chernick's design and General Nuclear's study, while others made calculations about flux and power distributions and neutron spectra. The modeling and calculating went on for several months, allowing Hendrie to work out a practical way to lay out the core, reflector, and general configuration. Meanwhile, Downes and Kouts constructed a critical facility to carry out benchmark measurements.

During the transformation of theoretical concept into workable real object, unpleasant surprises began to surface. The first was that General Nuclear had underestimated the power requirement by a factor of two; to get the planned neutron flux, the power of the reactor would have to be doubled, to 40 MW. Hendrie was thus forced to rethink the design: first to discover which parts had to be altered to double the flux, then how to redesign them for 40 MW. The change also meant, potentially, an unacceptably large increase in the reactor budget.

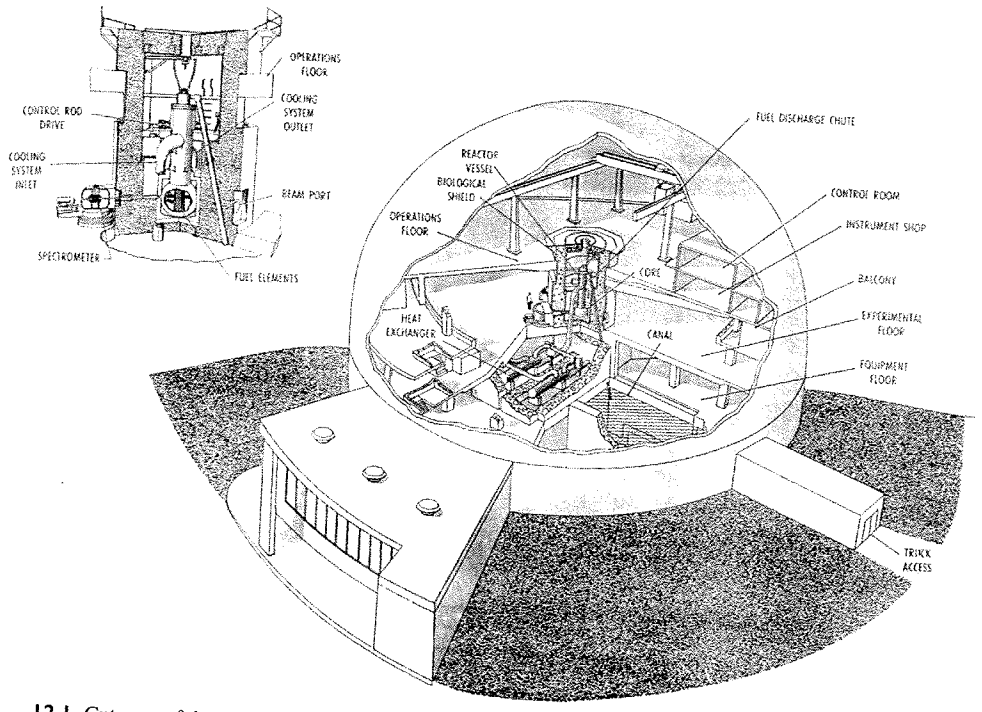
> The Congress had already appropriated $10 million. And it was in a bill which said that such projects can overrun up to 25 percent without the need to come

> back to Congress for further authorization. That didn't mean that they would put up 25 percent more money—only that the AEC would have to find the extra money itself, if it could. The feeling was that under no circumstances did one want to go back to Congress. That would have been the kiss of death—both at AEC headquarters and at the joint committee. We had gone to the AEC and said, in effect, "We need a reactor and here's what it can look like and we need $10 million." If before we begin we run back and say, "We've reconsidered and we need $15 million," we would have been bounced right out of the room. OK. So our next question was, "Can we make it work?" [3]

Hendrie felt that the basic layout of the building, including the pit structure, floor loading, cranes, and air conditioning, could stay the same. But shielding and almost everything connected with the heat transport system, including heat exchangers, pumps, and flow rates, had to be significantly upgraded. To pull this off without cost overruns, the project group sought cheaper civil engineering contracts than intended, planning to zealously ride herd on the contractors to make sure of quality, and eliminated expensive items where they could. During the design stage, the group members did not think seriously about installing an extra liner around the spent fuel pool for several reasons: it would have been a costly addition to the budget at a time when they were worried about making the budget at all, was not required by contemporary safety regulations, and seemed unnecessary given that the BGRR's fuel pool of similar construction—reinforced and tile-lined concrete—apparently had not leaked.[4] At the end of the lab's second quarter-century, this decision would come back to haunt the reactor, and the laboratory.

Hendrie then had a second unpleasant surprise. The HFBR was to be moderated by heavy water. At the time, heavy water was considered a special nuclear material by the AEC, all stocks of which were owned by the government and loaned to government operations. After the HFBR project was approved, the AEC changed this policy, and suddenly Brookhaven was required to purchase the heavy water at $28 a pound. The unexpected additional expense, a whopping $1.25 million, further cut the overrun flexibility.

These surprises pushed the effective construction date ahead about two years, but construction proceeded quickly once it began in earnest in spring 1962. By the middle of 1963, one of the few key outstanding items was the safety report. Until then, such reports usually consisted of appendixes to the reactor design report, but Hendrie wanted something more substantial. He first tried dividing up the task and farming pieces out to various members of the safety committee, but was dissatisfied with the results: "I needed to write an integrated document." He took the project upon himself. "It was a bloody sweat. I got a staggering writer's block; it seemed to



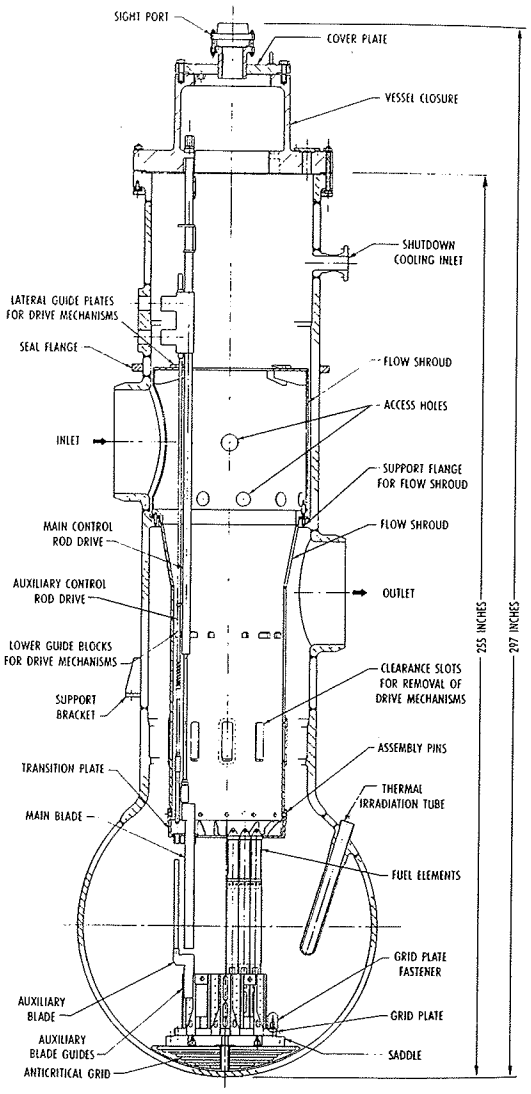
12.1 Cutaway of the HFBR, showing arrangement of building and reactor. The reactor, auxiliary equipment, and experimental facilities are housed in a steel hemisphere 176 feet in diameter. The building is kept at a slightly lower pressure than the outside, so that any air flows in the building rather than out.

me there was so much to be said that it was impossible to say it."[5] In September 1963, he began working on the report seven days a week. He'd work at the lab all day, return home for dinner and to see his family, and then be back to the lab to work until late at night, every day for nine months, including Christmas Day 1963. The result, a two-volume work of 612 pages issued in April of 1964, is a remarkable document, for it amounts to a complete design report of the reactor.[6] The report earned him fame in the AEC's Washington office, and became for them the model of what they were looking for in a safety report.

The reactor's construction was completed in September 1964 (at a cost of $12.5 million), when testing began on the various systems (fig. 12.1). Operation was scheduled for October 1965. Hendrie now felt he knew what Borst had meant.

> It's something that occupies you totally. You pour energy into a project like that at a rate that a young, healthy man can maintain for four to five years, but you

burn out. You juggle a thousand balls in your mind, all of them important, and keep an eye on how they all fit together. I was never able again to focus the energy and encompassing understanding of everything that went on that I did on the HFBR. It's not just the technical details; in some ways, these are not the most wearing. On a given technical problem you get good people together and they haggle and turn it and twist it until they get it. No, it's the administration: the reactor division, the steering committee, the users (who always want more flux),



12.2 Cutaway of the reactor vessel and its internal components, including the core, control rods or blades, and control rod drive mechanism. The inlet and outlet are for the heavy water moderator/coolant.

