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Physics in Perspective

The National Synchrotron Light Source, Part I: Bright Idea

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The National Synchrotron Light Source (NSLS) was the first facility designed and built specifically for producing and exploiting synchrotron radiation. It was also the first facility to incorporate the Chasman-Green lattice for maximizing brightness. The NSLS was a $24-million project conceived about 1970. It was officially proposed in 1976, and its groundbreaking took place in 1978. Its construction was a key episode in Brookhaven's history, in the transition of synchrotron radiation from a novelty to a commodity, and in the transition of synchrotron-radiation scientists from parasitic to autonomous researchers. The way the machine was conceived, designed, promoted, and constructed illustrates much both about the tensions and tradeoffs faced by large scientific projects in the age of big science. In this article, the first of two parts, I cover the conception, design, and planning of the NSLS up to its groundbreaking. Part II, covering its construction, will appear in the next issue.

Key words: Kenneth Green; Renata Chasman; Martin Blume; Sam Krinsky; Arie van Steenbergen; Richard Watson; Brookhaven National Laboratory; National Synchrotron Light Source; synchrotron radiation; Chasman-Green lattice; accelerators.

Introduction

In 1972, an interdisciplinary coalition of scientists at Brookhaven National Laboratory (BNL) on Long Island, New York, began planning a synchrotron-radiation facility, eventually to be called the National Synchrotron Light Source (NSLS). The NSLS was the first facility designed and built specifically for producing and exploiting synchrotron radiation, and its principal design feature, invented at Brookhaven, would be incorporated into successive generations of synchrotron-radiation facilities. Although in many respects the NSLS, as a materials-science facility, complemented research at Brookhaven's High Flux Beam Reactor (HFBR), it would significantly transform the lab's research culture in unanticipated ways. These ways included strengthening the lab's solid-state physics and materials-science programs, fostering interdisciplinary research, and creating new kinds of interactions among laboratory, university, and industrial researchers.[1] A $24-million project conceived about 1970, officially proposed in 1976, and breaking ground in 1978, the NSLS was a major episode in Brookhaven's history. It was a key element, too, in the story of the transition of synchrotron radiation

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from a novelty to a commodity, and of synchrotron-radiation researchers from "parasitic" to autonomous researchers.

Yet planning and constructing the NSLS was an arduous, even traumatic experience for Brookhaven, for a host of reasons. It was promoted by an interdisciplinary crew of solid-state scientists who had no experience in conceiving and planning such a large facility. As a solid-state enterprise, it had low priority at a high-energy physics lab. Its priority fell still lower because Brookhaven was just then in the process of building a forefront high-energy accelerator, deemed to be essential to the lab's future, whose scale was an order of magnitude larger in cost and size than the NSLS. Its original feature – high brightness – unexpectedly posed extraordinary demands on its construction. The two principal designers of the NSLS died before groundbreaking. It was recognized at the outset to be hopelessly underfunded. And supporting its large and diverse array of experimenters – much larger than at reactors or high-energy accelerators – required transformations in the organization of users at federal laboratories.

As a result of these problems, the planning and construction process of the NSLS exhibits in a dramatic and instructive form – writ large – many tensions that now accompany the process of building a modern forefront scientific facility.

Synchrotron Radiation

Synchrotron radiation began life as an unexpected and unpleasant nuisance. Builders of particle accelerators in the 1930s and 1940s knew that, according to well-understood laws of classical electrodynamics, charged particles that accelerate – change their direction, velocity, or both – radiate energy, and tangentially to their trajectory. This is how electrons moving in an antenna create radio waves, for instance. Initially, accelerator builders assumed that, for their purposes, the radiation losses of the circularly moving beams of the machines would be negligible. They were, but purposes can change swiftly in science. In 1944, two Russians, Isaak Pomeranchuk and Dimitri Iwanenko, sent a brief, three-paragraph letter to the *Physical Review* declaring that the energy loss would rise sharply with the size of particle accelerators, imposing, they wrote, "a limitation for maximal energy attainable" by such machines.[2] This was bad news, for the basic method of particle acceleration involved using magnetic fields to bend particles in orbits while slowly boosting their energies. At that time, scientists at the General Electric (GE) Research Laboratory in Schenectady, New York – then a world-class laboratory whose program included accelerator and solid-state research, and whose staff included the future Brookhaven accelerator physicist John Blewett – were building large electron accelerators as X-ray sources. Dismayed and a bit skeptical, the GE scientists set out to see whether the bad news were true.

It was.[3] Just as the Russians had predicted, the electrons were radiating energy tangentially to their orbits the way a spinning wheel throws off mud. In 1947, the GE scientists installed a mirror in the latest of their machines, a synchrotron, and observed a small but bright speck of light that represented the visible portion of the energy (figure 1). They were able to do this because unlike most synchrotrons built elsewhere the GE device had a glass vacuum chamber. The GE scientists discovered that theorist Julian Schwinger was examining this phenomenon, and although he had a preprint of

Fig. 1. General Electric Scientists inspecting the vacuum chamber of the 70-MeV synchrotron in which synchrotron radiation was first observed. *Left to right*: Robert Langmuir, Frank Elder, Toly Gurewitsch, Ernest Charlton, and Herb Pollock.

his work in 1945, and spoke about it at a meeting of the American Physical Society in 1946, he did not publish an article on it until 1949.[4] The new phenomenon was baptized "synchrotron radiation" – which is a curious historical accident, for it has nothing intrinsically to do with synchrotrons but only with changes in velocity in a magnetic field, and thus occurs in any accelerator. But so the name was born. Because synchrotron radiation was one of the few new phenomena uncovered by science visible to the naked eye, the discovery caused a small stir in the scientific community and at GE; those who paid calls to the machine included six Nobel prizewinners and the entire GE Board of Directors, as well as a reportedly unimpressed GE public relations servant named Ronald Reagan.[5] The novelty of the phenomenon's visibility was small consolation for the dismal prospect of diminishing returns for accelerators; at some point any extra energy added to the electrons would be promptly radiated away.

But the accursed nuisances of science have a way of turning into valuable tools. A few years later, in 1953, two Cornell scientists, Diran Tomboulain and Paul Hartman, constructed a primitive beam line at an accelerator there to carry off the synchrotron radiation and measure properties such as its energy at the angles at which it was thrown off.[6] The next year, 1954, Tomboulain and Robert W. Johnston used synchrotron radiation experimentally, to obtain an absorption spectrum of beryllium. They noted, "The possibility of utilizing this remarkable source as a tool in absorption spectroscopy shows promise." [7] In 1960, two National Bureau of Standards (NBS) scientists, Robert P. Madden and Keith Codling, demonstrated further applications of synchrotron radiation at a 180-MeV (million-electron-volt) synchrotron (later converted to an electron storage ring, the Synchrotron Ultraviolet Radiation Facility, or SURF) by creating synchrotron radiation with which they studied the excitation spectra in gases.[8] The number and diversity of applications grew, thanks to two distinctive features of the radiation: first, it consisted of a continuous spectrum of light, making it useful for studying things like absorption spectra; second, it could be finely tuned by instruments called monochrometers, making it useful for diffraction studies of crystals and proteins as well as for angiography.

By the mid-1960s, enough new physics applications had been uncovered that when funding dried up for a proposed 12.5-GeV (giga-electron-volt) proton storage ring at the Midwestern Universities Research Association (MURA) laboratory at the University of Wisconsin, scientists there decided to convert its prototype into a 240-MeV electron storage ring for synchrotron radiation; when funding nearly fell through a second time, the seemingly ephemeral machine was baptized "Tantalus." The MURA laboratory head was Frederick E. Mills, and the Tantalus project director was Ed Rowe. Tantalus, stitched together mainly from used parts, came on line in 1968.[9] Though not designed as such, Tantalus was the first machine to function exclusively as a synchrotron-radiation source. It emitted ultraviolet radiation in two beam ports, later expanded to ten. From the beginning, IBM and AT&T scientists participated, performing research into the surface structures of materials. University and National Science Foundation (NSF) regulations restricting proprietary research was not yet a problem; "nobody could think of any proprietary research to do yet," Rowe recalled.[10]

Scientists at the Stanford Linear Accelerator Center (SLAC) proposed a Stanford Synchrotron Radiation Project (SSRP) as an addition to the 2.5-GeV Stanford Positron Electron Accelerating Ring (SPEAR) storage ring being used by the high-energy physicists.[11] And at Harvard University and the Massachusetts Institute of Technology (MIT), scientists at the Cambridge Electron Accelerator (CEA), which the Atomic Energy Commission (AEC) had scheduled to be shut down as a high-energy physics facility, conceived an ambitious plan to convert that accelerator into an X-ray region synchrotron-radiation source supported by NSF's materials-science budget, and succeeded in carrying out a few highly successful experiments[12] Meanwhile, synchrotron radiation was also being studied at about two dozen places abroad – almost all at facilities that, like Tantalus, had been built for other purposes but had begged, borrowed, or transformed their equipment. "We, synchrotron radiation people, were pirates," recalled an early pioneer.[13]

But by 1972 the thinking about the applications of, and facilities for synchrotron radiation was still underdeveloped. In the mid-1960s, the materials section of the National Research Council (NRC) charged a panel to brainstorm the uses of synchrotron radiation in condensed-matter science: BNL's director George Vineyard was one of its members. But the results were disappointing – with one exception, they came up with extensions of current work, and their report did not bring out the possibility of new applications in a vast array of disciplines. The report was never published.[14]

Birth of the NSLS Idea

At Brookhaven, a small crew of scientists sought to end the piratical life when they began to push the idea of a large facility designed from scratch as a synchrotron-radiation facility.

