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ACCELERATORS, COLLIDERS, AND SNAKES

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■ **Abstract** The author traces his involvement in the evolution of particle accelerators over the past 50 years. He participated in building the first billion-volt accelerator, the Brookhaven Cosmotron, which led to the introduction of the “strong-focusing” method that has in turn led to the very large accelerators and colliders of the present day. The problems of acceleration of spin-polarized protons are also addressed, with discussions of depolarizing resonances and “Siberian snakes” as a technique for mitigating these resonances.

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1. BEGINNINGS

1.1. Growing Up

I came to science naturally. I was born in 1920 in Göttingen, Germany, the son of the mathematician Richard Courant. My mother's father was also a mathematics professor, Carl Runge (probably best known for the Runge-Kutta method for numerical solutions of differential equations). His father-in-law, my great-grandfather, had been the nineteenth-century physiologist and philosopher Emil DuBois-Reymond, a pioneer in electrophysiology and a leading figure in the arguments against vitalism and for the physical basis of all natural processes, including life. The immediate neighbors on our street included the mathematician David Hilbert (my father's mentor and teacher, in whose honor I was given the middle name David) and the physicists Max Born and James Franck.

With such a background, it was not surprising that arithmetic was my best subject in school from the beginning. When it came to secondary school, the choice was between the Gymnasium, which emphasized the classics, and Oberrealschule, which emphasized sciences and modern languages; my parents (and I) chose the latter. There I was a good student, in languages as well as sciences. My marks were consistently 1's (A's), except in athletics, drawing, and handwriting, which were 4's ("mangelhaft"—deficient; my handwriting remains "mangelhaft" to this day). When the son of an American visiting professor joined our class, I got a better mark in English than he did, because his spelling was not very good.

My big enthusiasm was chemistry. I had a lab at home full of test tubes, Bunsen burners, and chemicals. Once there was a small fire (easily put out), but I got a sense of how things were put together.

On January 30, 1933, Adolf Hitler became chancellor of Germany. One of his first acts was to "cleanse" the government service, including the universities. In April my father had the good fortune of being among the first Jews who were placed on "compulsory leave" from their university posts—never mind that, even by the Nazis' own regulations, he was exempt from dismissal as a wounded World War I veteran. Needless to say, he did not regard it as good fortune at the time.

I spent the winter term of 1933–1934 at the Perse School in Cambridge, England, where my father had a temporary lectureship. There I brushed up on my English. After a few months back in Germany, my father accepted a temporary position at New York University, with the prospect that it might be extended. This was enough to enable us to obtain immigration visas to the United States, and on August 11, 1934, we set sail for New York.

Immediately after our arrival, J. Robert Oppenheimer, who had known my father in Göttingen when he was a student there, arranged a scholarship for me at the high



Figure 1 Augustus Klock, my chemistry teacher at Fieldston School (*left*); Margaret Koch, my American history teacher at Fieldston School (*center*); Arnold Dresden, my mathematics professor at Swarthmore College (*right*).

school he had attended, Fieldston School of the Ethical Culture Schools. There I became thoroughly immersed in my new American environment. The school had some outstanding teachers. Mr. Klock, who had also been Oppenheimer's teacher, taught me the fundamentals of chemistry and of scientific attitudes—both in the lab and the classroom (Figure 1). And Mrs. Koch's American history class, with a strong current-events component, taught me about the structure and meaning of the way this country functions; my political attitudes and interests were largely shaped by what I learned from her (Figure 1).

I graduated from Fieldston in 1936 and went to Swarthmore College in Pennsylvania, where I initially majored in chemistry but also took a lot of mathematics and physics.

My fascination with my major paled a bit when I found myself unable to cope with the demands of analytic chemistry. So, after encountering physical optics, and especially the precision of the Fabry-Perot interferometer, I switched my major to physics, with a strong involvement in mathematics as well.

At Swarthmore, in the "Honors" study program, professors met with seminar groups of two to five students instead of giving class lectures. I was particularly stimulated by physics with Winthrop Wright and William Elmore, and by mathematics—especially complex variable theory—with Arnold Dresden (Figure 1).

For graduation, instead of being examined by our professors, we were judged by outside examiners, professors from other institutions. They set comprehensive written examinations, covering the last two years' work, followed by orals. After this two-week ordeal, the examiners decided whether and to what degree we should graduate with honors.

My examiners passed me with "highest honors." One of them, Lee DuBridges of the University of Rochester, persuaded me to come to his university for graduate work, and that is what I did in September 1940.

1.2. Rochester

At Rochester, I was one of just 12 graduate students in physics. Many in that illustrious group have become prominent in the years since: Leroy Apker, Esther Conwell, Robert Dicke, John Marshall.

My principal teachers at Rochester were Victor Weisskopf (Figure 2) and Robert Marshak. Weisskopf taught a marvelous course in quantum mechanics, as well as nuclear physics; Marshak introduced me to electromagnetic theory and solid-state theory.

When I was studying nuclear physics, Weisskopf suggested I analyze an interesting pair of nuclear reactions, $p + {}^7\text{Li} \rightarrow 2\alpha$ and $p + {}^7\text{Li} \rightarrow \gamma + {}^8\text{Be}$. By examining the “excitation functions,” i.e., the dependence of the cross section for

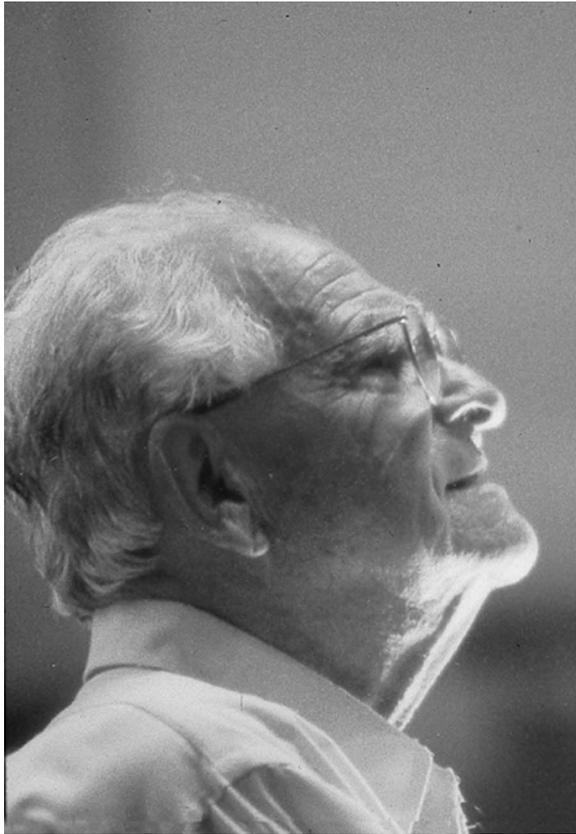


Figure 2 Victor Weisskopf was one of my principal teachers in the graduate physics program at the University of Rochester.

these reactions on the energy of the incident proton, I concluded that the (p, γ) reaction takes place with the proton in a p-state and the $(p, 2\alpha)$ reaction occurs with the proton in an s-state; therefore, the wave function of the of the Li nucleus must have odd parity. I wrote up this result as my master's thesis (1). With war looming, it was uncertain whether we students would be able to stay in school long enough to complete a PhD, so we were encouraged to obtain master's degrees on the way.