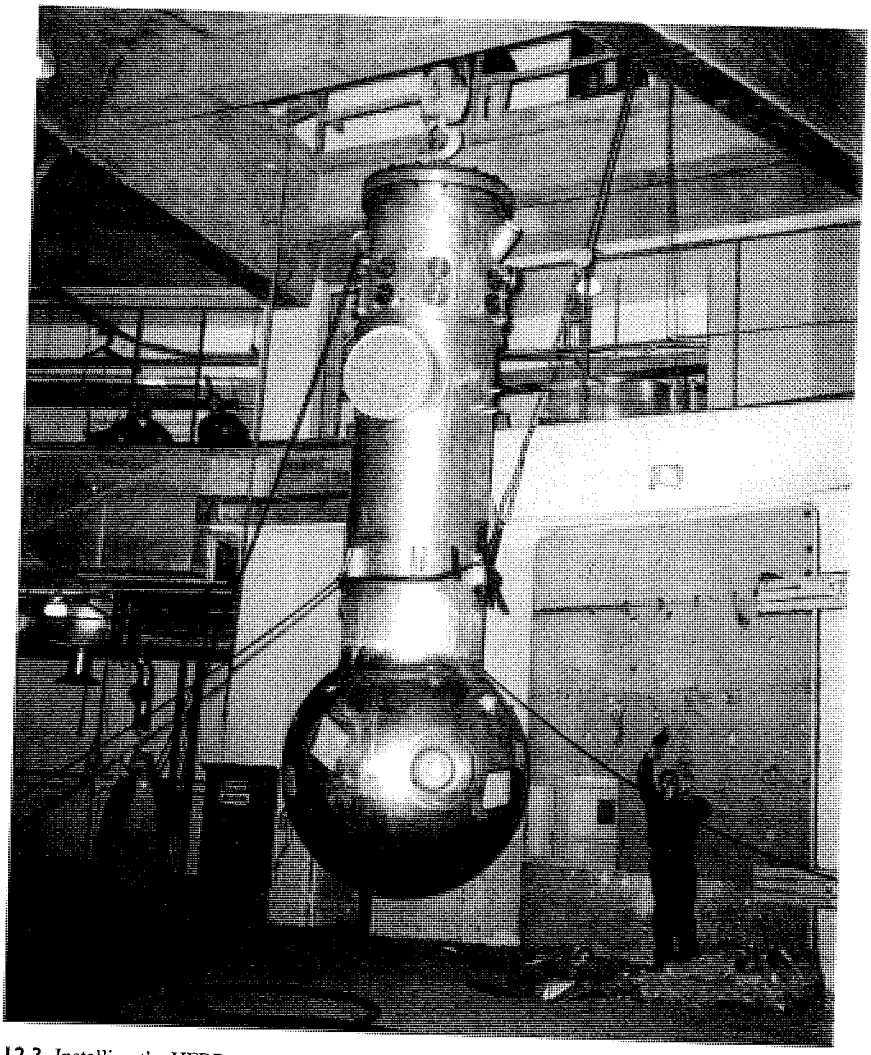
the AEC (which always wants lower cost). And in this array of groups and "ethnicities" there are all kinds of personal quirks and egos, and a lot of them don't like each other, or have quite different views on things, and are not in a mood to be amiable about sitting down and compromising. You've got to keep that all in hand. I more than anybody else understood everything and worried about everything. I *possessed* the HFBR concept. This job is like a Swiss watch. Everything has to work, otherwise it won't go. And you've got to keep all these people patted into shape and working amiably together.[7]

The HFBR finally went critical on Halloween evening, 31 October 1965 (Oak Ridge's HFIR had gone critical that August). Several weeks later, the cover of the lab Christmas card sported a picture of the HFBR's silvery dome: snow-clad, silhouetted against a deep blue sky, framed by pine trees.

Like the AGS and any other large instrument, the HFBR required a testing period of a few months, during which a number of problems were ironed out. One of the most serious concerned the anticritical grid, a set of stainless steel bars that had been installed in the bottom of the core vessel. One of its functions was to ensure that, if some freak accident melted the core, the fuel would not coalesce in a critical mass and generate heat. Another function was to break up the jet of water flow and prevent erosion of the vessel; instead, the flow (thirty-five feet a second, about seventeen thousand gallons a minute) broke it up, jarring some bars enough to crack the welds and break free. This created the danger that some might strike the beam pipes. At first it looked like repair would have to involve removing the entire reactor vessel, but a way was discovered to reach inside the vessel and remove loose bars as well as inessential ones that might eventually break free.

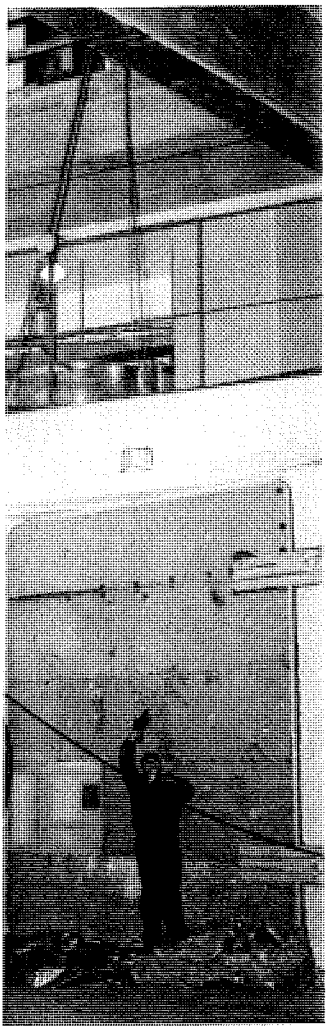
The HFBR was water cooled and did not produce argon as a waste product, as had the BGRR. Defective fuel elements were quickly identified by the presence of fission products (usually iodine) in the heavy water; the machine would be shut down and the defective element found and replaced. The new, troublesome by-product of the HFBR was tritium, an isotope of hydrogen produced in the heavy water coolant/moderator when a neutron reacted with the deuterium. The reactor engineers established a limit of tritium concentration in the heavy water, and when this was reached they would send it to Savannah River and replace it with "clean" heavy water.

In early 1966 the HFBR had reached its design power and was ready for experimenters. The look of the HFBR experimental area was much different from that of the BGRR. Instead of a pegboard-like cube with its sea of regularly spaced holes, the HFBR was an eight-sided solid about twenty-five feet across, with one beam hole (occasionally subdivided) per face. The total number of neutrons produced by the HFBR, which would run typically at about 40 MW, was only a third more than that of the BGRR, which



12.3 Installing the HFBR reactor vessel, March 1964.

usually ran at about 22 MW—but since the HFBR's active core area is much smaller (90 liters) than the BGRR's (270,000 liters, in the original design), its flux (a measure of neutron density) was much higher. But only so many beam pipes could be installed around the undermoderated core before it would adversely affect the reactivity itself; thus the number of holes was limited to nine.[8] The HFBR was also much cleaner (i.e., having fewer emissions and waste products) than the BGRR, one reason being that



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so much cleaner (i.e., having
BGRR, one reason being that

the vertical irradiation holes were completely isolated from the experimental floor.

As long as a reactor operates reliably, operators and users are essentially decoupled; typically, many fewer conflicts between the two groups occur at a reactor than at an accelerator. But the vast shrinkage in the number of experimental holes, from sixty-one at the BGRR to nine at the HFBR, drastically changed the character of the experimental community and created a potentially serious management problem. Already in 1963, requests for experimental space seemed to exceed supply, and lab officials feared a fierce competition not unlike the mad scramble for beam time at accelerators. To head it off, Goldhaber decided to appoint what would amount to a program committee. Users resisted, arguing that the determination of who would use which hole should be done informally, in traditional Brookhaven style.[9] Goldhaber went ahead anyway to establish an HFBR Experimental Facilities Committee, chaired by Powell.[10]

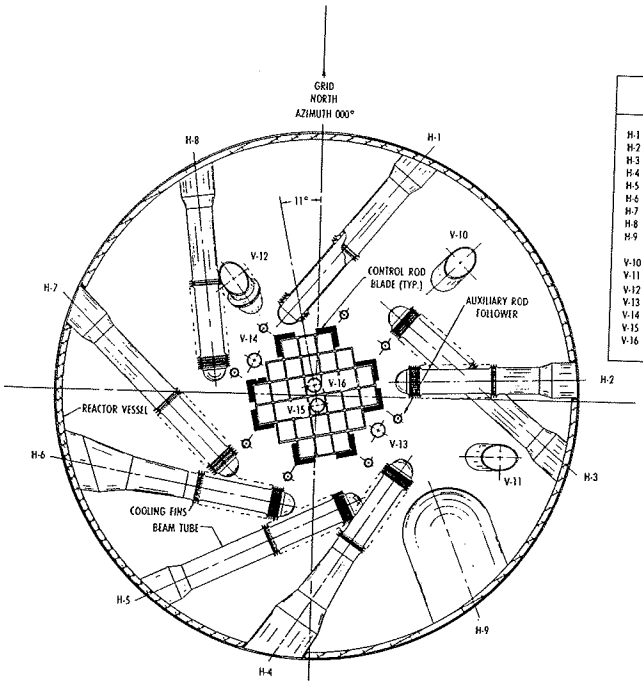
Powell so loathed the idea that he resolved that it never meet. He succeeded. One reason was the way the hole charges were assigned. These fees were significant, due to the small number of holes: few, if any, outside users could undertake a financial burden of that magnitude (also, the fact that the HFBR had been optimized for beams rather than irradiations reduced its attractiveness for some potential users). The charges were assigned to the departments that paid for them, which made it relatively easy to put the decision of who worked at which hole in the laps of individual departments rather than the program committee.

Another factor involved instrumentation. Spectrometers and detectors were larger and more complicated than they had been at the BGRR, and had the effect (though to a smaller degree) that bubble chambers had on accelerator research: promoting greater group stability and long-range planning. Plus, the techniques of neutron scattering (unlike those of X-ray and electron scattering) were hard to learn and could be practiced only at a small number of holes with the right equipment. "It's like playing a cello," says Gen Shirane, later the head of the HFBR's experimental solid-state group, "they're rather difficult instruments to play, there are not many around, so not many people can play them well." As a consequence, the ability of an outside group to come in and start working from scratch was almost nil. For all these reasons, Powell succeeded in his effort to encourage prospective users into collaborations with existing groups. The HFBR was not a user facility, certainly not what the BGRR had been. A large visitor program was no longer possible, and no longer needed.

The HFBR is a classic example of a scientific instrument created for one purpose (nuclear physics) and used mainly for another (neutron scattering, especially inelastic scattering). The impetus for it was largely Hughes's

quest for a facility with more intense flux for neutron physics research. But by the end of the 1950s, that field had changed. Mapping cross-sections for practical applications, in which so much of the exploration of the new world of atomic energy consisted, was largely completed. Many other traditional areas of nuclear research, including low-energy nuclear levels and states, had also been largely explored. And all of Hughes's gamesmanship could not obscure the fact that accelerator-based neutron sources were now rival facilities for most remaining cross-section measurements. Finally, Hughes's death in 1960 deprived Brookhaven of its most aggressive and influential voice for traditional nuclear physics. While Palevsky, Hughes's loyal lieutenant, assumed the mantle of the neutron physics group, Palevsky was no Hughes. Not only did he lack the commanding presence of his former boss, but his interests had changed to accelerator research and he was rarely seen on the HFBR floor.