The idea emerged in the summer of 1970 during the course of a series of informal lunch gatherings of chemists and solid-state physicists whose members included Fred Mills, who by now had become chairman of Brookhaven's Accelerator Department. Mills had been brought from the University of Wisconsin to Brookhaven to replace G. Kenneth Green (figure 2) as head of the Accelerator Department when the Alternating Gradient Synchrotron (AGS) upgrade, whose construction had dragged on since 1966, ran into trouble. Mills, too, would soon displease lab administrators, for temperamentally he was more university professor than project head, and lunched with physicists and chemists rather than with engineers and administrators. He was highly competent, but not the right person to extricate the laboratory from its difficult position: If its principal accelerator builders John Blewett and Ken Green did not know how to easily remedy the problems of the then-ailing AGS, it was a sure bet that an outsider and research director like Fred Mills would not, either. But Mills was imaginative and thought seriously about long-term research interests. Like Green, he was more concerned with what the scientists needed than what they wanted, and as a good scientific administrator often did their thinking for them.

The lunch meetings of the chemists and solid-state physicists were informal, consisting of "whomever happened to be going to lunch and arrived at the same time and there was still space at the table," as one member once put it. One regular was physicist Chalmers Frazier, Raymond Pepinsky's former graduate student at the University of Pennsylvania, who by then had joined Brookhaven and become head of the solid-state physics group in its Physics Department. Another was chemist Morris Perlman. Perlman was born in Detroit in 1916, received his Ph.D. degree in physical chemistry at the University of California, Berkeley, in 1940, and had worked on the Manhattan Project at Los Alamos before becoming one of a group of nuclear chemists at GE's Schenectady laboratory (whose other members included Gerhard Friedlander who later went to Brookhaven). Perlman joined Brookhaven's Chemistry Department in 1949, and was interested in photoemission, the emission of electrons when light of various wavelengths strikes the surface of a material, for instance gold in various compounds. By measuring the energy of the ejected or "photoemitted" electrons, chemists can learn a great deal about the similarities and differences of such compounds. The standard "front end" for a photoemission experiment at that time, however, was an X-ray tube that usually produced only a single band of light, from which particular wavelengths could not be selected. Perlman became fascinated by the promise of synchrotron radiation for chemical research.

Yet another of Mills's regular table companions was Richard Watson, a solid-state physicist who had worked under John C. Slater at MIT and then thanks to an NSF post-

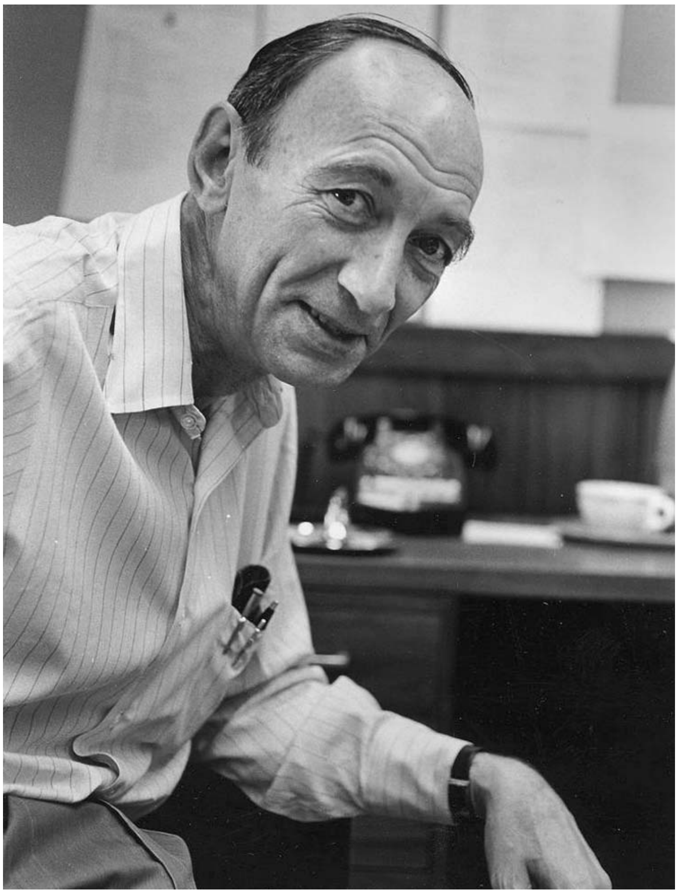

Fig. 2. G. Kenneth Green, one of BNL's premier accelerator builders, Head of its Accelerator Department from 1960 to 1970, and codesigner of the Chasman-Green lattice of the NSIS. *Credit*: Brookhaven National Laboratory.

doctoral fellowship came to Brookhaven's solid-state group from another of its training grounds, the Atomic Energy Research Establishment at Harwell, England. There he was exposed to John Hubbard and Walter Marshall, who tutored several future Brookhaven solid-state physicists – Watson, Martin Blume, and Victor Emery – not only in science but in thinking like a scientist. Hubbard taught them the importance of looking at things in a deep analytical way, thinking them through from first principles. Marshall taught them the importance of looking at things intuitively, of being able to "wiggle your fingers" to show what makes something work. He also provided them with a model of how to solve a problem by thinking out loud, a rare talent.

> In order to do that [Watson recalled], you have to be prepared to make mistakes, and you have to be prepared to cut other people off at the knees if they try to take

> advantage. Walter was very good at that. Walter was a lesson in thinking out loud, and in how you can move subjects forward: in his case, it involved neutron scattering and magnetism.[15]

In 1965, as the High Flux Beam Reactor (HFBR) was coming on, Watson joined Brookhaven from Bell Labs, and his interests in electronic structure meshed with Perlman's. "He and I greatly enjoyed collaborating by shouting at each other," Watson recalled. "He and I enjoyed a loud relationship." By the time of the lunch meetings, Watson was collaborating with Perlman. Their first paper, with chemist Jerry Hudis, explained a puzzling result in the photoemission data from gold.[16]

Mills's presence at the lunch meetings meant that discussion would be less "a bull session about so-and-so's current experiment or calculation" and more a discussion of "what you could or in principle could not do" with present and future instrumentation – especially synchrotron radiation sources. Everyone recognized instantly that they would be a godsend not only to scientists doing photoemission, but to many other kinds of physicists, chemists, biologists, and medical doctors. "We realized we had been waiting for this tool," Watson said. That was not all.

> Most of the experimenters that were using [the early synchrotron-radiation] facilities were busy getting results that were better than they could otherwise get. So they were not sitting down and saying, "Gee, what could we do if we had an ideally designed facility?" They were living for the next six months, not for the next five years.[17]

But Mills temperamentally thought five years ahead. He began walking around the lab, looking at buildings and the spaces between them, mentally estimating how to fit in a synchrotron-radiation source. Brookhaven, everyone agreed, would be an ideal place for one, given its interdisciplinary resources, superb accelerator building, and history of supporting facilities for outside users. The first memo outlining this appeared in September 1971.[18]

The group members recruited other potential users of such a device, from the physics, chemistry, biology, medical, and engineering departments, and formed a local study committee. The group's first important action was to sponsor an interdisciplinary symposium, in September 1972 (figure 3), to explore "the usefulness of an electron synchrotron or storage ring which would be used exclusively as a photon source in the ultraviolet [UV] and X-ray wavelength regions."[19] Participants included Brookhaven researchers as well as Peter Eisenberger of Bell Labs and Dean Eastman of IBM. Though they discussed facilities that could be converted from current machines, including Tantalus II and the CEA, their principal focus was on the prospect of something built *ab initio*. The participants estimated that several hundred researchers in the Northeast alone would be interested in UV and X-ray photons, in a variety of fields and at a variety of wavelengths. For instance: medical researchers needed wavelengths as short as 0.2 Angström (A) for X-ray studies; researchers interested in structural diffraction needed wavelengths between 1 and 5 A; and scientists interested in optical and photochemical research required long wavelengths, above 5–10 A, to create the kinds of excitations they were studying. One surprising promise was the use of synchrotron

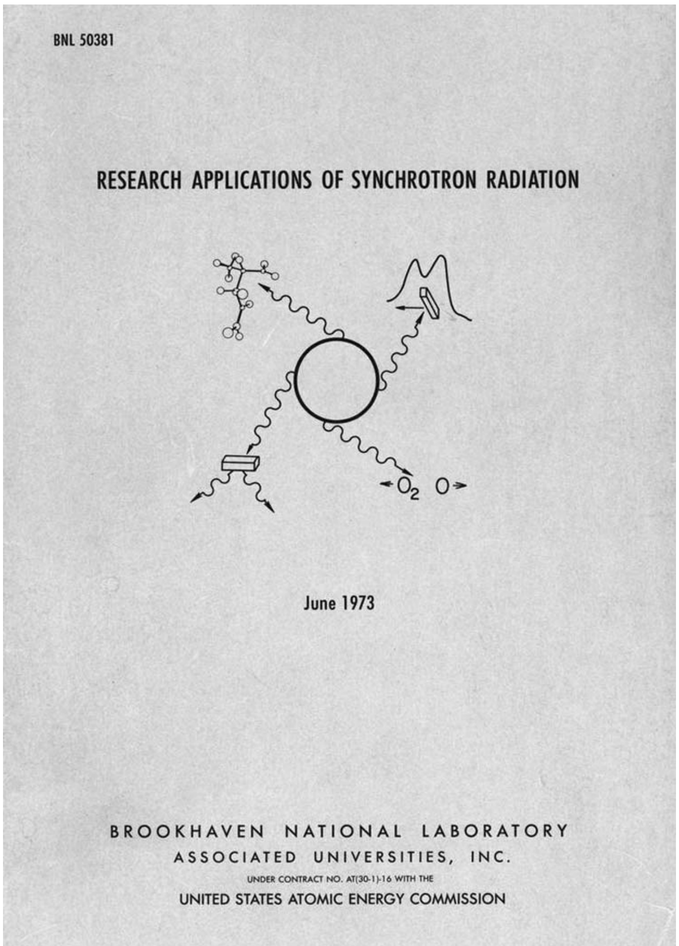

Fig. 3. Cover page of the proceedings of the 1972 BNL symposium to explore the potential of a dedicated synchrotron-radiation machine. *Credit*: Brookhaven National Laboratory.

radiation to determine protein structures, which was fast becoming an important area of science; this fit in well with Brookhaven's own recently established Protein Data Bank.