Because of the war, Rochester had a subcontract with the MIT Radiation Laboratory, which was working on radar development. An important component was the crystal rectifier-detector, a rectifying junction of a metal and a semiconductor. I was given the task of calculating the rectifying voltage versus current characteristics of such junctions. Weisskopf felt that this work, though not very profound, would qualify as a doctoral thesis, and so I obtained my PhD with a rather short paper, "Effect of Image Forces and Tunnel Effect on Crystal Rectifiers." As part of the radar project, it was classified and could not be published, but I presented an abstract at an American Physical Society meeting after the war (2).

I was really more interested in nuclear physics than solid-state physics. Weisskopf began to disappear from Rochester for weeks at a time, and there were hints that this had to do with the recently discovered phenomenon of fission. One afternoon, while I was in the lab, a long-distance phone call came in for Weisskopf, who was not there. The operator left a message: "Will Professor Weisskopf please call Operator 25 in Albuquerque." So now I knew where the mysterious project was located.

Weisskopf discouraged me from joining him in New Mexico. As I recall, he felt that this project, isolated in a remote corner, would not be congenial for a young single man. He told me that his friend George Placzek was involved in the startup of another project with the same goals in Montreal and suggested that I might find this just as interesting.

As soon as I had passed my PhD orals, I set off for Montreal and joined the fledgling British-Canadian project there. Somehow my draft board was persuaded (without, of course, knowing what we would be doing) that this would aid the war effort more than my joining the army.

1.3. Montreal

The Montreal Laboratory was established in 1942 or early 1943 as part of the British atomic energy project. The centerpiece of the project was the precious supply of heavy water that H.H. Halban and L. Kowarski had brought from France to England, and it was thought wise to move the effort across the Atlantic. So the Montreal laboratory was established as a collaboration between Canadian and British (with other European) scientists; Americans were also recruited.

When I arrived, Placzek explained that the focus of the effort was to design and build a reactor using natural uranium with a heavy-water moderator, primarily with the aim of producing element 94 (then code-named 49, now known as plutonium)

to make bombs for use in the war. “It is essentially a chemical project,” he said. I worked on the theory of neutron transport, diffusion, and slowing down in uranium-moderator lattices. I also worked with Halban on the neutron budget of possible breeder reactors.

The principal effort was the design and construction of the NRX heavy-water reactor at Chalk River. It was decided to build a low-power prototype. This project, headed by Lew Kowarski, was called ZEEP (Zero Energy Experimental Pile). I worked out the detailed lattice design for ZEEP, and it went critical in late 1945, the first reactor outside the United States. With P.R. Wallace, I also worked on the problem of fluctuations of the neutron reaction rate in a reactor; we found that in a multiplicative system the expected fluctuations would be significantly larger than the square root of the number of neutrons (3).

In Montreal I encountered an attractive young lab technician, and Sara Paul and I were married in December 1944.

1.4. Cornell

Once the war ended, it was time to look for academic work. I found a research associate position at Cornell University, working under Hans Bethe.

I went to Ithaca in January or February 1946, leaving my wife in Montreal until she could get a US visa. That turned out to be difficult, presumably because the colleague who had helped us find an apartment in Montreal, Alan Nunn May, had passed classified information to the Soviets—and that made us suspicious characters. But eventually Sara got her visa, and we got back together in Ithaca.

At Cornell I was just about the only member of the newly established Institute of Nuclear Studies who did not come from Los Alamos. I shared an office with Richard Feynman.

Those were exciting days at Cornell. Feynman was devising the path-integral formulation of quantum mechanics. Then he came up with positrons corresponding to electrons going backwards in time, and the new quantum electrodynamics was born. Bethe came up with the essential explanation of the Lamb shift, and Feynman made it relativistic. A newly arrived student from England, Freeman Dyson, put all this into mathematically consistent form. I did not participate directly in these historic developments but was close enough to be fired by the intense excitement.

In nuclear physics, I analyzed the nuclear photoeffect, i.e., the emission of protons or neutrons when nuclei are bombarded by gamma rays. I found that the spectrum and angular distribution of the emitted particles indicate that the process is mainly one of direct absorption of the photon by one nucleon in the nucleus, rather than the production of a heated compound nucleus followed by “evaporation” of a nucleon (4).

Hans Bethe was a consultant at the General Electric laboratory in Schenectady, where work was going on to build one of the first large electron synchrotrons. He asked me to help with some problems on that project. The result was my first work in accelerator physics, a collaboration with him on beam extraction

(5). I also analyzed resonant orbit behavior in circular accelerators, quantifying the parametric resonance that occurs when the oscillation frequency is half the frequency of revolution (6).

2. BROOKHAVEN

2.1. The Cosmotron

Brookhaven National Laboratory was set up in 1946 to enable research on a scale beyond the reach of individual universities. One major feature was a plan to build a particle accelerator, a proton synchrotron, in the energy range of 10 billion electron volts (BeV, now called GeV), an order of magnitude far beyond anything existing or contemplated until then.

A parallel project was under way at Berkeley. In 1947, Ernest O. Lawrence and his people proposed a 10-GeV proton synchrotron at Berkeley. The Atomic Energy Commission (AEC) decreed that there was at most enough money for *one* such ambitious project, but said they might manage to finance projects at both Brookhaven and Berkeley if one were in the 6-GeV range and the other at 3 GeV. As a result, the 6-GeV Bevatron project at Berkeley and the 3-GeV Cosmotron at Brookhaven got under way (7).

In the spring of 1947, M. Stanley Livingston (Figure 3), who was in charge of the Brookhaven accelerator project, and Philip Morse, director of Brookhaven, went on a recruiting trip to find staff for this new enterprise. They presented their plans at Cornell, where Livingston had previously worked with Bethe. I was fascinated, and when they invited me to spend the summer at the new laboratory, I was delighted to accept. I went for the summer of 1947, and a year later I joined the staff full time. Among others in the accelerator group were G.K. Green, John and Hildred Blewett, and a young theorist named Nelson Blachman with whom I worked on several of the theoretical problems of the proposed machine.

Brookhaven in the summer of 1947 was an exhilarating and stimulating place. I was swept along by Livingston's enthusiasm for this marvelous new project. There were frequent meetings to discuss and explore all aspects of the accelerator. The main thing I learned from working with Livingston that summer and in the following years was how one field—say theoretical orbit dynamics—can impact on a very different field, such as vacuum technology, and vice versa.

I collaborated with Blachman. Our first achievement was an analysis of the possible limiting effects of scattering by the residual gas in the imperfect vacuum of the vacuum chamber in which the particles circulate; we found that a vacuum of the quality then technically attainable—in the range 10^{-5} to 10^{-6} Torr—would be sufficient (8).