Of the HFBR's nine holes (H-1 through H-9), three were for nuclear



EXPERIMENTAL FACILITY		INSIDE DIAMETER OF TUBE
H-1	FAST NEUTRON BEAM	3.5″
H-2	FAST CHOPPER BEAM	3.5″
H-3	THERMAL BEAM	3.5″
H-4	DUAL THERMAL BEAM	3.5″
H-5	THERMAL BEAM	3.5″
H-6	DUAL THERMAL BEAM	3.5″
H-7	THERMAL BEAM	3.5″
H-8	INTERMEDIATE BEAM	3.5″
H-9	THERMAL OR COLD BEAM	12″
V-10	THERMAL IRRADIATION THIMBLE	3.5″ (VESSEL); 1.87″ (SAMPLE TUBE)
V-11	THERMAL IRRADIATION THIMBLE	3.5″ (VESSEL); 1.87″ (SAMPLE TUBE)
V-12	THERMAL IRRADIATION THIMBLE	3.5″ (VESSEL); 1.87″ (SAMPLE TUBE)
V-13	CORE-EDGE IRRADIATION THIMBLE	1.37″ (SAMPLE TUBE)
V-14	CORE-EDGE IRRADIATION THIMBLE	1.37″ (SAMPLE TUBE)
V-15	IN-CORE IRRADIATION THIMBLE	1.37″ (SAMPLE TUBE)
V-16	IN-CORE IRRADIATION THIMBLE	1.37″ (SAMPLE TUBE)

12.4 Diagram of the beam tube and irradiation thimble layout in the reactor vessel, showing the twenty-eight fuel element positions, and two irradiation facilities, of the core. The eight right-angle control rods or blades are alongside the core in black. H-9 is a large (twelve-inch-diameter) thimble-shaped hole for the cold neutron source.

physics and six for neutron scattering. (It also had seven vertical thimbles for irradiations, V-10 through V-16, with slightly different neutron energy spectra.) Each beam had a somewhat different neutron profile; the more directly its pipe pointed to the core, the harder (more energetic) its neutron spectrum. The instruments at each hole were tailored to specific projects.

H-1 to H-3

Nuclear physics was conducted at H-1, H-2, and H-3. But nuclear physics at the HFBR was a much different effort from what it had been at the BGRR. Gone were the days of cozy intimacy between basic and applied interests, and the Cold War "emergencies" of the early 1950s, when the AEC breathed down researchers' necks to churn out quickly new cross-section data and was willing to contribute endless resources for the cause. The cross-section work of the 1950s at many of the national laboratories, and completion of the Bohr-Mottelson model, had stabilized the picture of the nucleus for basic researchers and provided reactor and weapons engineers with much of the essential information they needed. Accelerator-based neutron sources had substantially reduced the viability of reactor-based cross-section work. Nuclear physicists at the HFBR therefore applied the new flux in expanded research programs, usually in nuclear excitation spectroscopy; the study of high excited state levels and their properties.

Vance Sailor's nuclear cryogenics group took up residence at H-1, where it used the HFBR's more intense neutron flux to study spins of resonance states of nuclei. Because he needed neutrons of somewhat higher energies than neutron scatterers desired, he used a beam tube that pointed more directly at the core than theirs, but still tangent. That it was tangent meant neutrons in the beam had to have at least one collision with the moderator (eliminating gammas and very high-energy neutrons from the reactor core), while the fact that it was pointed more directly at the core meant the neutrons would not have had a large number of collisions. To polarize the targets, his group built another refrigerator, much larger than his previous one, with a superconducting coil able to handle bigger samples and cool them to lower temperatures. It was massive, a two-story piece of equipment that took more time and money to build than expected, far and away the largest piece of equipment built during the first years of the HFBR.[11]

But the group members ran into unanticipated difficulties once they began to tackle higher excited states of uranium. One was that the uranium resonances were extremely close together or even overlapped, requiring exceedingly high resolution to disentangle. And after all the labor involved in building the refrigerator, studying such states seemed anticlimactic and to have run its course, even to Sailor himself. Lab administrators took note.

When, in the late 1960s, they were forced to cut back on research areas deemed unlikely to move forward, Sailor's was one. With the wind taken out of his sails, Sailor grew unhappy, a little resentful, and seemed to lose his sense of purpose. "I'd been at it for almost twenty years," he recalled, "and we were repeating the same kinds of things we'd done before but with different targets. Basically, we were contributing more data to the "Barn Books." After a certain point you lose enthusiasm." [12]

In 1970, he saw the opportunity for a change. Leland Haworth had returned to the lab as a consultant to the director and established a small group to carry out a series of studies on U.S. energy consumption and resources. With the encouragement of the administration, Sailor left H-1 and the physics department to join Haworth's group in the department of applied sciences.[13] David Rorer, a member of Sailor's group, ran the program for a while, but the huge refrigerator was soon dismantled. Sailor also became interested in nuclear power. The Long Island Lighting Company (LILCO) was planning to build a nuclear reactor in the nearby town of Shoreham and hearings on the construction permit began in late 1970. Some conservation groups opposed to the plant became involved in the hearings; Sailor, an advocate, founded a private organization called Suffolk Scientists for Cleaner Power and Safer Environment that became involved in support of the reactor. He began to invest considerable time and energy in the proposed Shoreham plant—but he would be bitterly disappointed, too, in the fate of that project.

The neutron physics group was stationed at H-2. Its head, Harry Palevsky, worked at the Cosmotron almost full-time, and Robert Chrien was in charge of the group's HFBR effort. Chrien had arrived at Brookhaven in 1957 to work with Hughes, only to be sent to Chalk River in 1959, where the flux was higher and resolution better. There, Chrien measured cross-sections using a copy of the fast chopper, built by Hughes and shipped off to Chalk River a few years previously with group member Robert Zimmerman, in the days when the resolution of accelerator-based neutron sources was just beginning to overtake that at the BGRR.

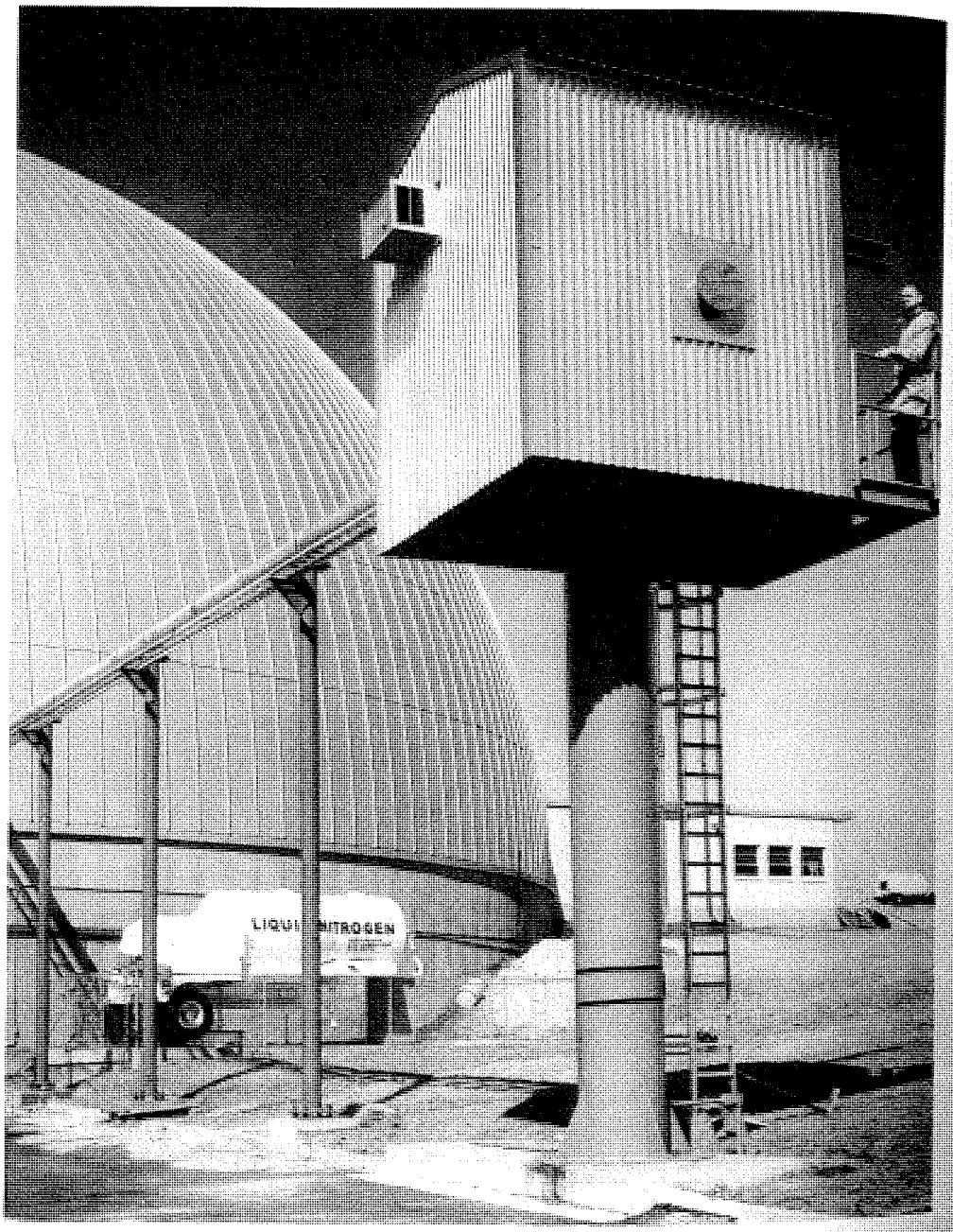
> In those days [recalls Chrien], we had hunks of uranium all over the place and carried it around and nobody paid any attention, except once, when Zimmerman got into trouble crossing the border into Canada with some uranium in his car. We had all these rare earths, *pounds* of them, and stored them in the cabinets and desk drawers, wherever, along with a lot of gold and platinum.[14]