The light given off by a synchrotron-radiation source is typically illustrated by a chart that plots the wavelength of the light given off against the number of photons emitted per second (figure 4). Mapped this way, the curve looks like a whale's head, with the photon intensity slowly rising as the wavelength decreases or photon energy increases up to a peak or "cut-off" energy – the whale's forehead – whereupon it abruptly tapers off. The greater the energy of the machine, the farther into the X-ray region it could go, though it would also have ultraviolet radiation. Deliberations

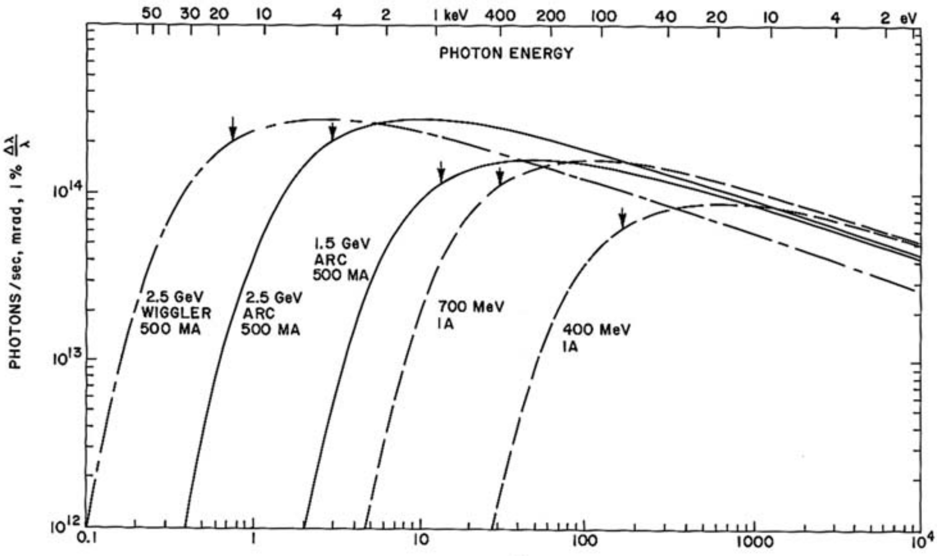

Fig. 4. The light given off by a synchrotron-radiation source. The wavelength of the light is plotted against the number of photons emitted per second. As the wavelength decreases (or the frequency or photon energy increases), the number of photons emitted per second (or photon intensity) increases slowly and then abruptly falls to zero. *Credit*: Brookhaven National Laboratory.

focused on a "mixed-mode" ring, able to supply a range of wavelengths. It would normally operate at 1.4 GeV, at which it produced a spectrum of wavelengths, from which specific wavelengths could be filtered out. Longer wavelengths could be obtained by running the machine at lower energy. Intense bursts of shorter-wavelength light could be obtained by insertion devices – specially designed instruments placed in straight sections that induced the beam to give off light in particular ways. The kinds under serious development in 1972 were known as "wigglers" but also included "undulators."

When electrons bend around a circle, they spread out the radiation they emit throughout the entire plane – again, like mud from a wheel. A wiggler is a device to concentrate this emission. It is installed in a straight section of the accelerator, and consists of a series of high-field magnets that alternate positive and negative fields, making the electrons "wiggle" when they flow between the magnet poles, going through a sinusoidal trajectory, like ripples on the surface of a pond, whose trajectory never deviates far from the forward direction. As they wiggle they emit radiation tangentially to their trajectory in a cone in the forward direction, and the bursts of light from each wave add up at its crest. The result is an intense beam emitted as the light goes up and down in a transverse plane, like a cone riding the wave. The resulting flashes resemble those of a lighthouse beacon as the cone passes up and down. Instead of a whale's head, the radiation is emitted in a kind of spike.

Within the next few years, other types of insertion devices were developed, including helical wigglers (soon to be called free-electron lasers) and undulators. Helical wigglers rotate the emitted radiation circularly. Undulators are "gentle" wigglers, so to speak, and make the emitted radiation oscillate at just the right frequency, like the electrons in an antenna, so that the electrons in effect "see" their path through the undulator not as a series of up and down wiggles but as a single episode, meaning that the emitted radiation coheres. That is, while wigglers make the electrons move up and down, emitting synchrotron radiation at the peaks in the forward direction, with its intensity from successive peaks simply adding up, in undulators the radiation emitted at successive peaks interferes, producing instead of a smooth addition of wavelengths a series of sharp and intense spikes at specific and controllable wavelengths. Insertion devices proved to be extremely important in developing applications of synchrotron radiation, and were being pioneered at the SSRP, concurrently with many theoretical developments at Brookhaven.

At the end of the 1972 meeting, BNL Physics Department Chair Joseph Weneser expressed the hopes of the solid-state physics group when he noted how much of a leap past X-ray tubes a dedicated synchrotron-radiation source would be for researchers. "[Y]ears of measurement are changed into days," he said. "The accumulation of the number of related experiments to really explore ideas would promise the compression of the efforts of a lifetime to an altogether different tempo. It seemed to me to be the stuff of which breakthroughs are made."[20]

Shortly after the conference, Weneser summoned Watson and Perlman into his office. He told them that securing a synchrotron-radiation source for Brookhaven was not the usual solid- state project – it would be a major undertaking, and require new organizational structures and political efforts. He reminded Watson and Perlman of the competition both inside and outside the lab. The competition inside the lab was with a neutron source to study radiation damage that had been proposed by some of the reactor engineers. The competition outside the lab included the CEA, Tantalus, and the SSRP – the latter two of which were doing excellent work and planning upgrades, Tantalus II and SSRP-II – and the U.S. Energy Research and Development Administration (ERDA) would be faced with the question of whether it was wiser to spend funds to improve fine existing facilities or to build a new one – that is, to support a set of productive pirates or to build something new from scratch. Now that Mills had decided to step down as head of the Accelerator Department, Weneser told Watson and Perlman that it was up to them. If they decided to go ahead and plan the project, the lab would back them; if they decided not to, the project would die. Giving the increased lab commitment to ISABELLE,[21] as well as rising inflation, the time to start was now.

Developing the NSLS Concept

Their efforts, however, were hampered for over a year owing to competition with the CEA conversion: It would be awkward for Brookhaven to compete for funds with a nearby facility, already built, one run by two of the lab's sponsoring universities, Harvard and MIT.

Moreover, to justify their machine, the solid-state physicist and chemist faced two challenges that were unknown to their high-energy physics colleagues. One was to make clear to funding agencies the value of building from scratch a machine that was not a standard electron ring but optimized to produce synchrotron radiation. This was unnecessary for high-energy physicists, for accelerators were already optimized for their needs. Funding agencies would be sorely tempted to stay with the successful piratical approach, and would need to see the value of an optimized machine. The second challenge was to stimulate enough interest in the scientific community to justify building a machine for that purpose. This, too, was unnecessary for high-energy physicists, for in their field the justification for ever-more energetic machines was already accepted, and a set of review committees had been formed with that assumption in mind.

To meet the first challenge, regarding an optimal design, Watson and Perlman sought help from the Accelerator Department. Ordinarily, finding help from this quarter would have been difficult, given that the department was in the process of designing ISABELLE. But the turmoil within that department, so harmful in the long run to that collider, was a boon for Watson and Perlman's project, for they found plenty of talent among the department's alienated members.

One was Renata ("Rena") Wiener Chasman (figure 5), one of the few female accelerator physicists at the time. Chasman was born in Berlin in 1932. After the Nazis came to power, her family moved first to Holland, then Sweden, where she spent her early youth, sometimes traveling the three miles to school on skiis. She studied nuclear physics at the Hebrew University of Jerusalem (Ph.D. 1959), then moved to Columbia University where she was a research associate working with C.S. Wu. In 1962, Chasman and her husband Chellis moved to Yale University, and a year later, arrived at Brookhaven – Renata in the Sigma Center (where nuclear cross-section data was collected and collated), Chellis in the Physics Department. But Renata found her work in the Sigma Center less than challenging, and in 1965 switched to a new field, accelerator physics, in which she had a stellar, too-brief career. She transferred to the Accelerator Department, where she was mentored by Ernest Courant, one of the "fathers" of the strong-focusing synchrotron and an ISABELLE designer, who worked with her on various magnet arrangements for accelerators – called lattices – and among other things showed her how to use the prevailing computer program for that purpose, developed at Berkeley, known as SYNCH. She grew familiar with the performance of high-intensity beams in linear accelerators, became the principal theorist of the group constructing the 200-MeV Linac, the centerpiece of the AGS upgrade, and was one of the early accelerator physicists to work on the lattice in connection with the ISABELLE project – but was also one of the early ones to become disenchanted with that project, one reason being that she never felt she belonged in its tightly knit male-dominated environment. When Mark Barton raised the possibility of working on a synchrotron-radiation source, she leapt at the chance.