One of the important problems we tackled was the dynamics of particle oscillations, both transverse (betatron oscillations) and longitudinal (synchrotron oscillations), as modified by the fact that this machine, unlike the earlier electron synchrotrons and cyclotrons, had straight sections between the circular arcs, i.e.,



Figure 3 M. Stanley Livingston was head of the Brookhaven accelerator project in 1947.

noncircular orbits. [Dennison & Berlin (9), following a suggestion by H.R. Crane, had tackled a similar problem at Michigan; Serber was also involved.] We derived a matrix formalism for handling the spatially periodic force fields seen by the particles, and found that (a) the frequencies of the oscillations are more complicated to calculate than in the circular case; (b) the amplitudes of oscillations are modulated; and (c) there might, especially if the straight sections were long, be a “transition energy” at which the stable and metastable phase equilibrium points that give phase stability exchange roles—but we saw that in the Cosmotron, with its rather short straight sections, this problem would not arise (10).

At the end of 1948 Livingston returned to MIT, and the Cosmotron project was run by Milton White and later George Collins, along with Ken Green, John Blewett (Figure 4), and many others. During this time I got to work with and appreciate experimental physicists and engineers as well as theorists; a few names that come to mind are Martin Plotkin, Irving Polk, David Jacobus, and Abe Pressman. Nelson



Figure 4 John Blewett was among those who ran the Cosmotron project at Brookhaven after Livingston's departure in 1948.

Blachman soon left the project and the field of accelerator physics to work in information theory, where he has made quite a name for himself.

A crucial problem was the aperture of the machine. How much space was needed to keep the protons from being lost during the acceleration process? At Berkeley, where the Bevatron was being designed, Lawrence decided to be safe and make the aperture very large—4 × 14 feet, requiring huge magnets. We felt that, on the basis of the orbit calculations Blachman and I had done, it should be quite safe to make the aperture much smaller—9 × 36 inches. John Blewett outlined the design of a fairly compact magnet with a C-shaped cross section (11).

The Berkeley people, with their more conservative approach, built a working quarter-scale model of their machine and found that it seemed to require a larger aperture than we were providing in our version. Had we made a disastrous mistake? We repeated our calculations on orbit requirements and remained firm in our

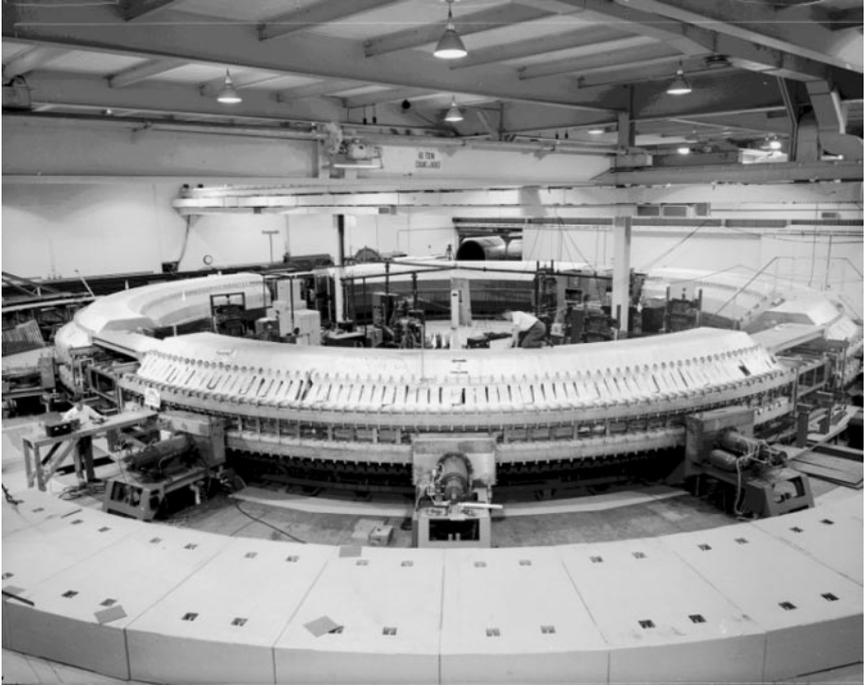


Figure 5 The Cosmotron in 1952.

opinion that 9×36 inches was sufficient. For one thing, the fields in each of the 288 magnet blocks were measured individually, and the blocks were then arranged around the circumference in such a way that the random variations in field would have the least effect on the orbit (12). This encouraged confidence in our estimates.

Everyone worked hard to put the machine together (Figure 5). On May 20, 1952, everything was in place, and the machine worked. A beam of protons was accelerated to a little over 1 GeV—by far the highest energy ever attained by artificial acceleration—just 20 years after Livingston and Lawrence had achieved the first million volts with the cyclotron. Soon the energy got close to the design value of 3 GeV.

2.2. Strong Focusing

Almost immediately, we started to wonder how our success could be extended to higher energy. Our former project head, Livingston, came back for the summer to lead a study group.

In the meantime, CERN (Conseil Européen pour la Recherche Nucléaire) was being formed in Europe: an international laboratory, a joint venture of a dozen European countries, to be devoted to high-energy physics. A delegation of Europeans

was expected to visit us to see whether they could pick up some pointers. They were planning, as the centerpiece of their new international laboratory, to build a proton synchrotron even bigger than the Berkeley Bevatron, with an energy range around 10 GeV. Livingston's study group considered what advice we could give them.

One problem that Livingston brought up was this: The magnets of the Cosmotron all face outward; therefore, negative secondary beams are easily obtained, but positive secondaries tend to hit the inside wall of the machine. In addition, magnet saturation effects tend to reduce the usable "good-field" region at the fields corresponding to top energy. Therefore, it might be better to alternate the magnet sectors, with some having the back legs on the inside and others on the outside.

I pointed out that this might lead to a problem. The focusing gradients might easily be different in the inward and outward sectors, especially in the fringing fields. Because of my earlier work (10) with Blachman on straight sections, I knew how to attack this problem mathematically: Set up matrices for the focusing action of each sector, and multiply them together.

Almost at once, I saw that the alternating gradients could enhance stability rather than weaken it! With the right parameters, the stability could be made stronger than in the conventional case. Livingston quickly saw that this was something fundamentally new, and that the focusing could be pushed to make it much stronger so that the magnet aperture could be really small. That, in turn, makes the magnets—and other components—much cheaper, so "strong focusing" makes it possible to go to higher energies. We published a design (13) with a 1-inch aperture for 30 GeV. Hartland Snyder (Figure 6) explained the new results in terms of optical principles.

This paper described the new strong-focusing principle and presented a conceptual design for a machine that could reach 30 GeV—10 times the Cosmotron's record and five times the energy of the coming Berkeley machine. For our example, we took 120 pairs of sectors with the gradient index $n = \pm 3600$ (compared to $n = 0.6$ in the Cosmotron); the space needed for the particles in the aperture of the magnet was calculated as less than 1 inch, in contrast to 9×36 inches in the Cosmotron and 12×48 inches in the Berkeley Bevatron (whose design aperture had already been reduced from the original 4×14 feet). But we also found that the phase-stability transition, which had come up as an ignorable curiosity in my previous work with Blachman (10), would now occur at an energy in the middle of the acceleration range, presenting a problem for which we proposed a solution—the "transition phase jump."