In 1962, Chrien returned to Brookhaven to begin building a fast chopper for use at the HFBR. Because accelerator-based systems were now superior for traditional cross-section research, they modified the chopper's design to study what were called capture gamma reactions, which were one way of

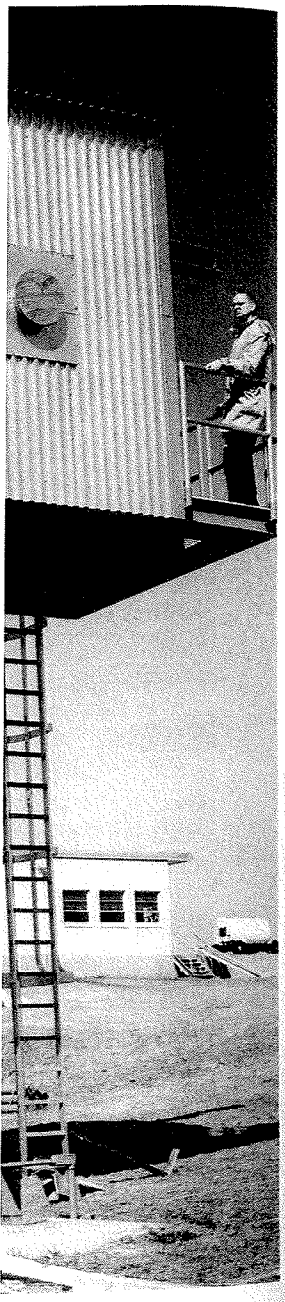
examining high-energy nuclear levels. While at low excitation energies, nuclei occupy a relatively small number of clearly defined fixed states, at high energies the states are numerous and apparently chaotic. A highly excited nucleus typically sheds its excess energy by dropping from state to state to state in a cascade process, via emission of a series of gamma rays and often by different routes, until it reaches the ground state. By using a gamma counter and coincidence electronics, nuclear physicists could examine these cascades to see what patterns (selection rules, for instance) might lie in them. Hughes's group had done such work at the BGRR, and Chrien carried it forward with the new high flux. A new type of "lithium drifted" gamma detector, made from germanium crystals doped with lithium, had recently been developed that vastly upgraded the ability to detect gammas over the old sodium iodide detectors.[15] Chrien's was the only group principally interested in high-energy neutrons, and the H-2 beam tube was pointed directly at the core to give it a beam rich in high-energy neutrons.[16]

Because Chrien had sacrificed much in the way of resolution for intensity, in 1968 he improved the neutron resolution by building a longer flight path (fig. 12.5). A hole was cut in the containment vessel through which neutrons would coast along a forty-eight-meter flight path to a special station (Chrien 1980). Eventually, accelerator-based sources (notably ORELA at Oak Ridge) became intense enough to overtake the resolution of reactor sources for capture gamma studies, too. In 1979, the chopper was removed and (at the request of the Department of Energy) replaced by an isotope separator called Tristan, whose purpose was to produce various fission products for collection and study in a mass separator. The chopper, its cobalt-loaded steel core slightly radioactive from years of assault by neutrons, now sits in a storage field.

The nuclear structure group, founded by the Goldhabers, was the first occupant of H-3. The group consisted of Gertrude Scharff-Goldhaber (the senior member), Ed der Mateosian, Walter Kane, and Andrew Sunyar. This group, too, was doing nuclear spectroscopy, but its effort at the reactor was small. The group had only enough resources to maintain one of the two ports at H-3, at which they built a crystal spectrometer. Thus nuclear spectroscopy at the HFBR, as at the BGRR, was carried out by two complementary techniques: time of flight (Chrien's group at H-2) and crystal spectrometer (the nuclear structure group at H-3). Otherwise the nuclear structure group employed the same basic method as had Chrien's, using the HFBR's neutrons to pump energy into nuclei, creating excited states that returned to the ground state via a cascade of gammas that were used as a clue to the resonance structure. When Sailor left H-1, Kane became a co-group leader with Chrien at one of the ports there. The nuclear structure



12.5 Cabin at the end of a long time-of-flight path to improve resolution for high-energy nuclear level studies.



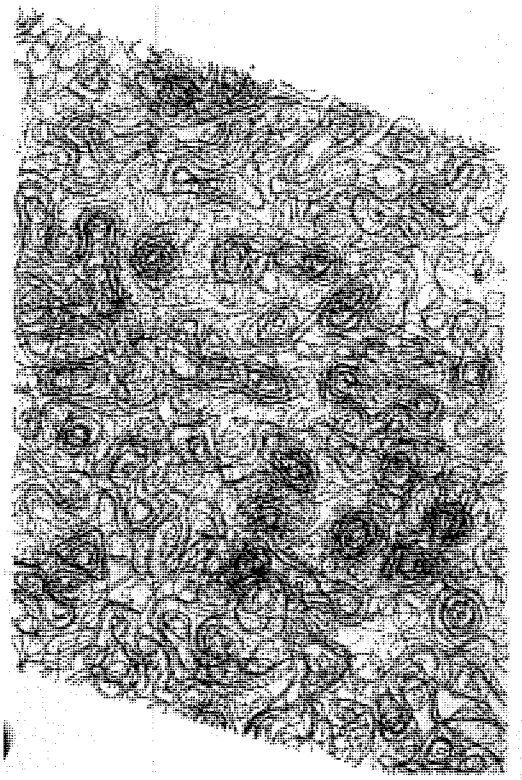
on for high-energy nuclear

group's H-3 port and crystal spectrometer were given to Swiss biologist Benno Schoenborn, who had been making substantial progress in neutron crystallography and needed a low-energy hole for his studies.[17]

In 1964, Schoenborn received a research fellowship to study at the Cavendish, a center of X-ray crystallography of proteins (now called structural biology). Two years previously, Cavendish scientist John C. Kendrew had received the Nobel Prize in chemistry for his X-ray crystallography of sperm whale myoglobin, a protein consisting of a single chain of some 150 amino acid units with a combined total of about twenty-six hundred atoms. Kendrew chose sperm whale myoglobin partly because it was a small and stable, easily crystallizable molecule readily available in large quantities, and partly because myoglobin was structurally related to hemoglobin, a protein that was not only a vitally important biological compound, but also had been closely studied by Kendrew's colleague Max Perutz, making it easy for Kendrew to tap a wealth of information about it. "[S]perm-whale myoglobin," Kendrew and company wrote, "possesses a structure the significance of which extends beyond a particular species and even beyond a particular protein" (Kendrew et al. 1960, 422). Kendrew had determined fairly well the location of everything but the hydrogen atoms, which X rays could not detect (Kendrew et al. 1960). As Donald Hughes had noted already in his *Pile Neutron Research* (Hughes 1953b), neutrons hold a great advantage over X rays in this kind of work given an intense enough beam. Schoenborn was one of those impressed by the significance of locating the missing hydrogen atoms, and aware of the possibilities of neutrons.

> About half the atoms in a protein are hydrogens, and virtually all functional activity in a protein is mediated by hydrogens. So every time you have an enzymatic reaction in a protein, a hydrogen is involved. In a lot of cases, one could speculate what's going on, but you didn't really know if its there or not. The only way to do it properly was to know where the hydrogens are. And the only way to find the hydrogen atoms, I began to realize, was neutron scattering.[18]

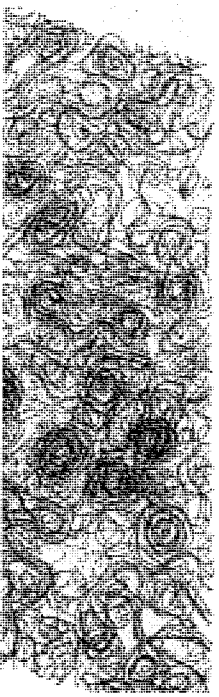
When his fellowship expired, he returned to San Francisco to apply neutron diffraction to proteins (diffraction being the part of scattering concerned with structural studies by elastic scattering), and figured that the required flux was about that of Brookhaven's new HFBR. Biology department chairman C. H. W. Hirs was encouraging and invited Schoenborn to visit. Schoenborn's project amounted to an entirely new direction both for biologists and for the HFBR. Previously, all crystallography of large biologically interesting compounds such as proteins had been done with X rays; when biologists used reactors it was either to irradiate samples or create tracers. All the compounds that had been studied with neutrons had



12.6 Neutron density map used by Benno Schoenborn.

at most a few dozen atoms; Schoenborn's project involved a structure with twenty-six hundred atoms.

Schoenborn's first major problem was poor instrumentation. While the technology of high-energy physics detectors was constantly being pushed to the envelope, that of neutron detectors suitable for crystallography lagged by comparison. Existing neutron detectors were mounted on an arm that was slowly moved, point by point, through a series of angles, stopping at each to scan a reflection or peak; this would be a hopelessly slow way of measuring the tens of thousands of reflections that Schoenborn needed. Members of the department of instrumentation and health physics developed an electronic refinement that dropped the resolution from several millimeters to 1.1, a vast improvement; they also applied their expertise in high-energy charged particle detectors to help him develop a detector that could collect data in two dimensions, reading out the coordinates on both the X and Y axis (fig. 12.6).



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As his first major target protein, Schoenborn like Kendrew selected sperm whale myoglobin, which plays for protein crystallographers somewhat the role sodium chloride has for chemists studying bonds; for various historical reasons it is *the* compound on which to test a new technique, about which the most on the subject has been worked out. By 1972, he had managed to use neutron diffraction methods to determine the positions of all its twenty-six hundred atoms, including those of the all-important hydrogen atoms—a breakthrough in protein crystallography. That year, a detail of Schoenborn's picture of the molecule graced the cover of the lab's Christmas card.