Chasman in turn attracted John Blewett's partial attention, and Ken Green's full attention, to the project. Both were embittered by their experiences with ISABELLE, and were the first of what, years later, would be called "ISABELLE refugees" to participate in the synchrotron-radiation project. Blewett was nearing retirement, and his contribution was mainly restricted to helping to prepare documents. But Green, who

Fig. 5. Renata (Rena) Wiener Chasman, codesigner of the Chasman-Green lattice of the NSLS. *Credit*: Brookhaven National Laboratory.

had experience with photon optics, was thrilled and threw himself full-time into the task. Someone for whom a day without a challenge was a day wasted, he began to examine the special character of synchrotron radiation, trying to come up with what would make a synchrotron-radiation accelerator different from all others.

Chasman and Green began collaborating on ideas for the lattice, and their work was the most creative that either would do in their careers. Although Green was a nonstop talker, he also could be an excellent listener, and consulted experimenters such as Peter Eisenberger and Dean Eastman to see what special properties an accelerator might have that would facilitate their research. Green soon realized that, while high-energy physicists were interested in maximizing energy and luminosity in their accelerators, synchrotron-radiation physicists should be interested in maximizing brightness, whose three components are intensity, spread in angle, and volume.

In common language, brightness means the amount of light emanating from a certain source. Accelerator physicists give it a more precise definition: the number of photons emanating from a particular volume in phase space (which includes the volume and angular spread of the photons emitted by the particles in the beam) per bandwidth (frequency spread of the particles). This property thus includes the intensity, angle, and source volume of the photons, or what synchrotron-radiation users would be interested in. Once synchrotron radiation is generated, its brightness can be cut down but never improved. While a beam's size and angular spread could be improved by installing slits, and its energy could be increased by using wigglers, these transformations come at the cost of photons. A beam's size could be focused to a point with quadrupoles – but at the cost of the angular spread, and thus the brightness. No trick could cause a source of radiation to gain in brightness. While some synchrotron-radiation experiments do not require high brightness, many do and it never hurts.

Chasman and Green thus seized on this as the critical property, and their "design philosophy" for the accelerator was: optimize brightness. To do so, they had to keep the beam size small, particularly during bends, which was not the practice in most accelerator lattices.

A beam of electrons going round and round in an accelerator is like a beam of photons passing through an array of prisms and lenses. The passage of a beam of charged particles through accelerator magnets can be compared to a beam of light passing through prisms and lenses. Prisms bend a shaft of light, while lenses can focus it. In bending the shaft of light, however, a prism causes it to fan out into a spectrum of wavelengths, because the high-energy components bend more than the low-energy ones, though other prisms can be used to gather the spectrum back together. Dipole magnets cause a beam of particles to bend, and in doing so spread them out into a wide and flat beam depending on their energies. Such so-called "chromatic" bends were well-known to high-energy accelerator physicists, but did not matter in their accelerators. The particles in a high-energy accelerator can bend and oscillate about what is called the "design orbit" – the one that they ideally would want the particles to travel – without following it exactly. The beam emittance,* or deviation from this design orbit in terms of spatial and angular spread, matters little as long as the particles are reined in by the dipoles and quadrupoles, and eventually focused to a small spot at the point of collision.

Chromatic bends do, however, affect the beam's brightness. Chasman and Green therefore designed an innovative lattice, which besides dipoles and quadrupoles used an arrangement of sextupoles (which in optics are analogous to lenses that correct chromatic aberration) to produce "chromatic" bends that minimized emittance, keeping the beam small in phase space all the way around. But sextupoles make the equations of motion nonlinear, giving rise to certain problems involving space charge – effects of squeezing like charged particles into tight quarters – and orbital instabilities, or oscillations of the beam about the design orbit. One particular space-charge phenomenon, known as the "Touschek effect," arises because charged particles in a bunch

* Emittance refers to the size of the beam. It is directly proportional to the area of phase space of the beam, phase space being an area in a space where one coordinate is a distance and the other a momentum.

repel each other, creating "intrabeam scattering" that pushes the beam apart and transfers transverse momentum to longitudinal momentum, making the bunches spread out longitudinally until they exceed the "radiofrequency (rf) bucket," or space in the accelerating cavity in which they are accelerated, limiting the lifetime of the stored beam. Chasman devised ways to counteract or limit these effects as much as possible. She also worked on ways to compensate for the insertion devices. This was critical, for as the electrons emit radiation – and the whole purpose of the facility is to make them do so – they also lose energy, which affects their orbit.

The resulting magnet array optimized the brightness of the machine, making it an order of magnitude greater than that of existing accelerators. Green saw the experimental benefits of brightness before many experimenters, some of whom, worried about the time structure on their experiments, initially resisted his focus. What is now known as the *Chasman-Green lattice* was incorporated into the design of successive generations of synchrotron facilities.

While Chasman and Green worked out the principles of the lattice, Watson and Perlman worked on their second challenge: drumming up support among solid-state peers for the big facility. This is not something that high-energy physicists had to do for new accelerators. One did not need to convince high-energy physicists of the rationale for building more energetic accelerators, nor even the agencies that funded them. "The problem we faced was getting our peers to think past the next six months," Watson recalled.

Watson and Perlman set about touting the potential of synchrotron radiation as a research tool, and the virtues of a source that optimized it, in several publications and at various conferences. In July 1974, for instance, they published an article in *Physics Today* entitled "Synchrotron radiation–light fantastic."[22] The subtitle: "The many experiments that use the synchrotron radiation from existing accelerators prompt serious consideration of an advanced, specially designed, radiation source." They published a subsequent article in *Science* called "Seeing with a New Light: Synchrotron Radiation."[23]

Still, early progress toward a proposal was slow and somewhat disorganized. One reason was that the attention of the laboratory's high-energy and (with the exception of Chasman, Green, and Blewett) accelerator physicists – the single most influential group of scientists in the lab – was caught up in ISABELLE, then in its early planning stages.[24] A second was that the solid-state scientists had little experience putting together a proposal for a big facility, which took them into unfamiliar areas, such as large-scale construction and political promotion. And a third was the growing budget problem, including the fallout from the Mansfield Amendment, which though operative only for a year, forced the Department of Defense (DOD) to cease supporting many long-term projects and facilities – including Tantalus – which then had to appeal to the NSF for help, straining that agency's budget.

The NSLS Proposal

Then three key developments occurred in quick succession that suddenly gave the project a boost. Two involved competitors, the third a reorganization of Brookhaven's Physics Department.

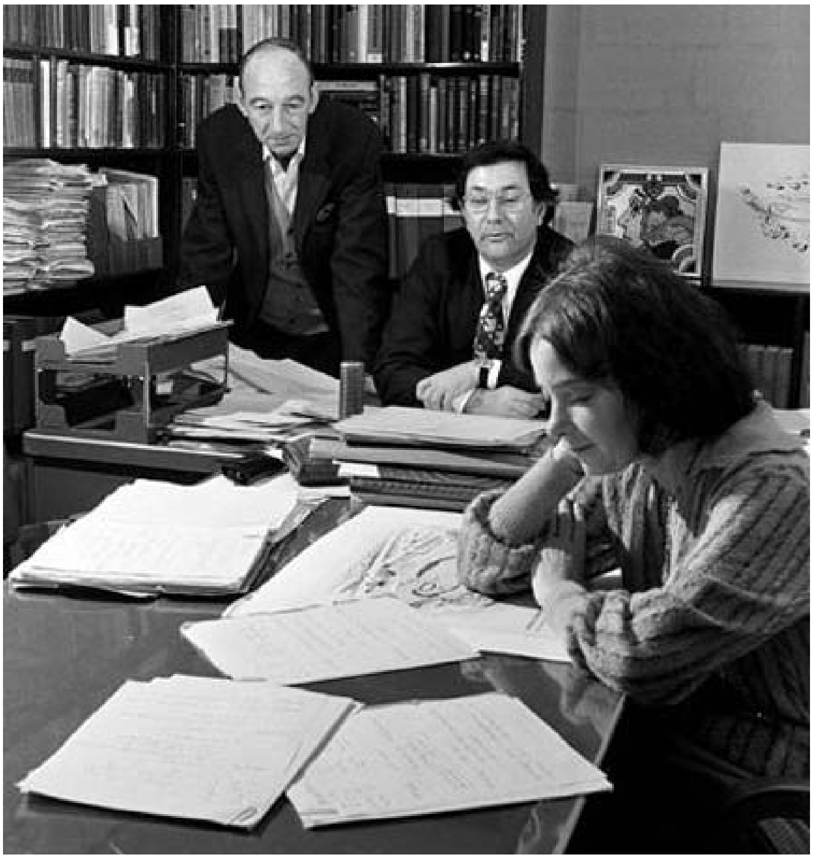

Fig. 6. *Left to right*: G. Kenneth Green, Martin Blume, and Renata (Rena) Wiener Chasman. *Credit*: Brookhaven National Laboratory.

The first involved the CEA. By 1974 it was clear that the CEA conversion would not materialize, owing in part to the considerable amount of money ($1 million per year) and personnel it would have taken to operate the machine. The parasites were not yet in a position to take over the host. This suddenly eliminated the awkward competition with nearby peers. The second involved the SSRP. In November 1974 the J/ψ – or rather the ψ part of it, with the J part discovered at Brookhaven's AGS – was discovered at SPEAR. The high-energy physicists at that machine started running it at the energy needed to make the particle and to seek its resonances – at about 1.6 GeV, an energy at which X rays could not be created. This suddenly exhibited the vulnerability of solid-state researchers at parasitic facilities, and demonstrated the need for a dedicated machine.