In the same paper, we also recognized that the new focusing principle was separate from the problem of acceleration and could be applied to beams of particles being guided in paths of any shape, keeping them focused with what came to be known as quadrupole lenses. John Blewett's companion paper (14) showed that this method could make linear accelerators much more attractive than before.

Figure 7 shows four of us with cutout models of the magnet cross-section of the Cosmotron and the new machine.



Figure 6 Brookhaven's Hartland S. Snyder explained strong focusing in terms of optical principles.

A week or two later, the Europeans arrived (Figure 8). They included Odd Dahl, who had worked with high-voltage machines in Washington before the war; Frank Goward, one of the people who had first made a synchrotron work; and Rolf Wideröe, the Norwegian who had first devised a scheme to use radio frequency repeatedly to produce more energy than the corresponding voltage, and whose 1928 paper (15) had set Lawrence on the track that led to the invention of the cyclotron. They went home duly impressed and advised the nascent CERN organization to use the new method to build an accelerator for 30 GeV rather than the 10 GeV that they had planned on.

Two difficulties soon became apparent. First, a group of physicists in England pointed out that imperfections of the magnets, differences between supposedly identical units, could lead to resonant beam blowup whenever the oscillation frequencies were integral or half-integral multiples of the frequency of revolution (16). For a while this caused great pessimism. But we soon saw that the resonances could be avoided by staying between them—albeit at the cost of tightened



Figure 7 The author, Livingston, Snyder, and Blewett comparing the sizes of the Cosmotron magnet and the strong-focusing magnet.



Figure 8 G.B. Collins (Brookhaven) hosting the visitors from Europe: Odd Dahl (Norway), Rolf Wideröe (Switzerland), and Frank K. Goward (United Kingdom).

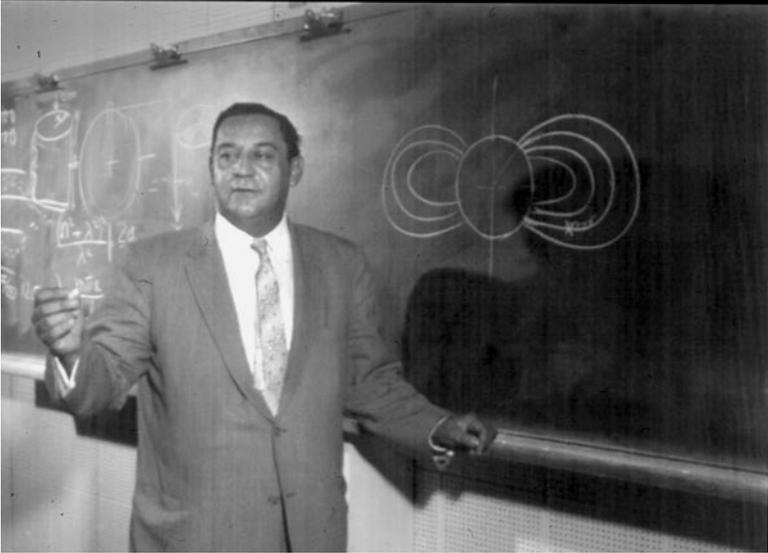


Figure 9 N.C. Christofilos, the engineer who anticipated the strong-focusing accelerator.

tolerances and enlarged apertures (our original estimate of 1 inch had to be amended to 2–3 inches). The second difficulty was that the transition energy, i.e., the energy for change of the position of phase stability, now came right in the middle of the interesting energy range. Fortunately we saw right away, thanks to my earlier work with Blachman, that at transition energy the beam tends to be sharply bunched, making it reasonably easy to jump from the old to the new stable phase. But that seemed awkward, and many people were skeptical.

While all this excitement was going on, some red-faced people at Berkeley dug up and sent us what they had thought was a crank letter from Greece, which they had received a couple of years earlier. An engineer named Nicholas Christofilos (Figure 9) in Athens had thought up essentially the same scheme (17) after reading about plans for the Bevatron. We soon saw that he deserved full credit (18)—and we hired him at Brookhaven. Later he moved to Livermore to work on fusion and on weapons ideas; he died some years later.

Actually, strong focusing had also been anticipated by L.H. Thomas (19) in 1938. He had devised a modification of the cyclotron that would have strictly constant orbit frequency and would achieve the necessary orbit stability by means of azimuthal field variation—indeed, a (weak) version of alternating-gradient focusing. Furthermore, unknown to the open physics community at that time, a project was under way at Berkeley and Livermore to construct a high-intensity accelerator as a spallation neutron source, in order to produce plutonium by bombarding uranium, as an alternative to reactor production for weapons use. A Thomas cyclotron was considered for this purpose. This project was classified, and there

were those in the AEC who wanted to put the lid on our work too because it was related. Leland Haworth, the director of Brookhaven, lobbied vigorously—and successfully—with the AEC people to keep us in the open; this may have been helped by the fact that we had already discussed it with the Europeans.

Work began promptly on projects to build new accelerators incorporating the new principle—in Europe as well as at Brookhaven. During the following years, we continued to exchange ideas and visits with our counterparts at CERN.

At Brookhaven, we (primarily Hartland Snyder and I) worked out the principles of the dynamics of particles as they circulate and are accelerated in the machine (20).

2.3. Elements of Orbit Theory

The heart of the theory of particle dynamics (20) in accelerators is the transfer matrix, which governs the trajectory of the particle. The magnet structure consists of a (usually fairly large) number of identical magnet sectors, each composed of a combination of horizontally and vertically focusing magnet elements. Thus, the magnetic field is periodic around the circumference with period C/N , where C is the circumference and N the number of identical sectors.

If the particle has horizontal and vertical excursions x and z from a reference orbit, its equations of motion are, to first approximation,

$$\begin{aligned}\frac{d^2x}{ds^2} &= -K_x(s)x \\ \frac{d^2z}{ds^2} &= -K_z(s)z,\end{aligned}$$

where s is the distance along the reference orbit and the functions $K_x(s)$ and $K_z(s)$, which depend on the magnet structure, are periodic in s with the sector length as the period. Each particle has initial horizontal and vertical displacements x_0 and z_0 and slopes x'_0 and z'_0 . If the sectors are identical, then in each sector the horizontal displacements and slopes x_1, x'_1 , after traversal of the sector, are given by

$$\begin{bmatrix} x_1 \\ x'_1 \end{bmatrix} = M \begin{bmatrix} x_0 \\ x'_0 \end{bmatrix}$$

where the matrix M may be parameterized in the form

$$M = \begin{bmatrix} \cos \mu + \alpha \sin \mu & \beta \sin \mu \\ -\gamma \sin \mu & \cos \mu - \alpha \sin \mu \end{bmatrix}; \quad 1.$$

an equation of the same form applies to z and z' . Here the parameters α, β, γ , and μ depend on the magnet structure of the cell, and $\gamma = (1 + \alpha^2)/\beta$. The matrix M has unit determinant (this is phase-space preservation as per Liouville's theorem). The parameter μ is the “phase advance” for the sector. If a whole accelerator ring consists of N identical sectors, the matrix for the whole ring is simply M^N , and it

is easily seen that

$$M^N = \begin{bmatrix} \cos(N\mu) + \alpha \sin(N\mu) & \beta \sin(N\mu) \\ -\gamma \sin(N\mu) & \cos(N\mu) - \alpha \sin(N\mu) \end{bmatrix};$$

that is, α , β , and γ are the same as for one sector, and the phase advance is N times that of one sector. This means that x oscillates $\nu = N\mu/(2\pi)$ times per revolution; the parameter ν is customarily called the tune. In general, horizontal and vertical oscillations have separate tunes.