Neutron Scattering: Holes H-4 to H-8

By the time the HFBR was completed, the field of inelastic scattering was on the rise. In inelastic scattering, neutrons gain or lose energy as they rebound from the lattice excitations, meaning that energy transfer has occurred. If it is an energy gain, they have picked up energy from an excitation of the solid; if a loss, they have given it up in creating some excitation. This reveals information about lattice vibrations, which may be of several kinds, including phonons (density fluctuations of the lattice, analogous to the fluctuations caused by a sound wave), and spin waves (a similar kind of wave effect, but involving the spin orientation of the atoms rather than their locations). Phonons and spin waves, in turn, are crucial for understanding such phenomena as phase transformations, superconductivity, and magnetic properties.

The field was booming for several reasons. First, the instrumentation had considerably improved, thanks largely to the development, by Bertram Brockhouse at Oak Ridge in work that would later earn him a Nobel Prize, of the triple axis spectrometer, soon to become the basic tool of inelastic scattering (described in Brockhouse 1986). In addition, the theory of neutron scattering had advanced to a new level of sophistication, thanks to the cumulative work of several people, among them Fermi, J. Schwinger, O. Halpern and M. H. Johnson, G. Placzek, and Van Hove. Walter Marshall, a theorist from Harwell (then a world capital of neutron scattering), synthesized and extended this work in an influential set of lectures at Berkeley and Harvard in the course of a year spent in U.S. in 1958–59 (Marshall and Lovesy 1971). Finally, in the early 1960s, a number of breakthroughs had taken place in the understanding of some of the processes examined by inelastic scattering.

Inelastic scattering had been possible, just barely, at the BGRR, but the low flux meant that research was effectively restricted to detecting effects and the most important research took place at reactors elsewhere. By the

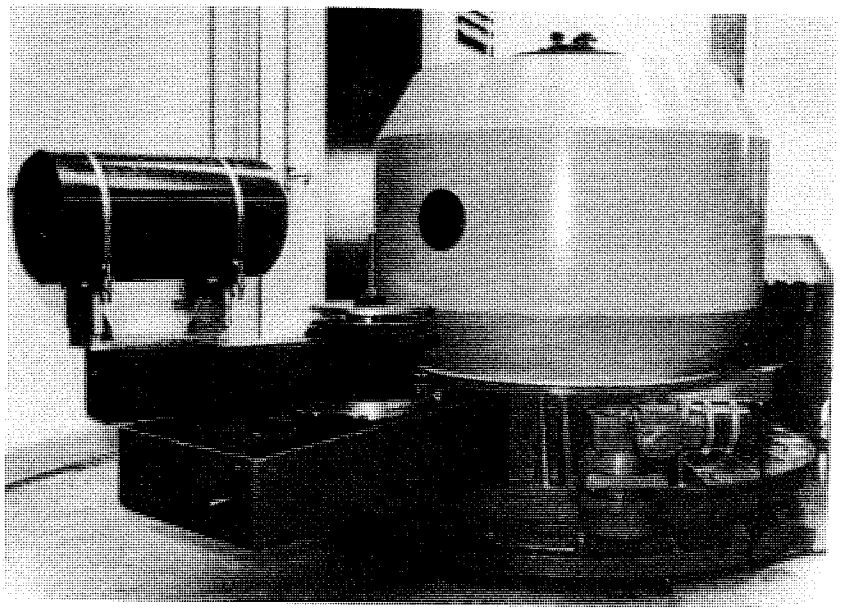
Jacket photographs: Courtesy of Brookhaven National Laboratory

mid-1960s, it was clear that it would be the field of promise of the HFBR. One sign of the HFBR's newly reconceived purpose was a conference held in September 1965, a month before the reactor was scheduled to go critical. The conference, Symposium on Inelastic Scattering of Neutrons by Condensed Systems, was attended by over 160 of the most renowned experimenters and theorists in the field.[19] The opening talk, "Comparison of Electromagnetic and Neutron Studies of Solids," was delivered by a young neutron scattering theorist, Martin Blume.

As a graduate student, Blume had shared an office with Marshall, from whom he picked up an interest in neutron scattering, during a visit by the latter to Harvard in spring 1959. When Blume arrived at the lab, in June 1962, he joined Dienes's solid-state group at a time it was undergoing both an expansion and change of direction. While initially it had been preoccupied with radiation damage in solids, a subject closely linked to applied engineering questions related to the reactor, it was now moving more toward the mainstream of solid-state physics, which involved many-body problems. These involve properties that are a consequence of the interactions of large numbers of particles: superconductivity is a many-body problem (there is no such thing as a single superconducting electron), as are phase transitions of all kinds (magnetic and structural), crystal growth, and applications of statistical mechanics. As it happens, a large number of these problems can be addressed through neutron scattering. Blume's arrival in 1962 both reflected and reinforced a reorientation of Brookhaven's solid-state program in this more mainstream direction.[20]

The five holes H-4 through H-8 were to be used for various solid-state research projects by members of both the chemistry and physics departments, solid-state physics still having somewhat ill-defined interdisciplinary borders ("squalid-state physics," in Gell-Mann's phrase). Similar equipment was needed at each. To economize on design and construction, a basic spectrometer was built, known as US-1, which (though sounding like the name of a highway) stands for Universal Spectrometer 1 (fig. 12.7). US-1 was designed to be readily convertible to a number of applications: single-crystal spectrometer, double-axis spectrometer for elastic scattering, or triple-axis spectrometer for inelastic scattering. Its physics conception was by Harvey Alperin, its engineer was Andrew Kevey.

Kevey was born in Hungary in 1923, and his route to Brookhaven was, literally, material for a novel (Kevey 1991). He had a childhood ambition to enter the Ludovika Academy (the Hungarian West Point); was refused until the war against the Soviets depleted Hungary's military academies and created a scramble for cadets; was accepted into the Ludovika and sped through in barely a year and a half; trained briefly in Germany be-



12.7 US-1, the Universal Spectrometer developed for the HFBR.

fore being sent against the British and captured without ever having fired a shot against the enemy; spent a year and a half in Allied POW camps; languished another year and a half in rat-infested displaced-person camps; found work in England as a domestic servant; took the boat to New York City with no marketable skills and poor English; and went to night school and eventually transferred to Brooklyn Polytechnic Institute, from which he graduated in 1959 with a degree in mechanical engineering.

That year, he was hired by Brookhaven, was responsible for assembling the AGS injector, and then was put to work on a neutron spectrometer under Chalmers Frazier and Robert Nathans. Kevey had to learn the need to anticipate the rapidly changing demands of experimental physicists. One day, Nathans told Kevey to design a small calibrated turntable for mounting a crystal, telling him that it would have to bear a weight of a few ounces—certainly never more than a pound. Three months later, Nathans asked Kevey to install a fifty-pound magnet on the table. Kevey protested. "But Andy," Nathans replied, "don't you know that, in physics, *never means three months*!" [21]

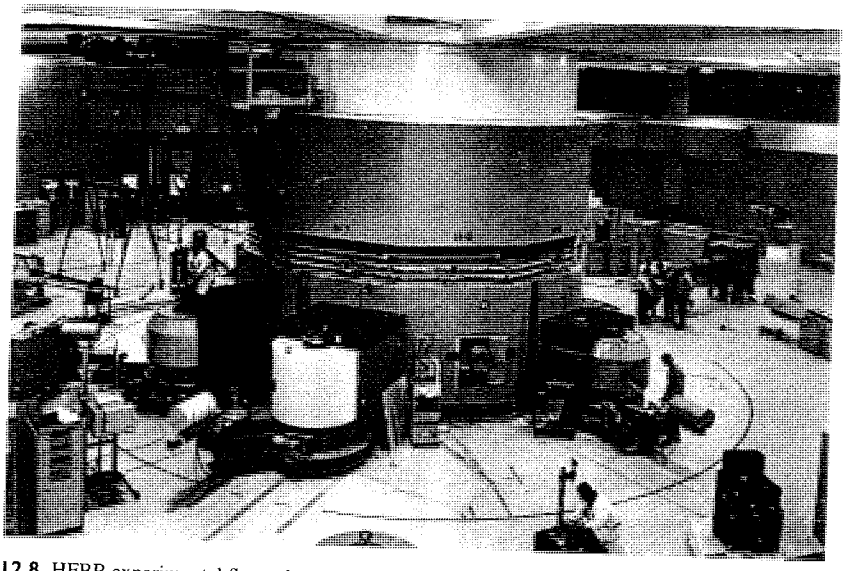
After construction began at the HFBR, the former domestic servant was put in charge of engineering US-1, the huge new spectrometer. One

of Kevey's biggest engineering challenges was to provide a suitable bearing to support the weight of the drum (about five tons), the overturning moments from the second (and possibly third) axis, and the detector itself, all the while with gearing of high accuracy. Kevey also worried about the Nathans syndrome; future loads placed by cryostats, magnets, and detectors much heavier than currently conceivable. In addition, he knew that building a bearing and drive from scratch would be prohibitively expensive given the budget.

One day Kevey gave a lift to an old retired navy captain named Mead who worked at the lab in procurement. Kevey happened to mention his bearing problem, and Mead said he would inquire through his old navy channels. A few days later, Mead called back with good news. The navy had nine 40 mm World War II antiaircraft gun mounts in its warehouse near San Francisco, and it might be possible for Brookhaven, as a government laboratory, to obtain them. Kevey flew to San Francisco, inspected one of the mounts, obtained the drawings, and saw that these would make perfect bases for the new spectrometers. The bearings were rigged to withstand the kick of the guns when fired, and the gears were cut with high precision to orient the guns properly. He flew back and put in paperwork for all nine. The gun mounts were free; the lab paid about a thousand dollars for cleaning, regreasing, and shipping. Although many of the HFBR spectrometers have since been modified to accommodate new demands, the gun mounts have never been replaced.