The third was a reorganization of Brookhaven's Physics Department. The new Deputy Chairman under Nicholas Samios was solid-state theorist Martin Blume (figure 6). And Blume took charge of organizing the proposal.

Blume was born in New York City in 1932. He had some acquaintance with synchrotron radiation from his work on his undergraduate thesis at Princeton – in the course of which he had read Schwinger's article – before turning to neutron scattering as a graduate student at Harvard, earning his Ph.D. degree in 1959. He and his wife Sheila then spent a year in Japan, each having managed to get Fulbrights there, then spent two years at Harwell, where Blume worked with Marshall and Hubbard.

Blume arrived at Brookhaven in 1962, and on his first day dropped in on the physics colloquium, which happened to be an announcement of the discovery of two kinds of neutrinos, one of the most significant discoveries in physics of the post-War era, which eventually was rewarded with a Nobel prize. "I thought this seemed like a nice place to work," Blume recalled. His arrival bolstered Brookhaven's neutron-scattering program, which was en route to becoming the centerpiece of its solid-state physics group. In 1965, he delivered the opening address outlining the rationale and promise of neutron scattering at a conference on the eve of commissioning the HFBR. In 1971, he became head of Brookhaven's solid-state theory group. In 1972, he spoke at a conference on neutron scattering held to commemorate the new reactor – based in part on the HFBR design – at the Institut Laue-Langevin (ILL), which had just opened in Grenoble, France.

Blume had attended the 1972 conference as a skeptic. At the time, he was investigating ways of improving Brookhaven's neutron facilities, by either upgrading the HFBR or building another reactor. These seemed to him much better ways of carrying forward Brookhaven's solid- state program, and keeping it competitive with ILL, than the newfangled synchrotron-radiation scheme. Blume took his skepticism to Weneser. "I asked Joe, 'Why are we looking at this? Neutron scattering is our strength; why not look at the next generation neutron source?' He said in his typically deadpan way, 'I don't know. You tell me. Look into it.'" Over the next three weeks, Blume did.

> The first thing I did was talk to Joe Hendrie and Herb Kouts about what kind of next generation reactor could be built, to find what was possible in the relatively near term. I discovered that we might be able to get a factor of 2 or 3 improvement; spallation sources were not really in the cards yet. Then I looked into what had been done at the parasitic sites – Tantalus and Stanford's SSRP just about to open – and found what was possible in the relatively near term with a dedicated synchrotron radiation facility. To my astonishment, it was marvelous. First, the increase in brightness over the present was not just quantitative – a factor of 2 or 3 – but qualitative – orders of magnitude. A quantitative step on a major new facility can be a difficult push and the rewards questionable, while a qualitative leap can open up entire new avenues of science. Second, you filled in the entire spectrum. With X ray tubes, you can produce intense lines, like the copper K alpha line that you get from bombarding copper with electrons. It knocks out an inner electron [WIGGLING HIS HANDS TO SIMULATE THE PROCESS] and others fall into its place, producing an intense band of X rays at approximately 1.5 A. If you need 1.6 A, you are pretty much out of luck, for the intensity is way down. A synchrotron source gives you the entire spectrum, and you extract any band you want with a monochrometer. That was exciting.[25]

As the saying goes, scientists are treacherous people, for they are apt to change their minds in response to argument. Blume became a convert, and a sympathetic observer of the project over the next few years. When the Physics Department was reshuffled in 1975 – Samios replaced Weneser as chairman, Blume replaced Chalmers Frazier as Deputy Chairman and head of Solid State Physics, and Victor Emery replaced Blume as the group leader of Solid State Theory – Blume became more than a sympathetic observer: He was officially charged with worrying about the long-term interests of solid-state physics.

And he found the group's presentations in support of the machine cause for worry. Frazier was an excellent physicist, but when making management decisions he was prone to the tentativeness of those unsure of themselves. Green's presentations, meanwhile, were informative but so informal that someone who did not know him might be forgiven for thinking them untrustworthy. Blume, who hated to see opportunity slip away by poor planning, feared that the project would not get off the ground without a more professional approach. He told Vineyard that someone more forceful had to be put in charge of the project-design group. Vineyard took the classic step of putting the person who had made the suggestion, Blume himself, in charge.

Blume was exactly what the group needed. As a scientist, he was highly respected; he was attracted to fundamental and elegant science like most everyone else, but also recognized the importance of valuable but mundane research. As an administrator, his style would be unlike that of many other physicists; he was a master diplomat. He could flatter and cajole, but also come down hard when necessary. He was articulate, often peppering his talks with Latin phrases and classical references, and was politically astute.

In May 1975, Blume prepared and submitted to ERDA's Division of Physical Research a "Schedule 44," an expression of intent to submit a formal proposal in 1976 for the construction of a $19,600,000 facility requiring funding in the 1977 budget, with construction to follow in 1978.[26]

Blume also prodded Chasman and Green to formalize their design. They designed a 1.5-2 GeV machine of high brightness that partially coped with vexing problems of space charge and orbital instabilities by assuming a high (600–700 MeV) injection energy.[27] It had six straight sections to allow room for insertion devices. Green, meanwhile, asked Watson and Perlman to work on shielding – again, a relatively new concept to the solid state researchers.

> Our first reaction was, "What's that?" We hadn't worried about it before. We went to Sam Lindenbaum, who had been in charge of the original shielding at the AGS, who put us in touch with a shielding expert on electrons from Oak Ridge. This guy gave us what we needed: he had the experience, knew the scaling law, came up and talked with us one day, and that was it. Bob Casey from health physics then vetted our estimates and conclusion.[28]

To plan the civil engineering, Blume recruited Jules Godel, an engineer in the Chemistry Department who had worked on the HFBR; he took the scientific plans and worked out a practical budget and schedule. To review Godel's work and provide additional estimates, Blume called on John Lancaster, a retired lab engineer from the construction office, who as a friendly favor to lab employees would often check out the structural soundness of houses they were contemplating purchasing, accepting no more in return than a bottle of cognac.[29]

Still, Blume found managing the group a challenge, for its members, out of habit, acted more like a university senate than proposal planners.

> At one meeting a question involving shielding came up: Do we need one foot of concrete or two? Either Watson or Perlman pointed out a subtlety in the scientific aspect of the problem that the other began to discuss and explore enthusiastically. I got *furious*, the issue on the table was what *worked*. I learned that it was impractical to have the two in the room at the same time. They were smart, knew what had to be done, and caught fire from each other – whereupon you couldn't then get a word in edgewise and all you could do was sit back and let the fire die out. And Green treated formal presentations as if he were in a classroom – he'd sometimes literally pick up a piece of chalk and begin writing on the blackboard. He'd either have the information *in pectore* or in the back of his brain, rather than in the carefully prepared form you need to have when addressing review panels, some of whose members were from rival institutions like SLAC for whom this project was a threat and would be out to attack you.[30]

Blume also had to fend off competition both inside and outside the laboratory, and had to work to hold the community together.

Inside the lab, the most important competition was ISABELLE. At one point Blume was summoned into Associate Director Ronald Rau's office to be reminded that he could not interfere with ISABELLE as the lab's top priority. Blume replied that he had no problem with ISABELLE being the lab's *major* priority, but did have a problem with it being the lab's *only* priority. Other competing lab projects included a linear accelerator to be used in an intense pulsed-neutron source. Outside the lab, over 20 machines worldwide were able to produce and use synchrotron radiation. Half were at synchrotrons and half at storage rings, though almost all were parasitic at high-energy physics facilities. The United States had four: SURF, or the 180-MeV converted storage ring at the National Bureau of Standards in Washington, D.C., which had been renovated as SURF II in 1974; the 12-GeV Cornell Electron-positron Storage Ring (CESR), where some X rays were produced during high-energy physics runs; the 240-MeV Tantalus at Wisconsin; and the 2.5-GeV SSRP at Stanford's SPEAR, the latter two then being transformed into national facilities. What was the best way of extending this: to put money into existing facilities, or to build new ones? With interest soaring, the National Academy of Sciences commissioned the National Research Council, in 1976, to establish a "Panel to Assess the National Need for Facilities Dedicated to the Production of Synchrotron Radiation," chaired by Robert W. Morse, Deputy Director of the Woods Hole Oceanographic Institution, and thus someone with no vested interest in the project.[31] The Morse panel found that interest in synchrotron radiation had risen so swiftly that existing facilities would be overwhelmed within a decade. It estimated that, by 1986, it would take about 40 UV stations and 60 X-ray stations to satisfy the demand – but the four existing facilities, CESR, NBS, SSRP, and Tantalus, had a total of only 17 UV stations and 7 X-ray stations, and even if these facilities were fully expanded, they would meet only a third of the demand. The Morse panel recommended a geographically distributed set of major facilities. Its report bolstered the case for the Brookhaven synchrotron-radiation source.

The UV Revolt

Blume's third challenge was to hold the community together. Until the summer of 1975, the planners had envisioned the NSLS as a single ring that could be run sometimes for the ultraviolet (UV) users, sometimes for the higher-energy X-ray users. But a cultural difference existed between the two groups. For one thing, a single setup often sufficed to carry out several experiments, while UV experiments tended to be more complex. Moreover, UV experimenters were concerned that they would have to get rid of an enormous amount of background in the form of unwanted X rays, creating cooling and radiation problems. Finally, while at X-ray energies one could still get ultraviolet radiation, the reverse was not true. Thus, most of the design parameters of the machine wound up catering to X-ray usage, and the machine would run most of the time in the X-ray mode. The UV users feared that the X-ray users would always get what they wanted.