The matrix for one complete period from one point, s , to one turn later, $s + C$ ($C =$ circumference), is thus of the form of Equation 1 with $\mu = 2\pi\nu$. The parameters α , β , and γ depend on the reference point s on the circumference, whereas μ does not. A fundamental aspect is that the coefficient $\beta(s)$ governs the amplitude of the oscillation. The oscillation is generally of the form

$$x(s) = a\sqrt{\beta(s)}\cos[\varphi(s) - \delta] \quad 2.$$

with a and δ constant; furthermore, the phase $\varphi(s)$ is related to $\beta(s)$ by

$$\varphi(s) = \int \frac{ds}{\beta(s)}, \quad 3.$$

which shows that β is not only an amplitude function but at the same time the local oscillation wave length divided by 2π . Thus, the tune is

$$\nu = \frac{1}{2\pi} \oint \frac{ds}{\beta(s)} \quad 4.$$

with the integral taken around the whole circumference.

If the magnet elements are misaligned (displaced from their perfect positions) or the magnet sectors are not exactly identical, then perturbing fields arise, which are periodic with the period C but not exactly with the subperiod C/N . Thus, the perturbing field contains all integral multiples of the revolution frequency, and if the oscillation frequency ν equals any integral k , the perturbations produce a resonant blowup, as our English colleagues (16) had pointed out. Further resonances occur whenever the combination tones of the oscillation and the azimuthal perturbing field, $k \pm \nu$, resonate with ν , i.e., if ν is any half integer. It is also possible that the focusing magnet elements are tilted, in which case the horizontal and vertical focusing actions are coupled. This leads to ‘‘coupling’’ resonances whenever the sum or difference of the horizontal and vertical tunes, $\nu_x + \nu_z$ or $\nu_x - \nu_z$, is integral. But we showed (20) that whereas the sum resonance $\nu_x + \nu_z$ generally leads to instability, the difference resonance $\nu_x - \nu_z$ does not.

2.4. Nonlinearity

The above assumes that the equations of motion are linear in the displacements. In fact there is always some nonlinearity, i.e., the restoring forces contain terms in $x^2, x^3, z^2, z^3, \dots$. These produce combination tones with the oscillation frequencies,

and therefore one might think that resonances would occur at all rational values of ν . Fortunately, in general, it turns out (21) that in the presence of nonlinear perturbations only rational frequencies $\nu = p/q$ with $q = 2$ or 3 , or sometimes 4 , lead to instability. Therefore, it is possible to design machines with stable orbits by ensuring that the tunes avoid these values. But high precision is needed.

I looked at some of the problems of nonlinearity using the new—and then revolutionary—UNIVAC computer at New York University. That computer had a random access memory of 1000 words (10 kilobytes); a significant calculation took all night. With a grossly simplified model of a periodic cell, I found that oscillations were stable until the nonlinearity became quite large. Figure 10 (recreated on today's computers to duplicate the old results found then), shows portions of a phase plot, x versus x' , for a certain nonlinear parameter of 48 and 49. It is seen that between $\alpha = 48$ and 49 there is a sudden transition between a smooth invariant curve and random stochastic behavior (22). This can be seen as an illustration of the Kolmogorov-Arnold-Moser (KAM) theory of dynamics; at the time (1953) it was not well understood.

2.5. CERN—A Friendly Race

Almost immediately after our first proposal in 1952, John and Hildred Blewett and I were invited to Europe to meet with the people organizing the new European project. We presented our new results in Paris to a group initially sponsored by UNESCO, under the leadership of Pierre Auger. We then traveled to Geneva to inspect the future site of the project. I went on to Göttingen to give a talk to Heisenberg's group on our work, and to Copenhagen to discuss our ideas with Niels Bohr.

CERN began a project to build a 25-GeV strong-focusing proton synchrotron. They invited the Blewetts to spend a few months in Bergen, Norway, to help with the preliminary design work. The laboratory was then set up in Geneva, led by John Adams (Figure 11).

In the following years, we at Brookhaven collaborated closely with CERN. There were numerous visits back and forth. CERN set up international conferences on accelerators in Geneva—a preliminary one in 1953 and large-scale ones in 1956 and 1959; I participated in all of these.

The CERN proton synchrotron, called the PS, was very similar to our Alternating Gradient Synchrotron (AGS), not only in size but in most details of the design. We kept exchanging ideas and reports. One small difference: To control focusing strength and nonlinearities, CERN proposed auxiliary windings (pole-face windings) on the faces of the magnet poles. We accomplished the same ends with discrete correcting elements between the main magnets. At one of the Geneva meetings, John Adams bet me 10 Swiss francs that we would also go to poleface windings before we were through. After the machines were finished some years later, I collected the 10 francs from John.

The race went on. CERN won—they had an accelerated beam in their machine in 1959, about nine months before we did. At least part of the reason was Brookhaven's electron analog.



Figure 11 John B. Adams, director of the Proton Synchrotron project at CERN.

2.6. The Electron Analog

Because of worries about the phase transition energy, Brookhaven built a test accelerator to investigate whether and how the necessary phase jump could be handled. This machine used electrons, whose critical energy lies in the MeV range rather than the GeV range, and used electrostatic rather than magnetic fields (Figure 12) to guide and focus the particles. It turned out—as we expected—that the beam behaved very well as it went through the transition. However, oscillograms (Figure 13) showed that nonlinear resonances were real; we saw that there were orbits locked in to resonances and that part of a beam could be lost when traversing a resonance (23).

Just as the electron analog was showing that the transition-energy phenomenon was manageable, we had a visit from Vladimir I. Veksler, the Russian physicist who had invented the synchrotron (24). Veksler and his group were planning to build

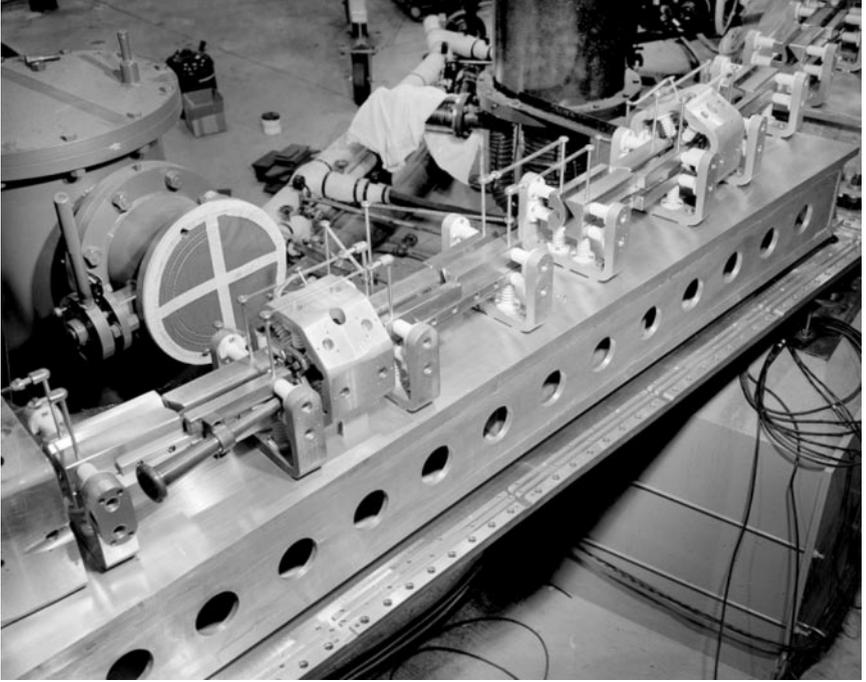


Figure 12 Section of the electron analog showing electrostatic dipole and quadrupole lenses.