Nine spectrometers were under construction in the Brookhaven shops: five for the HFBR, two for the naval ordnance laboratory, and one each for labs in Puerto Rico and Israel. The HFBR spectrometers were hooked into a large-scale time-shared computer data acquisition system, in which the spectrometers were serviced by SDS 910 (neutron physics) and SDS 920 (neutron diffraction) computers, and in 1970 by a PDP-11. The computer would not only take data on line, but instruct each spectrometer how long to stay at one stop, how many degrees to move to get to the next stop, and so forth, a far cry from the manual settings and pencil data taking of just fifteen years previously. The prototype for this system, at Brookhaven, had been the time-shared slow and fast chopper data acquisition facilities at the BGRR.[22] By 1966, the spectrometers were ready on schedule and within budget, and worked well (fig. 12.8). It was the first major project Kevey led himself.

> It meant much more to me than my degree. That's only a dead piece of paper with a stamp and several signatures now hanging in my den. But those spectrometers are alive. They hum day and night, spewing out data for a dozen scientists. And they were my creations. I had an inner glow within me. (Kevey [n.d.])



12.8 HFBR experimental floor, showing three neutron spectrometers in position.

H-4, H-7, H-8

In 1965, the solid-state group had four senior staff members (Dienes, Frazier, Nathans, and Shirane), as well as several postdocs. Dienes was head of the group. But, as was often the case at Brookhaven, the actual functioning of the group bore little resemblance to its official structure. An informal, anarchic situation prevailed on the HFBR experimental floor, with power in the hands of the most ambitious, notably Nathans and Shirane. For a while these two worked relatively independently of each other, with Frank Langdon, the chief technician in charge of the experimental floor area, serving as a key intermediary in deciding the chief priorities. Shirane became head of the solid-state neutron group after Nathans's departure in 1968.

Shirane was a leader in the Donald Hughes mold. He liked nothing better than to focus deeply on a technically demanding experiment, and was notorious for driving subordinates hard. Economical with both words and time, he was known for scheduling not only the start of a meeting but also its end. If at the end of twenty-five minutes or however long the meeting was scheduled to last the issue in question was unsolved, Shirane would dismiss the group, sending them back to their offices to work on it.

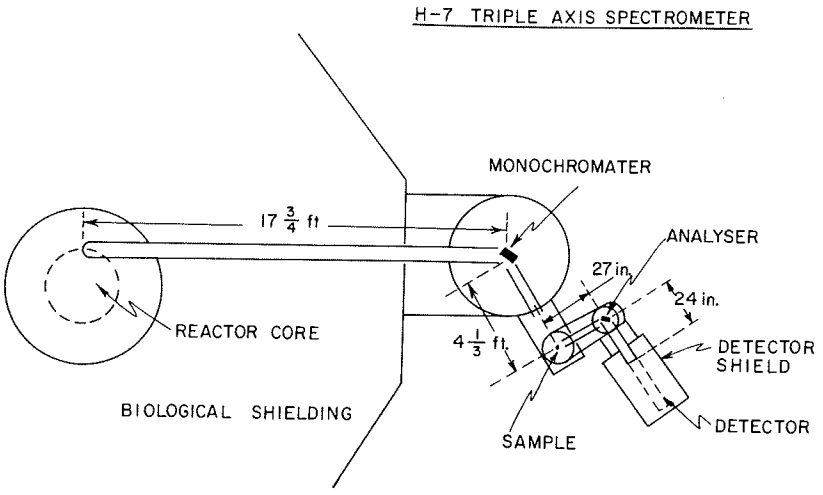
Shirane was another who followed a circuitous route to Brookhaven involving an extraordinary amount of luck. Born in Japan in 1924 in a small

town between Osaka and Kobe, he began to study science after passing an exam that entitled him to enter a fast-track high school. "Therefore," says Shirane, "I didn't die. All my friends in the soft sciences, liberal arts and literature, were drafted. Most went into the navy, became pilots, and died. But students in science and technology were not drafted."[23] Even so, he came perilously close to not surviving the war. When he entered the University of Tokyo as an engineering student, he and his classmates were randomly assigned to different campuses. Shirane's happened to be outside Tokyo proper, which was not subject to the massive bombings that devastated most of that city. He studied aeronautics, intending to become an airplane designer, and among his instructors was Itokawa, designer of the famous Japanese Zero fighter. The toughest year of the war was the last, when Shirane and his colleagues had little to eat except the pumpkins that grew even in ravaged soil. But the war ended abruptly. The occupation force disbanded all activities it decided were war related, among which was the University of Tokyo aeronautics department. What was left of the department was converted to applied physics, and Shirane suddenly found himself in a new field. He graduated in 1947.

> For me, it was lucky. Only about one out of hundred aeronautics graduates design airplanes; the other ninety-nine calculate things like the strengths of materials. Therefore, you might say I became a physicist by chance. I didn't want to become a physicist. But I was not disappointed.

When Ray Pepinsky, a scientist at Pennsylvania State University, asked a senior Japanese scientist visiting Penn State to find a young scientist from his homeland who might be recruited, a friend of a friend recommended Shirane. In 1956, Pepinsky sent Shirane to Brookhaven for a year, the same year that Clifford Shull was at Brookhaven. Shull was using a spectrometer at the BGRR to study the magnetic properties of single crystals. Shirane was impressed both by Shull and by the work he was doing, and decided that he could figure out no better way to advance his scientific career than to hang around Shull and pick up whatever he could. Besides Shirane, two other young associates were smart enough to have reached a similar decision: the already-mentioned Robert Nathans (another Pepinsky recruit from Penn State) and Tormod Riste from Norway.

> It was wonderful [Shirane recalled]. It happens only once in your lifetime, if you are lucky; you are young and fresh and just happen to meet by chance somebody who is really great. The only trouble was, Shull would rather work alone, though doesn't mind teaching. And there were three youngsters wanting to learn from him—me, Nathans, and Riste. That was hard. Only one or two people could really work at once with him, and it was his experiment and he had all the ideas



12.9 Drawing of triple-axis spectrometer at H-7.

and you couldn't learn anything until he showed up. I had a strategy. I tried to show up just as Cliff showed up at the reactor, and if he was alone I would go work with him. If one other guy was there, I would join them, so that's Cliff and two youngsters. But if the two other guys were already there, I wouldn't even try, I'd just go home. That strategy worked extremely well.

At the time, solid-state physics was regarded as an inferior cousin to high-energy physics at Brookhaven, and there were no opportunities for Shirane to remain at the lab. He left for the Westinghouse research lab outside Pittsburgh, one of the best industrial laboratories, which had its own materials-testing reactor where Shirane could conduct neutron-scattering experiments. Westinghouse had planned a research reactor that would vastly improve the neutron-scattering facilities, but abandoned the project early in 1962, in a widely publicized decision that made the front page of the *New York Times*. Physics department chair George Vineyard saw the article and called Shirane, telling him of Brookhaven's commitment to a new reactor, and successfully recruited him.

He arrived in 1963 and was assigned to Dienes's solid-state group, now much expanded and with a bright future. At the soon-to-be-completed HFBR, the group shared three facilities, H-4, H-7, and H-8. The facilities at each hole were slightly different: H-4 was optimized to support very heavy accessories, H-7 had a lower background, and H-8 had a slightly higher energy. While at the beginning H-7 had the only triple-axis spectrometer, a number of others were soon converted (fig. 12.9).

> No one guessed how big a field inelastic scattering was going to be [recalls Shirane]. It became a major field at Brookhaven for two reasons. The first was the HFBR, which gave a factor of ten increase in the neutron flux; the second was that a new type of crystal was invented around that time, a pyrolytic graphite monochrometer which can be shaped so that it not only reflects but also focuses the beam, which gives you another factor of ten. A factor of ten is a big factor. So all of a sudden you get a factor of a hundred.

Shirane keeps several of these crystals in his office. Each is a black and glossy lens-like surface an inch or so square. "Fantastic stuff," he said, lifting one carefully out of its box.

> This one costs $10,000. For what it does, $10,000 is *cheap*. The old way, we made three phonon measurements in a given year. With these, we can do thirty-five in that time in much more detail. It made a very difficult type of experiment easier, and by doing so opened up a new field.

Shirane achieved a level of skill at triple-axis spectrometry that has rarely been matched, and applied the skill to a number of issues involving inelastic scattering. He liked working in collaboration with one or two others, and his early collaborations with Robert Birgeneau proved especially fruitful. While Shirane was deeply focused on technique, Birgeneau was proficient in both theory and experiment and had an ability to identify model systems whose exploration would shed light not only on themselves but on related systems as well. One involved one-dimensional magnetic materials, whose magnetic ions are arranged in one long chain so that each ion interacts mainly with its fore and aft neighbors, and each chain is relatively insulated from the next. Theorists were extremely interested in such systems because a number of many-body problems that were hopelessly intractable in three dimensions were solvable in one, allowing the building and testing of models. In 1971 Shirane, Birgeneau, and others began exploring one example of such a system, tetramethyl ammonium manganese chloride or TMMC, using neutron scattering, taking one of the first steps in what would soon become a large field.[24] Shirane, Birgeneau, and Roger Cowley also studied two-dimensional systems, or planes whose atomic moments were separated so much that they acted like two-dimensional entities, which again interested theorists because of the prospect of model building.

Another important effort of Shirane's involved understanding certain kinds of phase transitions. Phase transitions, which include the melting of ice and the boiling of water, are familiar phenomena in nature. What happens when a phase change takes place—substances metamorphosing into different forms—is the sort of mysterious question that can inspire child-

like wonder even in seasoned scientists. One important phase transition occurs in magnetic materials in the transition from paramagnetic (i.e., disordered) to ordered (ferromagnetic or antiferromagnetic) states. How this process takes place was vital to understanding what makes one material ferromagnetic and another antiferromagnetic. Another kind of phase transition, structural phase transition, involves soft phonons; when a structural phase transition occurs, the phonons or lattice motions get slower and slower and finally "freeze out," with the atoms locking into a fixed position. The study of this process was a major focus of Shirane's work.