During a meeting in the summer of 1975, Ward Plummer, a UV user from the University of Pennsylvania, had had enough and spoke up. He pointed out that the machine was being optimized to suit the needs of X-ray users in a way that would make UV users second-class citizens. He was roundly criticized by X-ray users for his remarks. But at cocktails later that day, many other UV users came up to him to voice support, apologizing for not having spoken up earlier.

From this point on, the UV users grew more vocal about standing up for themselves. Other prominent UV users, most notably Eastman, forcefully took up the cause. He did so in a politically shrewd way, promoting a UV ring at Columbia University's Nevis Laboratories. Columbia's C.S. Wu took the bait, arguing that Columbia's Physics Department should not devote faculty and resources to supporting a facility instead of physics. Better to let a place like Brookhaven do that, which after all was founded to support facilities.[32] The result was that Brookhaven was motivated to commit more strongly to support UV research at its planned facility.

Ken Green, who was present at the 1975 summer meeting, noted the problem and began to look into the idea of building two rings. It turned out that adding a 700-MeV ring would cost about a third as much as a 2.5-GeV ring, and add only 10 percent to the cost of the entire facility, while doubling its capacity.

The Proposal

Between the summer of 1975 and the summer of 1976, therefore, the proposal was revised. The first major change was to incorporate two rings instead of one.[33] They were each smaller than the planned original ring – the constraint on size followed from the cost of the building they would be housed in – but they were at least optimized for two different energy regions. A second major change concerned the number of straight sections. Instead of both rings having four, the new interest in helical wigglers con-

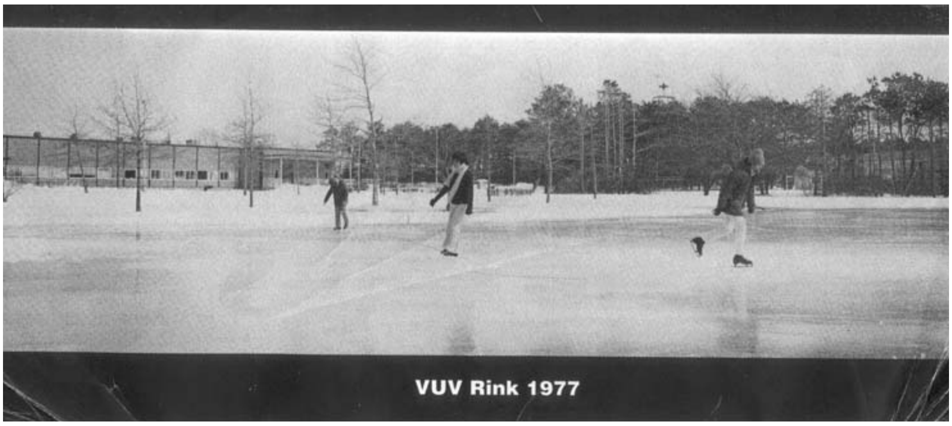

Fig. 7. The ice-skating rink in 1977 that was the future site of the Vacuum Ultraviolet (VUV) storage ring of the NSLS. *Credit*: Brookhaven National Laboratory.

vinced Green that the machine needed more experimental room for insertion devices, so he changed this to six.

A site committee recommended that the most favorable location was across the street from the Physics Department and a few minutes walk from the Chemistry and Biology Departments, the main user departments. That location was almost level with a little dip that tended to collect rainwater – in the summer it had a small pond with frogs that occasionally were harvested for food by gourmand lab scientists, in winter it was a skating rink (figure 7) – so would require little earth moving, and was close to site utilities. The site was also down from the terminal moraine on the island, so it was made of glacially compacted sand, which is as good as granite as the foundation for a high-precision machine that would be sensitive to vibrations.

In the spring of 1976, Blume submitted to ERDA a revised Schedule 44, for a facility now known as project number 78-CH-004 and estimated to cost $21,800,000,* followed a month later by a Conceptual Design Report entitled "Synchrotron Radiation Research Facility" (BNL 21589).[34] The planners had some indications at ERDA that were encouraging – provided that the cost was around $20 million. Everyone knew that this was too little to build a good machine. Watson and others tried to increase that amount but were told to keep quiet. The philosophy was that this would be like obtaining a poorly running, handyman car – better to have it than no car at all, and if you somehow can get one on the cheap, you can use it while slowly putting in better parts as you can afford them. But it was a philosophy with grave risks, and indeed would bring the laboratory to the brink of disaster.

Between the summer of 1976 and the end of the year, when a final budget would be prepared and submitted, came more work, more reviews, and a final proposal. A key

* That year the fiscal year, which previously ran from July 1 to June 30 (and bore the name of the year in which it ended) was changed to run from October 1 to September 30. Thus the request submitted was for FY 1978, to begin October 1, 1977.

event was an ERDA review scheduled for July 14, which Blume dubbed the "Bastille Day" event. He arranged for a dress rehearsal and brought in a colleague who had attended ERDA reviews, who wrote up a thorough performance critique: "Renate Chasman – spoke for 15 minutes – however, if spoken at slower rate for the uninitiated to understand, it would have been much longer," "Ken Green – spoke 31 minutes. Opposite of Renate Chasman – talk could have been shortened a great deal if points were made more quickly and clearly."[35] Blume also arranged for the head of the ISABELLE project to be present, to demonstrate to the agency the lab's solidarity behind both projects. During the review ERDA officials allowed the budget to rise to $24 million. The additional $4 million, however, was not to fix the underbudgeting of the machine, but rather to incorporate additional factors that had been overlooked, such as building insulation and some inflation.

Shortly before the final proposal was submitted, in February 1977, Blume made a change that, while apparently trivial, was salutary in the long run. He removed a word from the title: "radiation." Just at that time, controversies over the biological effects of radiation were in the news locally, nationally, and internationally, and it was becoming a socially unacceptable word.[36] Blume therefore changed the name to "Proposal for a National Synchrotron Light Source" (BNL 50595). In truth it *was* a giant light bulb, one with finely tunable light, *as well as* being, technically, radiation.

The proposal incorporated the pair of new design decisions of the year before – the two rings and additional straight sections (figure 8). The smaller, 700-MeV UV ring had four straight sections and 16 beam ports. The larger, 2.5-GeV X-ray ring had six straight sections and 28 beam ports, some of which would be for the use of "wigglers." Each port could serve multiple lines. A 70-MeV electron linac would fill a small synchrotron, called a "booster," that would raise the energy of the electrons to 700 MeV before injecting them into one or the other ring. In the UV ring, radiofrequency cavities would replace the beam energy lost by radiation, while in the X-ray ring four rf cavities would further accelerate the beam as well as replace lost radiation. The current of each ring would be about an ampere. The proposal envisioned a three-and-half year construction period, including six months of preparation of the experimental facilities. The experimental program would begin April 1981.

The proposal was careful to state the case for a dedicated source over pirated ones.

> No "dedicated" sources of synchrotron radiation with energy above 250 MeV exist in the United States. Parasitic operations have been carried on at the Cornell Electron Synchrotron, at the now dismantled Cambridge Electron Accelerator and at the Stanford Linear Accelerator Center's electron-positron ring (SPEAR). All of these machines were constructed for experiments in high energy physics and utilization of their output of synchrotron radiation has had second priority. Also, their designs were aimed at the high energy physics applications and were far from optimized as synchrotron radiation sources. But, now, the wide range and high value of research done with synchrotron radiation lends strong support to our proposal for construction of a national facility dedicated solely to the production of synchrotron radiation.[37]

The proposal stressed the advantages of building the machine at Brookhaven, as a "major accelerator center" whose "staff has twice designed, constructed and operated

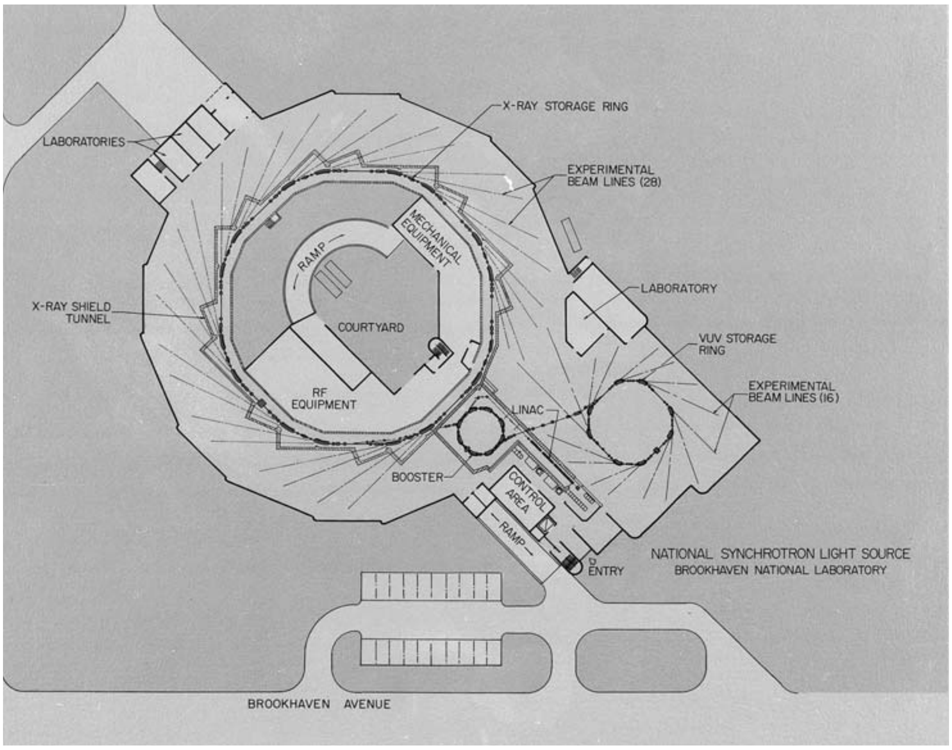

Fig. 8. The NSLS design in the proposal of February 1977, showing the large X-ray storage ring in the upper left and the Vacuum Ultraviolet (VUV) storage ring in the lower right. *Credit*: Brookhaven National Laboratory.