a large (50 GeV) alternating-gradient proton synchrotron in Russia, and believed that, to avoid the transition-energy problem, they had to go to a very inefficient (though ingenious) layout with reversed bending magnets. When Veksler saw our demonstration, he abandoned the negative bends, with the result that the Russian machine (at Serpukhov, near Moscow) was eventually completed at 70 GeV instead of 50.

But the electron analog cost us time. Our European counterparts at CERN, who were building a machine very much like ours, had decided from the beginning that the transition problem was not serious and did not have to be modeled. As a result, they proceeded faster than we did. The CERN PS achieved 25 GeV in 1959, whereas our AGS achieved its first beam, at 30 GeV, on July 29, 1960 (Figure 14).

Both the Brookhaven AGS and the CERN PS have been in continuous operation ever since (Figure 15).

3. IMPROVEMENTS AND EXTENSIONS

The AGS initially accelerated 10^9 protons per pulse, once every 3–5 s. Many improvements were made immediately to the ion source, the efficiency of the injector, and everything else. Soon the milestone of 10^{10} was reached, and the next

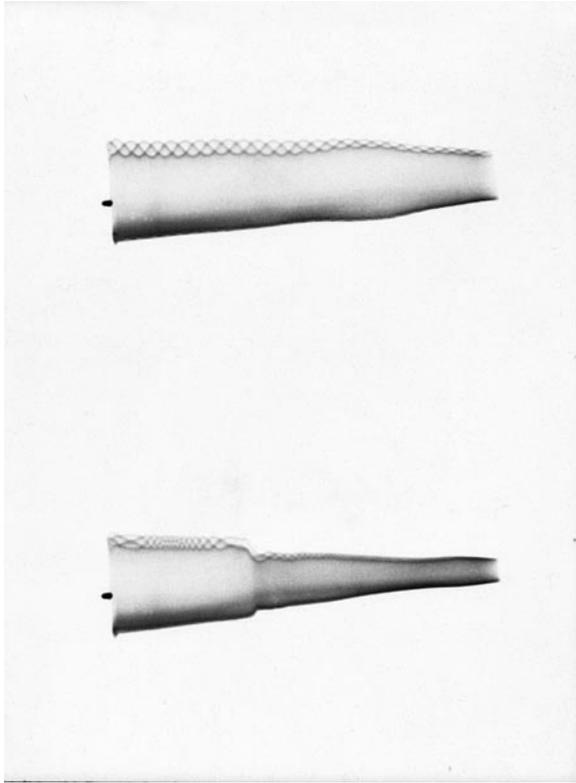


Figure 13 Oscillograms showing nonlinear resonance behavior in the electron analog.

factor of 10 did not take long (today the intensity is close to 10^{14} , with a repetition time of 1–2 s).

As soon as the AGS had its beam—in fact even earlier—studies were begun, at Brookhaven and elsewhere, to see how intensity could be improved and energy increased. I was involved, either as a participant or as an active observer, in several of these, particularly in what soon became MURA.

3.1. MURA

Shortly after the AGS project began, a study group was set up to propose a high-energy accelerator for the Midwestern region; the group soon became MURA (Midwestern Universities Research Association). It started with a two-week tutorial workshop at Brookhaven, where we discussed our new ideas with a group of the Midwestern physicists; at the end of the summer of 1953, I took part in an intensive session at the University of Wisconsin in Madison. The leader of the MURA study was Donald Kerst (Figure 16), who had built the first betatron, and



Figure 14 The first AGS beam! John Blewett (face hidden), the author, L.J. Haworth, Martin Plotkin, Hildred Blewett.

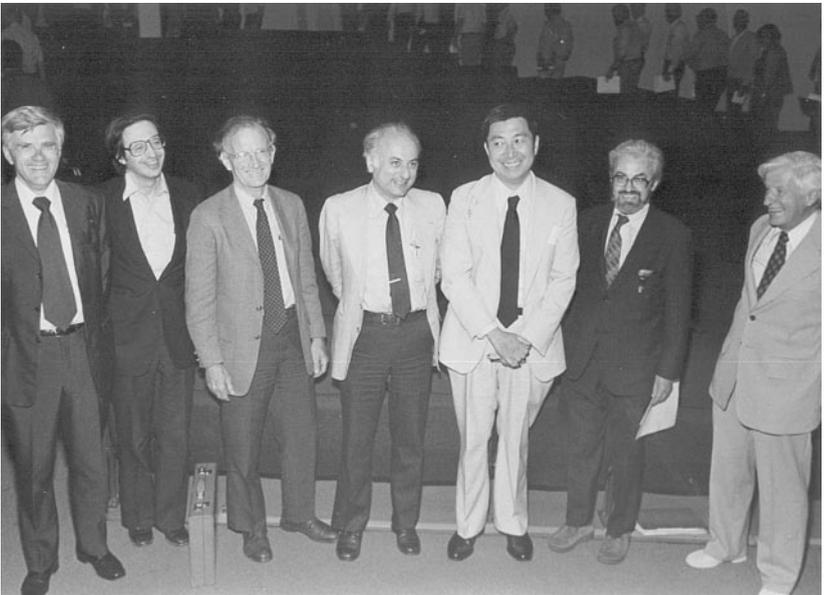


Figure 15 At the twentieth anniversary celebration of the AGS in 1980: Kjell Johnsen, Nick Samios, Val Fitch, Mel Schwartz, Sam Ting, the author, John Blewett.



Figure 16 Donald W. Kerst, leader of the Midwestern Universities Research Association (MURA).

had (with R. Serber) first elucidated the principal basis of orbit stability theory (25).

MURA's first major achievement was the FFAG (fixed field, alternating gradient) concept, first suggested by K.R. Symon (26, 27). Here the particles are accelerated in a ring of DC magnets; as a result, more particles can be accelerated in a given time so that higher time-average intensity can be achieved. The disadvantage was that, in the simplest version, the magnetic field contained sectors with reverse field, so that the overall circumference was larger than in a pulsed-ring machine. This defect was later overcome by using spiral sectors (28).

Working models of FFAG accelerators (using electrons) were soon built. Figure 17 shows the first FFAG model at the University of Michigan.