H-5 and H-6

Two solid-state holes, H-5 and H-6, were assigned to the chemistry department. Corliss and Hastings moved their collaboration to H-5, where they continued their work on the magnetic properties of solids, especially the behavior around the critical point. In their first years at the HFBR they sought out and explored magnetic systems that were experimentally challenging and of theoretical interest.[25] These studies led to the determination of the critical exponents associated with second-order phase transitions. H-6 was the domain of Walter Hamilton, whose introduction to Brookhaven was via a postdoctoral appointment with Corliss and Hastings and who within a short time became a legendary figure at the HFBR.

Hamilton, a crystallographer, was a commanding presence. Texas born and Oklahoma raised, he was over six feet tall and deaf in one ear with a booming voice. He would often crack up his audience at the beginning of talks by setting the microphone aside with the friendly words "Don't think I'll be needin' this!" He had endless energy, was an avid hiker, and whenever he attended a conference would seek out a nearby mountain to scale. He seemed in the thick of every discussion, every subject he touched seemed exciting, and he had an intuitive grasp of even remote aspects of his field. He attracted many younger assistants, whom he cared for well and looked after. One collaborator, in a remark typical of others, would later say that working with Hamilton was "the high point of my crystallographic life" (Kamb 1983, 337). When describing Hamilton, those who worked with him rarely fail to use the word "genius."

Hamilton grew up in Stillwater, Oklahoma, and in 1950, at the age of nineteen, graduated from the Oklahoma Agricultural and Mechanical College, where his father was a professor of mathematics. He did graduate studies at Caltech, a center for the structure determination of chemical compounds, where Linus Pauling worked. Hamilton earned a Ph.D. from Caltech in 1954 with a thesis concerned with electron diffraction, and spent the next year on an NSF postdoctoral fellowship working at Oxford with

Charles Coulson, an important chemical theorist. When Hamilton arrived at Brookhaven as a postdoc in 1955, he joined Corliss and Hastings at the BGRR, and quickly added neutron diffraction to his skills.

The field of crystallography involves handling massive amounts of numbers (it has provided a significant impetus for computer development), and its practitioners often rely heavily on tables, preferring techniques where one value can be inserted and another extracted without having to worry about what happens in between. Hamilton was different. Steeped in mathematics from his youth onward, he had a profound understanding of the mathematics and the physical properties on which the routine work in the field was based. He spent much of his career developing and improving methods for the computation and refinement of structures and the analysis of diffraction data. One of his papers on methods of crystallographic interpretation, written in 1964 and entitled "Significance Tests on the Crystallographic ***R*** Factor," was once at the very top of the Science Citation Index and is still cited today (Hamilton 1965). He was also versatile at computing. During construction of the chemistry building in 1965, the architects ran into difficulty figuring out how to arrange the bricks in the walls of the lecture hall, constructed in the form of a truncated cone; Hamilton promptly developed a computer program to solve the problem. He was also the driving force in the development of the single on-line computer that controlled the HFBR spectrometers. By then he was one of the most prominent scientists at the lab; in 1968, when Friedlander became the chemistry department chairman, he made Hamilton deputy chairman.

At the HFBR, Hamilton was assigned H-6, which had two ports. At one was a double-axis spectrometer used for elastic scattering; at the other was a triple-axis spectrometer, which would also be used for inelastic scattering. Both spectrometers had a chi circle, or device that could orient a crystal sample in any particular direction in space. If a crystallographic problem could be addressed using neutrons, Hamilton used one of these two instruments to tackle it. When a Caltech crystallographer approached Hamilton with a puzzle concerning the structure of high-pressure ice, Hamilton promptly joined him in a collaboration. For the first time, thanks to the HFBR's neutrons, they were able to detect the proton position in high-pressure ice (Kamb et al. 1971).[26] When a controversy broke out over whether a hydrogen atom that had a chemical bond to a metal had a normal bond distance or was somehow buried in the metal electrons, Hamilton and his coworkers decided the issue (La Placa et al. 1969).

One of his most important programs concerned amino acid structures. Hamilton foresaw the long-range need to get the best possible structures of the amino acids, including all the hydrogen positions, to construct a kind of directory to help determine the structure of proteins. Together with

Thomas Koetzle, a postdoc from Harvard, Hamilton mounted a major effort in determining amino acid structures at Brookhaven.

By the end of the 1960s, Hamilton was not yet forty but at the peak of his profession. He thrived amid all his projects, even seeming more youthful than ever. His appearance changed; while until 1968 he had short hair and looked like a marine, thereafter he let his hair grow long and unruly, adding a broad, friendly beard. He was one of the central figures in the profession: coeditor of *Acta Crystallographica,* a key member of many committees, and in 1969 the youngest president of the American Crystallographic Association. For many, he personified crystallography.

Early in 1971, one of a series of meetings called the Cold Spring Harbor Symposia on Quantitative Biology was held at Cold Spring Harbor, organized by James Watson around the subject of protein crystallography. Those who were on hand recall that something of a sea change took place among those who attended. It was not just that the great Nobel laureate James Watson (who, with Francis Crick, discovered the structure of DNA) was sanctioning protein crystallography as part of biology; there was a sense that it was a field about to explode. At the meeting, Hamilton was approached by Helen Berman, a postdoc from the Institute for Cancer Research in Philadelphia. Berman was one of a small group of crystallographers, which also included Edgar Meyer at Texas A&M, who had been agitating for a special database for proteins. Crystallography was not being deluged by protein structures; exactly seven were known. Also, crystallography, historically a close and well-organized discipline, already had an important database, the Cambridge Crystallographic Data Centre (CCDC), which had been founded in 1965 and was ably run by Olga Kennard, one of the movers in the field. But Berman and company were convinced that the number of known protein structures would soon skyrocket. They also felt that while the CCDC was suited for small molecules, databasing much larger structures like proteins would pose a special set of problems requiring larger data handling, larger amounts of storage, and a state-of-the-art computing facility. Berman knew the project required some politicking to get off the ground.

Hamilton was the right person to approach. He not only had the contacts and interest, but also the authority to compel people to pay attention to the subject and trust that it was important. He was already working on a grant proposal to establish a computer network for crystallography, the Crystallographic Computing Network (CRYSNET). Hamilton incorporated the Protein Data Bank (PDB) concept into the grant, which involved one central site, Brookhaven, and two remote sites, College Station, Texas (Meyer), and Philadelphia (Berman), and the grant was submitted in the fall of 1971. Berman began making the three-hour trip from Philadelphia

to Brookhaven about once a week to help set up the database. By then, the basic organization was effectively in place, with the seven known protein structures (Schoenborn's sperm whale myoglobin would become the first new protein added, the eighth overall, in May 1973). Hamilton and company began organizing to start public distribution in the summer of 1972, produced a standardized format, and developed a computer graphic system to store and disseminate the structures.[27] "The reason the Protein Data Bank ended up at Brookhaven had little to do with the obvious fact that neutrons were good for studying proteins," says Koetzle. "It had *something* to do with the fact that we had a state-of-the-art computing center, as well as a good computing infrastructure for the time. But mostly it was because we had Walter."[28]

In the fall of 1972, Hamilton went on an around-the-world trip that included talks and conferences in Denmark, England, and Japan, where he climbed a mountain in Hokkaido, but a last-minute meeting kept him from climbing Mount Fuji. When he returned, he admitted to not feeling well, though the last thing he had time for was a doctor. In November he went to Hawaii for a meeting, where he went snorkeling and did more hiking. On his return, his unprecedented complaints about his health were even worse. Koetzle and Berman nagged him to see a doctor, which he finally did in December. Tests revealed colon cancer, far advanced.

Hamilton was admitted to New York University Hospital a few days later, just before Christmas, and was operated on the following day. From then on, every piece of news about his condition was worse than the last. Still, Hamilton tried to disguise his condition from associates; phone callers could pick up little from the familiar booming voice he now had to affect. Only those who visited the hospital regularly, like Berman and Koetzle, knew how ill he really was. For them, it felt like a bad dream; neither could understand how anybody so full of volcanic energy just a few weeks before could possibly be so catastrophically sick. The sight was almost too much to bear, or even believe.

In mid-January 1973, the American Crystallographic Association held a meeting at the University of Florida in Gainesville. A few days before, from his hospital bed, Hamilton talked to Berman and Koetzle of going, and even made a poster that he said he wanted to present, while they spoke of seeing him there. By then he was too physically incapacitated to get out of bed. Berman stayed behind at the hospital, while Koetzle went to Gainesville. By the time Koetzle returned, Hamilton had lost his lucidity, and within a few days slipped into a coma. He died a few days later, on 23 January.

Meanwhile, the CRYSNET grant containing the Protein Data Bank had come through. Today, it is still at Brookhaven, a world-wide resource

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containing some five thousand entries. But in the second quarter-century of the lab's history, Hamilton's death would continue to exert an impact on the PDB.

H-9

H-9 was a fiasco.

When the HFBR's beam tubes were laid out in 1958, the designers envisioned a source for "cold" neutrons at H-9, those with lower energies and longer wavelengths than thermal neutrons. Cold neutrons are superior to thermal neutrons for studying low-energy collective motions, and relatively large-sized objects like biological tissues. Today, cold neutron sources are standard; it would be unthinkable to design a new research reactor without one. At the time, it just seemed like a good idea.[29]

In the early 1950s, Hughes and Palevsky had generated their beam of cold neutrons at the BGRR by simply filtering out the more energetic ones, but this produced an extremely weak beam. By the end of the decade, a far more effective technique was being developed that involved using a vat of liquid hydrogen to moderate the beam, which shifted the entire spectrum of energies downward, producing a much greater intensity. A neutron banging into a very cold (low-energy) proton tends to transfer more of its energy to that proton than a neutron hitting a room temperature proton vibrating with customary thermal energies.