accelerators with the world's highest energy," invented strong focusing, and is at work on ISABELLE. But Brookhaven also has "a great deal of experience in the operation of large research facilities, in their staffing by in-house research groups, and in their utilization by outside groups from university and industrial laboratories both from the United States and from elsewhere in the world," at the AGS, HFBR, and Tandem Van de Graaff.[38]

To cope with the National Environmental Policy Act (NEPA) regulations, the proposal contained an "Environmental Assessment." In comparison to documents drawn up just a few years later, its five pages seem crude. "In summary, it appears that, on the basis of this analysis, the construction and operation of this facility will not have significant adverse impacts on the environment."[39]

The NRC panel had a positive impact on the budget. In mid-January 1977, outgoing President Gerald R. Ford submitted his budget for FY 1978, which included a line item requesting that ERDA spend a total of $24 million for the NSLS, to begin construction in FY 1978, and to be completed by the fall of 1981. At the same time, the NSF budget included requests for funds for upgrading Tantalus, the SSRP, and the Cornell facility, the latter being a national synchrotron-radiation facility tethered to CESR called CHESS, the Cornell High Energy Synchrotron Source, to be finished in 1981.[40] The SSRP, however, was taken over by the DOE, and rechristened the Stanford Synchrotron Radiation Laboratory, SSRL.

By the time the final proposal was submitted and funding approved, however, there were ominous notes. One was that, even at $24 million, the machine was woefully underfunded. Of that amount, $8 million, or a full third, was for conventional construction; the machine portion had to be built for only $16 million, including staff funding. Moreover, instead of receiving the funding in the requested amounts of $11 million in the first year, $9 million in the second, and $4 million in the third, it came in amounts of $5 million, $10 million, and $9 million. That meant the first appropriation, to start on October 1, 1978, was for a whopping $6 million less than hoped for, which would force additional and unplanned-for economies. This new profile, too, magnified the impact of the raging double-digit inflation.

Another ominous note was tension with the SSRL. In the eyes of SSRL officials, BNL was forging ahead with its plans while keeping SSRL deliberately in the dark, to the long-term detriment of synchrotron radiation research. "I certainly believe that friendly rivalry and competition is to be encouraged," Seb Doniach of SSRL wrote to Vineyard in October 1977. "[But] I feel that deliberate attempts to divide the community are very dangerous in that it will weaken our position vis a vis other fields of science which are in need of large amounts of national funding."[41]

Another ominous note was that both Chasman and Green were ill. Realizing that the project desperately needed more accelerator physicists, and alarmed by the worsening health of the two principals, Blume had sought more accelerator-physics talent. With the Accelerator Department preoccupied with ISABELLE, he had to look elsewhere and convinced a theorist in his solid-state group, Sam Krinsky, to convert from solid-state theory to accelerator physics.

Like so many parts of the machine, therefore, Krinsky was borrowed, though he would quickly flourish in his new field. He was born in Brooklyn and received his Ph.D. degree from Yale in 1971 in theoretical high-energy physics. He then switched to condensed-matter theory, which had come to interest him in graduate school. While a postdoc at Stony Brook he met Blume, who was teaching there part-time; Blume recruited him into a temporary appointment in his solid-state theory group, whose other members included Emery and Watson. Blume, struggling to find more talent for the NSLS, took advantage of the dismal job market in physics at that time to lure Krinsky into joining the Accelerator Department unofficially in 1976 and officially a year later. In the spring of 1977, prior to becoming an official member of the Accelerator Department that fall, Krinsky was assigned an office near those of Chasman, Green, and Blewett on the third floor of Building 911. Krinsky thereby became part of the nucleus of a group that would later become the NSLS Department. Though totally new to the field of accelerator physics, he was in good hands. Chasman and Green were still alive and working and took him through the basics. They had worked out the linear (first-order) calculations of their lattice, but had not yet determined the nonlinear properties. These especially were a problem, since they tended to be exacerbated by the sextupoles needed to correct the machine's chromaticity or chromatic behavior, the dependence of betatron-oscillation frequency on energy. The nonlinear properties yield the dynamic aperture, or maximum stable amplitude.* Krinsky learned the new field in six months and began tackling the problems. His apprenticeship culminated that June, when he helped to organize a summer workshop on synchrotron-radiation machine-physics issues, at which experts from inside and outside the lab analyzed the Chasman-Green design (BNL 23695).[42]

Krinsky had arrived just in time. As Chasman's melanoma progressively worsened, she began traveling back and forth to New York City for treatment, and grew visibly weaker during the workshop. Once she nearly fainted and had to be helped out of the room. Green missed the workshop entirely. A chain-smoker with emphysema, he fell ill and left to visit his son in Texas.

During this period, Chasman's former mentor Ernest Courant recognized the need for a lattice design code (computer program) that incorporated the specific features needed to design an electron ring and predict the machine's chromatic behavior in the presence of synchrotron radiation, and took time out from his ISABELLE work to do so. Rather than start anew, he modified Berkeley's SYNCH program, which he got running just in time to be used in the initial ring designs.**

Instrumentation for the machine developed slowly. Green made extensive notes on the optics of various kinds of experiments: what kind of light would be required by the various classes of experiments to be mounted at the source, and what kinds of lenses and filters could be devised to produce it. Sometimes spatial resolution would be most important, at other times energy resolution, occasionally both. Green anticipated a set of canonical classes of experimental needs and the best kind of instrumentation to serve them. Later, Krinsky, Perlman, and Watson took these notes and incorporated them into a section of an extensive article on synchrotron radiation – its physics, the facilities needed to generate it, and its applications.[43] One of Green's ideas was that some of the magnets for the instrumentation be superconducting. Had this been done, it would have made the NSLS the first accelerator in the world with superconducting magnets. But the lab management rejected the idea as fraught with too many potential difficulties.

Project Manager

Meanwhile, with the onset of construction looming, Accelerator Department Chairman Mark Barton convened a committee (whose members also included Chasman, Green, Perlman, and Watson) to choose a project manager. Like almost everything else about the NSLS, this proved more difficult than expected. Since no one at Brookhaven

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* In a linear system, oscillation frequency is independent of amplitude and oscillations are stable for all amplitudes. In a nonlinear system, oscillation frequency can depend on amplitude and there can be a maximum stabile amplitude, called the dynamic aperture.

** The SYNCH program ("SYNCH – a program for design and analysis of synchrotrons and beamlines") was originally created by Al Garren, J. Eusebio, and Ardith Kenney in the 1960s, and evolved over time with additions by E. Courant and others. See, for example, SSCL-MAN-0030, LBL-34668, BNL-49925, FNAL-PUB-94/013.

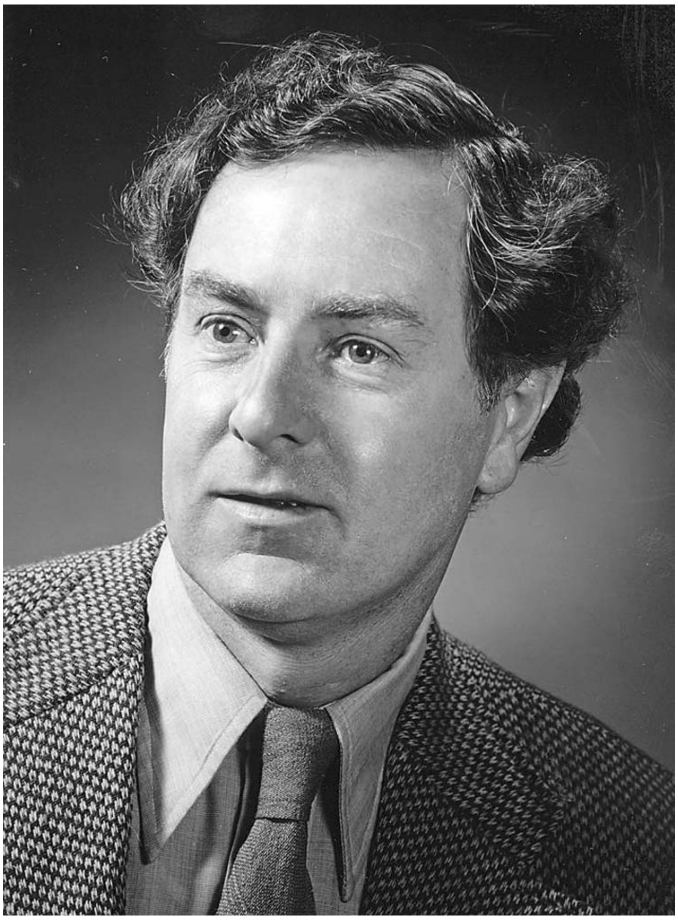

Fig. 9. Arie van Steenbergen, NSLS Project Manager. Credit: Brookhaven National Laboratory.

had experience building large-scale synchrotron-radiation sources – and no one anywhere had experience building dedicated synchrotron-radiation sources – the committee decided to seek someone from outside the laboratory with experience in building electron storage rings. Ewan Paterson was approached, but SLAC convinced him to stay by promoting him to its directorate. Likewise, Cornell promoted Maury Tigner to full professor when that institution was contacted. Neither Ed Rowe of the University of Wisconsin nor Herman Grunder of Berkeley would move to Long Island. Running out of time – in September, less than a month before construction was to begin – the group changed its mind and sought and found someone already at Brookhaven, Arie van Steenbergen (figure 9).