Kerst soon realized that with the high beam intensities that the FFAG promised to deliver, it would be feasible to build two such machines with the beams colliding, thus making all the energy of both beams available for interactions, since the lab system would now be the same as the center-of-mass system (29) (see below).



Figure 17 The Michigan FFA model in 1954 or 1955, with the author, Tihiro Ohkawa, Otto Frisch, and David Judd.

3.2. Toward Higher Energy

In the summer of 1959, I took part in a study at MURA's Wisconsin headquarters, along with people from many places, to look at all sorts of possibilities for the future. Here we speculated on improvements beyond the Brookhaven and CERN machines, which were approaching completion. The most significant improvement was Sands' suggestion of a "cascade synchrotron" (30). The idea—now a matter of course but then novel—was to use a moderate-energy synchrotron as an injector into a very large one, thus reducing the aperture requirements of the large synchrotron. Sands estimated that a 300-GeV machine could be built at comparatively modest cost, less than \$100 million (an amount that was nicknamed "a pittance" at this workshop).

In 1963, I served on a committee chaired by N.F. Ramsey to consider recommendations for the future of accelerator development in the United States. The committee believed that the best way to advance physics was to go for even higher energy than the AGS. It was felt that the most fruitful approach would be to build a real high-energy accelerator, in the range of 200 GeV or more, rather than building a colliding-beam extension to the existing AGS with the addition of storage rings. We noted that CERN had decided to go for colliding-beam storage rings, which

were to be added to their existing PS. We decided that one such experiment in the world was enough, and that the higher potential intensity of the high-energy ring would outweigh the higher center-of-mass energy of the storage ring system. Therefore, we did not recommend storage rings to be added to the Brookhaven AGS, but we did recommend that the next step after the 200-GeV machine might well be a 1000-GeV project at Brookhaven.

3.3. Fermilab

A design study for a 200-GeV machine had already been undertaken at Berkeley. As a result of the recommendations of the Ramsey Committee, the AEC decided that the new machine should not necessarily be located at an existing laboratory and solicited proposals from all interested parties. Sites for the new laboratory were proposed all over the United States, and a committee was set up to evaluate the many proposals. I took part in this evaluation process.

The final decision was to establish the new National Accelerator Laboratory at Weston, Illinois, 20 miles west of Chicago. Robert R. Wilson of Cornell University was named director.

Wilson rented the tenth floor of the “Executive Plaza” building in a shopping mall at Oak Brook, Illinois and assembled a staff of accelerator physicists and engineers to design the new project. I took a leave from Brookhaven in 1968 to join this group.

Bob Wilson did not believe in private offices. The tenth floor had desks all over, with, at most, rudimentary partitions between them, so that we could always easily wander about to see what others were doing. The crucial emphasis was on simplicity and economy. Wilson strongly believed in designing the magnets and other components so that they would just barely meet requirements—and aperture requirements, etc. were pared down to just slightly above the minimum indicated by theoretical calculations. I worked on these calculations, along with Lee Teng, Lloyd Smith, and others. I returned to Brookhaven in the spring of 1969.

Wilson’s design philosophy resulted in a machine that went up to 400 GeV instead of the 200 initially specified, and at a cost of under \$250 million, far less than had been envisaged in the original, conservative Berkeley study. The machine worked by 1972, less than three years after construction had started. In 1974, the laboratory was renamed the Enrico Fermi National Accelerator Laboratory (Fermilab).

4. COLLIDING BEAMS

When an energetic particle collides with a stationary particle, the energy available for interesting interactions (i.e., the energy in the center-of-mass system) is only part of the energy of the incident particle. It is half of the energy of nonrelativistic protons hitting protons, and is only proportional to the square root of the incident-particle energy in the relativistic case, as first emphasized by Feshbach & Schiff

(31). Therefore, colliding beams can reach higher effective energies than beams striking fixed targets—but, since a particle beam is generally much less dense than a solid target, the reaction rate is expected to be low, and was for years thought to be too low to be worth considering. With the development of FFAG, as described above, beam intensities promised to be sufficient for colliding beams with interesting reaction rates (29). Colliding beams from two FFAGs at 21.6 GeV would give the same center-of-mass energy as a single 1000-GeV beam. O’Neill (32) proposed an alternative approach: a pair of intersecting storage rings fed by a beam extracted from an accelerator, with the storage rings containing a beam just at the final energy.

At the International Conference on High-Energy Accelerators held at Brookhaven in 1961, several proposals were presented for colliding beams of electrons on electrons, electrons on positrons, and protons on protons. At the following International Conference, in Dubna (USSR) in 1963, we heard of progress in these projects, particularly that of Budker and his group in Novosibirsk, Siberia.

Early in 1965 I was invited, along with Andy Sessler, Fred Mills, and a few others, to visit Novosibirsk. We were welcomed by the head of the Institute of Nuclear Physics, Gersh Itskovich (also known as Andre Mikhailovich) Budker (Figure 18).

Budker was a dynamic and charismatic leader. He managed to keep his institute pretty independent of the Moscow bureaucracy 2000 miles away. I might just mention that on a later visit to Novosibirsk, shortly after the Soviets had put an end to the Prague Spring in 1968, I was at a dinner at Budker’s house with a few other Americans, and he said: “If you don’t question me about Czechoslovakia I won’t question you about Vietnam.”

Back to the 1965 visit. The electron-electron collider VEPI was operating, and we were intrigued to see, with our own eyes (via synchrotron radiation), changes in the beam shape as the beams were brought into collision—a graphic demonstration of the beam-beam interaction.

4.1. ISABELLE

When superconducting high-field magnets appeared on the scene, we began to look into the possibility of a proton-proton collider in the range of 200 GeV or more, to be fed by the AGS as an injector.

Workshops and studies began in about 1971. The project was called ISABELLE—ISA for “Intersecting Storage Accelerator”; BELLE for “beautiful.” I participated by examining the possibilities of various lattice configurations, the problems of beam-beam interactions, and many other aspects.

Soon we raised our sights to a goal of two 400-GeV beams intersecting in six places in a ring of about 3.8 km circumference, using superconducting magnets. (In the meantime, Fermilab started work on a superconducting second ring in their tunnel, which reached 800–900 GeV in 1984; it is called the Tevatron because its energy is close to 1 TeV). While I concentrated on the lattice configuration and overall questions of orbit stability, others worked out the radiofrequency acceleration