Palevsky took nominal charge of the project but did nothing about it for a long time; his interests lay elsewhere and the engineering challenges involved were daunting. Liquid hydrogen, highly explosive in contact with air to begin with, becomes still more volatile in the presence of ozone, which is created when neutrons interact with air. Placing it near a reactor's hot core raised serious safety concerns. Sailor found the idea such "an appalling engineering task" that he suggested building an entirely separate reactor just as a cold neutron source.[30]

When Kevey had finished the US-1 spectrometer project, Palevsky asked him to design a liquid hydrogen refrigerator to go in the H-9 hole, telling him that the French had worked out all the details in connection with a cold neutron source at their Saclay reactor. Costs, Palevsky said, would be modest; about $60,000 to $80,000. Palevsky neglected to tell Kevey that the Saclay reactor had a much lower power than the HFBR, that the amount of liquid hydrogen they were using was tiny, and that the French had a notoriously cavalier attitude toward reactor safety. But Kevey gamely began to tackle the engineering, scaling up the Saclay design. It did not take him long to realize the project was almost hopeless, due to pump reliability and the huge inventory of liquid hydrogen that needed to be pumped all the way into the beam hole and then out again.

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Brookhaven National Laboratory

Meanwhile, Palevsky's group began building a slow chopper to station at the hole. The slow chopper was a one-of-a-kind device designed with a tricky arrangement of two rotors to increase accuracy. The device had problems almost from the beginning. Some were mechanical; in 1963, during a test, the rotor suddenly failed at 9,500 rpm, destroying the housing and much test equipment. Others were design flaws: when the rotor was rebuilt to handle the stresses, the machine never worked right. A defect in the motor-generator system adversely affected the speed control unit, and the two rotors were rarely able to stay in phase.[31]

Robert Nathans then entered the picture. Nathans was an experimental impresario; he was ambitious, dreamed big, and was never intimidated by the magnitude of a task, for he felt that if somehow he got into something over his head he could recruit enough talent to pull him out. Nathans decided that the scientific rewards of a cold neutron source were worth the effort and that he wanted control of the project. "I don't ask; that's not my style," says Nathans. "I just take, and see if anybody protests."

Palevsky was only too happy to be rid of it, and Nathans would soon feel like an advertisement for the old adage about having to be extremely careful what one wishes for lest the wish be granted. He made two assumptions that would come back to haunt him. He assumed that he had a commitment from Shutt's cryogenic group in the physics department, the only group on-site with experience in handling liquid hydrogen in large quantities, to help him out. And he assumed that since he was using the technology of a major cold neutron project at Harwell, where an extensive safety evaluation program had already been conducted, he did not need to arrange for safety evaluation studies.

Both assumptions fell through. The cryogenics group said it was too overloaded with its own work to spare any help. After major remonstrations, it finally agreed to lend one of its members to supervise construction of the cold neutron source's cryogenic system—but the person they sent was not up to the huge task now thrust upon him. And with reactor and liquid hydrogen safety concerns on the rise, the AEC declared that the Harwell studies would not suffice and that Brookhaven would have to carry out an entirely new set of safety studies. (An important contributing factor was an explosion, on 5 July 1965, at the forty-inch liquid bubble chamber at the Cambridge Electron Accelerator, killing one person and causing over million dollars in damage [Galison 1997, 353–62].) Few guidelines or precedents existed for cryogenic safety at reactors, and every time the cold neutron group made a move, the AEC officials, growing ever more safety conscious, found fault with it. The unexpected safety studies alone ate up most of the initial budget.

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Then, in 1968, Nathans abruptly walked away.

> I'm an opportunist, and there's only so much time in your creative career. The cold neutron source was a tougher technical problem than we thought, especially concerning the cooling system. And then the AEC started hounding us about safety, and asking us to file report after report. Remember, we were antibureaucracy and not used to that. We did what we thought was right, and Washington was Mars! I said, I ain't writing a goddamned report to anybody, I wanted to get on with the physics, and if I can't do that I'll go on to somebody else. I'm not going to spend the rest of my life worrying about a goddamned piece of apparatus. So I dumped the project. I don't piss into the wind; that's one of my talents. I got better things to do. And I know that what I did was bitterly resented by the others at the time.[32]

Nathans left the lab and accepted a position with the State University of New York at Stony Brook. The overwhelming, undersupported, and now leaderless project dragged on without him, its entire budget exhausted before construction had even begun. Meanwhile, the group working on the slow chopper discovered that its speed control problem could not be fixed, though they managed to nurse it along for about two years. But the effort was proving more trouble than it was worth; to operate the slow chopper required more technician time than was used up by all the other spectrometers combined. It was finally junked and a triple-axis spectrometer installed in its stead.

In 1972, physics department chairman Joseph Weneser was able to turn an unfortunate situation in another department into a stroke of good luck. At the time, Ralph Shutt was clashing with accelerator department chairman Fred Mills. The conflict was so unpleasant and destructive that lab officials transferred Shutt out of the accelerator department into the physics department. Weneser then coaxed Shutt and his group into working on the cold neutron source. In 1972, two members of the cryogenic group undertook a thorough review of the cryogenic system, and came up with a neat, well-calculated conceptual solution to the problem.[33] They suggested not circulating the liquid hydrogen in and out the reactor, but liquefying it inside the reactor—which at a stroke cut the amount of liquid hydrogen involved from 40 to 50 liters to 1½. Once this idea was accepted, the design and engineering of the cold neutron source could begin in earnest. Over the rest of the 1970s, Shutt applied his meticulous engineering skill to bring the project to completion, though on money quietly borrowed from other projects (mostly by paying those involved out of the operations budget), a practice sometimes, and not unsensibly, resorted to on occasion but rarely to this degree. The HFBR cold neutron source finally became operational in 1980.

AFTER THE HFBR was completed, Brookhaven's nuclear engineers talked about the possibility of another reactor. The next research reactor should have a significant increase in the available neutron flux. But Hendrie and others in the nuclear engineering department did not see any realizable way to improve the power density capability of an HFBR/HFIR-type reactor by a significant factor. One possibility was to build a "pulsed" device, which delivers its neutrons in extremely intense bursts, but with a small duty cycle.[34] If one generates a 100-millisecond-long pulse of neutrons every 10 seconds, it can be run at 10,000 times the power density of a steady-state machine and achieve a flux of about 10^{16}. (Arguments about the relative merits of pulsed versus continuous neutron facilities continue today.)

Hendrie became nominal head of the project, which dragged along for a few years on reactor physics money. At the time (the mid-1960s), about a dozen reactor prototypes were being worked on at various places in the U.S. Every national laboratory, and several major private laboratories with AEC contracts, had a power reactor concept, and some two or three: Oak Ridge was working on molten salt, Argonne on advanced boiling water, Atomics International in Los Angeles on sodium, while several laboratories toyed with schemes to use organic fluids with good heat transport properties. But at the AEC, Milt Shaw, the head of the division that developed new reactors, had become "not only the hardest working but also the most despised administrator in the history of the AEC" (Weart 1988, 306). Shaw loathed the loose research atmosphere in the national labs and promoted, with the accord of AEC administrators, the idea that all these more exotic types of reactor designs were draining money away from the AEC's more important long-range goal: fast breeder reactor development. Beginning around 1964, several other reactor development programs were terminated to consolidate resources to that goal, including Argonne's A^2R^2 (which was far along to the tune of $80 million; Holl 1997, 257–59, 268) and Brookhaven's much more preliminary work on plans for a pulsed reactor.[35] "It was a decision," Hendrie says, "that brought to an end a grand time for national laboratory reactor engineers." Hendrie had to fire about half of the scientific and professional staff of the engineering division—and had to be clever about how to support who was left, taking advantage of revisions in the Atomic Energy Act allowing outside funding:

> We had all kinds of schemes. We got into rock bolts as an aid to mine stabilization because it looked like the Bureau of Mines had a little money. We got into polymer-strengthened concrete as a structural material for big desalinization vessels because it looked like the Bureau of Reclamation had a little money. Jim Powell had a scheme for using a technique that had been developed for reading cloud chamber pictures for putting targets on the walls of mines and then you'd scan them with this device and if they moved it would tell you things were about

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to happen. Things like that kept us alive for about three years while we waited for something to happen to the pulsed machine idea.[36]

The pulsed reactor project did not crystallize. Most reactor users were not that enthusiastic, because the pulsed operation limited the types of experiments that could be done. "There was hardly a tumult of enthusiasm for it," says Hendrie. "And even if there had been, whether one could have gotten development work funding, let alone construction funding, in the climate of the times was another question."[37] Tired of the fight to keep the department alive, Hendrie accepted a job in Washington as the first chief engineer for the AEC's nuclear regulatory staff. He left in May 1972.

Meanwhile, the RILL (Reactor Institut Laue-Langevin) in Grenoble came on that year. Based on Chernick's design for the HFBR, with an undermoderated core and tangential beam tubes, its flux was about 30 percent higher than that of the HFBR. More significant, its accessories were also well designed: it had both cold and hot sources, as well as a new kind of high-speed chopper, developed in Germany, that relied on a magnetic bearing suspension system in which no mechanical contact took place between rotor and housing. And the Grenoble facility clearly had more resources than Brookhaven for developing and upgrading its instrumentation. Though the RILL experimental program was managed more formally and inflexibly than the HFBR, this was outweighed for many experimenters by the advantages. For Brookhaven's reactor experimenters, as for its accelerator experimenters, 1972 was a bittersweet year: both groups saw their utilities surpassed by a superior version with no significant new one on the horizon.

The HFBR would also exert a major influence on the lab's history during its second quarter-century. In 1994, a small electrical fire at the Tristan experiment stationed at H-2 resulted in minor contamination to the building and to several emergency personnel, but raised major concerns about the lab's experimental review process and about safety at the laboratory in general. And in 1997, at the beginning of Brookhaven's fiftieth anniversary year, the announcement of the discovery of a small leak of tritium-containing water from the spent fuel pool of the reactor helped trigger a dramatic chain of events, including the cancellation by the Department of Energy of AUI's contract to manage the lab, the introduction of legislation in Congress to prevent the reactor's restart, and the search for a new contractor—in the process signaling what is no doubt a turning point in U.S. science policy.