Van Steenbergen was born in The Netherlands and entered Delft Technological University intending to become an engineer. In the library one day he happened to encounter an issue of the *Review of Scientific Instruments* devoted to the Cosmotron.

He was so intrigued by the machine and its subsystems that he secured his own personal copy of that issue, which he still has. He went to McGill University in Montreal for graduate work, and when Blewett came through Canada in 1957 on one of his periodic recruiting trips he enticed a delighted van Steenbergen to become a member of Brookhaven's Accelerator Department. Van Steenbergen headed the 50-MeV proton linear accelerator and became head of the AGS division. In 1967–1968, he spent a year at Fermilab, which Robert R. Wilson ran in somewhat the way Louis XIV ran France: a central, authoritarian leader with clear lines of authority who was constantly looking over the shoulders of his underlings. Van Steenbergen was deeply impressed by Wilson and his style of leadership. Van Steenbergen joined the ISABELLE project in 1974.

Van Steenbergen was known to have some conservative quirks. He insisted that acronyms not have vowels, to prevent cutsy pronunciations. He also insisted that people not go haywire with fancy colors as he deemed them to have done at Fermilab, but instead opted for soft "Delft" colors. He was also known to have conservative traits that could be a blessing or a curse. He took budgets and schedules as seriously as lab administrators demanded, but more seriously than did many scientific administrators. He respected seniority somewhat more than was usual at Brookhaven, and junior collaborators viewed him as reluctant to grant them the freedom to which the lab environment had accustomed them. He liked to personally oversee every aspect of the machine. He would approach the NSLS, in fact, the way Wilson had built the Fermilab accelerator, striving to keep it on time and on budget by driving people hard and compromising the hardware if necessary.

Van Steenbergen knew how to find competent people at Brookhaven, even when virtually all of the accelerator staff was attending to ISABELLE. One of his first recruits was Hank Hsieh, who became the NSLS chief engineer. Born in mainland China, Hsieh had been educated first at Taiwan University and then Virginia Polytechnic Institute, working in industry before arriving at Brookhaven to work in the AGS mechanical-engineering group, eventually becoming its head. Hsieh, a senior engineer with whom van Steenbergen had worked for years, was a strong personality himself, and one of the few members of the NSLS staff to whom van Steenbergen granted significant independence. Both were the kind of leaders whom it is easy to work with, and difficult to work for.

When funding for the NSLS started, on October 1, 1977, it was bittersweet. For in mid-August, Green passed away at his son's house in Texas, at the age of 65. Chasman grew sicker and finally succumbed in mid-October, at age 45. During her five-year struggle with melanoma, she had done the work for which she is best known. Neither lived to see the machine even start construction, and the loss of these two giants was a serious blow to the efforts to develop it.

Thus Krinsky, just in the field a year or so, found himself the sole accelerator physicist at the NSLS. He was soon joined by three others: Roy Blumberg, John Galayda, and Richard Heese. They were all installed, along with van Steenbergen and Hsieh, on the third floor of Building 911, the AGS building. Each was assigned a different part of the NSLS: Krinsky the X-ray ring, Blumberg the UV, Galayda the Booster, and Heese the Linac.

Krinsky was the principal theorist, and besides the X-ray ring worked on lattice calculations, estimating instabilities, properties of the insertion devices, the effects of putting them into the lattice, and so forth.

Blumberg came to Brookhaven from the CEA, and had previously done accelerator experiments at Oak Ridge and Los Alamos. He had worked at the AGS since 1966, much of that time under van Steenbergen. His main role would be to do the lattice-design calculations for the UV ring.

Galayda had met Blewett early in his graduate studies at Rutgers, who secured a job interview for Galayda shortly after he defended his Ph.D. thesis in the summer of 1977. Galayda was introduced to many groups with openings, including ISABELLE and the NSLS, and was enchanted by a lunch with Ken Green. He was hired that October, and would work on the magnet design as well as on the Booster.

Heese had been working at the electron linear-accelerator laboratory in Saskatoon, Canada, and had answered the ad in the October 1977 issue of *Physics Today* for accelerator physicists. When he came for an interview the following month, he was offered a job at ISABELLE or the NSLS; looking dubiously at the ambitious superconducting dipoles of the former, he chose the latter, a decision he never regretted. He worked on the magnets, set up a magnet measurement-and-testing facility, and oversaw the Linac. He arrived in February 1978, and immediately stood out from the others thanks to his shorts and T-shirt; having come from Saskatoon, the Long Island winter felt like spring.

"It was a small group," Galayda recalled, "so we got a great education. Each of us had to work on a bit of everything – the lattice, magnets, power supplies, rf, vacuum, diagnostics. In a bigger organization we'd have been organized and slotted, and probably not learned as much." [44]

Much remained to be done before groundbreaking. Krinsky analyzed and extended Chasman and Green's work, slightly modifying their fundamental idea to produce an improved, more economical lattice for the X-ray ring. They had designed it with six superperiods (a repeating segment of the lattice), each with four dipoles and three quadrupole triplets. Van Steenbergen and Krinsky noted that a structure with two dipoles and two quadrupole triplets per superperiod would make it possible to double the number of long straight sections to twelve while improving key machine properties – reducing emittance and enhancing brightness.

Krinsky, together with Galayda, Heese, and Blumberg, reconciled this conceptual design with engineering and hardware requirements, turning it into a detailed design. The conceptual design had only sketched out the hardware, and the four – together with a handful of engineers – had to plot in detail what the magnets would look like, where they would go, what the power supplies would be, what the vacuum system would be, the beam diagnostics, the rf, and so on, making certain that everything fit together. "For example," Krinsky recalled, "if I had put some magnetic components in a calculation, one of the others might come back and say, 'But now I can't fit this vacuum pump in there,' and I'd have to add 10 centimeters someplace. None of that had been done before the start of the funding."

And everything was driven by the budget, by the need to come in under $24 million. The Department of Energy (DOE) had lost patience with accelerator projects going over budget, and van Steenbergen was going to make every effort to stay within it. At one point he approached Vineyard and said that the project needed $32 million – but was warned in no uncertain terms that such a request would doom the project.

The team found itself having to incorporate painful compromises into the design that would affect the final shape and performance of that handyman car – its basic size and ability to bear loads, say. They tried to take care not to limit performance in ways that subsequent money would be unable to improve. This was not always possible.

One compromise involved an overhead crane for the building. Most experimental halls are designed with enough overhead space for a bridge crane, which is essential for moving heavy equipment and shielding blocks. But overhead space is expensive for heating and air-conditioning purposes, and the ceiling over the X-ray ring was made too low to accommodate such a crane. This would hamper construction of the machine, and of the eventual experimental areas, though it was partly solved by Perlman and the light-source staff with air cushions. Another economy was that the "crotch" or area between the two rings – which was prime real-estate space for an experiment that needed both X rays and UV – was to be used instead for mechanical equipment, which would cost some $25,000 to place elsewhere. Yet another economy concerned the Booster. Galayda and Heese designed a highly efficient Booster, only to have it rejected by van Steenbergen in favor of a less efficient but cheaper design. Blumberg designed a larger UV ring, which van Steenbergen rejected because it would increase the building size. Still another compromise involved the magnets. These were to be built out of thin, one-sixteenth inch steel sheets to carry the flux, each in the shape of a giant "C." Hundreds or even thousands of these sheets would be laminated together to make the magnets – about 1500 laminations per X-ray dipole, and about half that for the UV and Booster dipoles. The X-ray ring would have 16 of these magnets, the UV ring and Booster 8 each. Van Steenbergen insisted on reducing the width of the laminations, since each reduction in size would bring a substantial saving during construction in the copper material, and during operation in the energy needed to power the magnets. He was backed by Hsieh, who recalled how expensive it was to run the AGS – a machine that the laboratory, at times during the energy crisis of the 1970s, could not afford to operate full-time. This provoked a huge fight between van Steenbergen and the four accelerator builders, who feared its impact on the useful magnetic field and dynamic aperture, and thus on beam stability. They compromised on a reduced design, but not as reduced as van Steenbergen had wanted at first. "We cut it pretty close," Galayda said. "We pushed the magnets without a heck of a lot of margin. But they're still working."

Most painful, however, was a compromise on the lattice itself. Krinsky was forced to compromise his ambitious 12-superperiod scheme – which would have given the X-ray ring similar properties to the eventual third-generation synchrotron sources – and roll back the number of superperiods to 8, resulting in an increase in emittance.

Meanwhile, an architect and engineering firm was chosen. In the summer of 1978, the members of another workshop reviewed the overall facility design. In June, Blume gave a "Brookhaven Lecture" on "Synchrotron Radiation and What You Can Do With It." Watson and Perlman published more promotional articles.

"It was fun," Watson recalled. "It was fun until construction began."

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Note: Brookhaven National Laboratory publishes a set of reports (available in the BNL library) that are identified by Number. Thus BNL 21589 refers to Brookhaven Report #21589. Several citations are from Oral or Video interviews. The Oral interviews are in the BNL Historian's Office, the Video Interviews are at BNL Video, in Building 493.

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