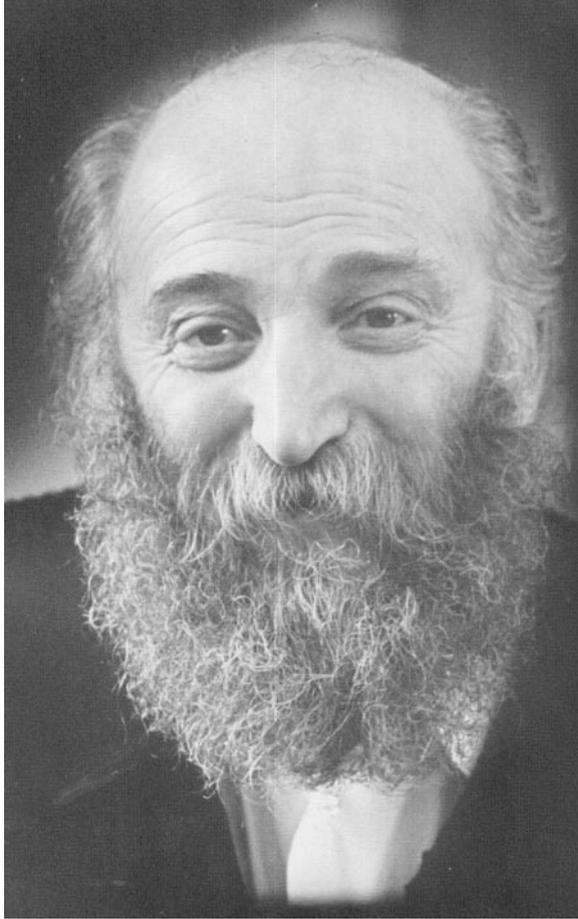


Figure 18 Gersh Itskovich Budker headed the Institute of Nuclear Physics in Novosibirsk at the time of my 1965 visit.

system, the vacuum system, and foremost of all the cryogenic magnet design and the refrigeration system.

Eventually the design was approved by the Department of Energy (DOE), and construction started in 1978. A ring tunnel was constructed on the Brookhaven site northeast of the existing AGS, with access tunnels to steer the beam from the AGS into the ISABELLE tunnel. The massive cryogenic system was built, with a 25-kW refrigerator liquefying helium at a temperature of 4K—the largest such facility at this temperature.

But the superconducting magnets gave more trouble than expected. The design, using cable with twisted strands of fine niobium-titanium wire, worked fine

in prototype, but the good behavior of the first prototype could not be repeated reliably in multiple production magnets—too many of them would quench (lose their superconductivity) before reaching the field strength (about 5 T) needed for operation at full energy.

The difficulty with the magnets was finally solved by a new design in the middle of 1983. But it was too late. Word of our troubles had spread widely in the community of high-energy physics, and also among journalists and DOE officials. A committee of prestigious particle physicists was convened to consider whether the project should proceed. By a vote of 9 to 7 (or something like that) these “wise” men and women recommended that the ISABELLE project be abandoned, and a new project started from scratch: a superconducting supercollider (SSC) with two 20-TeV (2×10^{13} eV) beams colliding. It was felt that this energy would be more productive of interesting new physics than the 400 GeV of ISABELLE.

The DOE wasted no time in accepting at least the first part of the recommendation, and ISABELLE became WASabelle. Brookhaven was left with a large, unoccupied tunnel and a large, unused cryogenic complex. I was furious—as were many people at Brookhaven.

The termination of the ISABELLE project, partly due to lack of support by physicists who should have known better, set a precedent for the termination of the SSC ten years later.

4.2. The Superconducting Supercollider

Workshops were convened in Ithaca, Snowmass, and Ann Arbor to explore whether a superconducting supercollider in the range of 20 TeV per beam was practical and desirable. It soon seemed clear that existing designs could be extended into this energy range. Initial guesses at the cost of the machine came up with \$2–3 billion.

A Central Design Group (CDG) was established at Berkeley, and I began to take frequent trips there to work with them. By 1986, a conceptual design was completed, proposing a ring of 6.5-T superconducting magnets in a circle of ~ 87 km circumference.

In 1987, it was decided that the SSC should be built, and site proposals were solicited. The National Academy of Sciences appointed a site-selection committee, on which I served. One day a huge Federal Express package containing 43 proposals arrived at my house. The proposals, some from state commissions, some from independent groups, came from all over the country—from the coldest places (near Fairbanks, Alaska) to the hottest (south of Phoenix, Arizona). The Alaska proposal pointed out that the site was conveniently located—not more than 10 hours’ flying time from Fermilab, CERN, or Moscow!

We attempted to identify the most suitable proposals. We considered geology—how easy would it be to dig a level tunnel? Or could it be slightly sloped (like the LEP tunnel at CERN)? Or could it have a kink in it—to what extent would that compromise orbit stability? Was there a risk of seismic disturbance? Other questions: What technical support infrastructure was available in the vicinity? How

accessible was the location from all over the country and the world? What about the cultural atmosphere in which staff members would live?

We were asked to come up with half a dozen finalists. Our final list included Illinois (adjacent to Fermilab, with the Fermilab accelerator as injector), Michigan (between the University of Michigan and Michigan State University), North Carolina (near the Research Triangle), and New York (near Rochester). I received a dozen letters from concerned citizens in the Rochester area begging us not to select the Rochester site. Remarkably, all these letters, handwritten, made exactly the same points and all had my address slightly wrong in the same way—clearly someone had organized that campaign.

The DOE made the final decision: The SSC Laboratory would be built south of Dallas, Texas, surrounding the city of Waxahatchie.

The focus of the study group moved to Dallas. I turned down an offer to join the SSC staff full time, but as a consultant I traveled to Dallas every few weeks or so for the next several years to join the design effort under way there.

The details of how the machine would be put together soon were worked out. There would be a cascade of four circular accelerators, starting with a “Low Energy Booster” and ending with the final ring. The necessary land for the ring was acquired, and excavation of the tunnel commenced. Prototype magnets were built, and a string of dipole and quadrupole magnets were operated together in a “string test.” Magnets for the Low Energy Booster began to be built in Novosibirsk, Russia.

But clouds were forming. The cost of the project had been estimated at \$4 billion when construction began (already twice the estimates of the initial workshops). As detailed design proceeded, it became clear that this was too low, partly because of overoptimistic guesses on components and partly because, to ensure reliable operation, some components—especially the magnets—would have to be built more generously than initially proposed. By the end of 1992, the estimated cost was up to \$8 billion, and if experimental facilities and the first few years’ operating expenses were included, it would be even more, in the range of \$10–12 billion.

Some physicists from fields other than high energy began to voice misgivings about whether particle physics was valuable enough to warrant such expenditures. Even a Nobel Prize winner and the incoming president of the American Physical Society joined the clamor. Members of Congress became aware of dissension in the ranks of physicists.

Late in 1993, the United States Congress voted to terminate the SSC project. A partially dug tunnel remained. 2000 scientists, engineers, technicians, and support people needed new jobs. \$2 billion had been spent for nothing.

4.3. The Relativistic Heavy Ion Collider

After the ISABELLE fiasco, we at Brookhaven considered what to do next. I don’t remember who first proposed it, but in 1984 we began to think of using the existing tunnel and cryogenic facility to build a colliding-beam accelerator after all, but with heavy-ion beams rather than protons. It was hoped that when two high-energy

heavy ions collided, the individual protons and neutrons would dissolve and form a plasma of quarks interacting by way of gluons, and that at ~ 100 GeV per nucleon this would be different from proton-proton or proton-neutron collisions.

To get 100 GeV per nucleon we needed superconducting magnets of around 3.5 T, less ambitious—and therefore more certain to work without difficulty—than the 5 T that had given us trouble with ISABELLE. We worked out a magnet lattice—again somewhat more relaxed than the old one—and all the other requirements for the proposed Relativistic Heavy Ion Collider (RHIC).

To get heavy-ion beams to collide, it is necessary to strip the ions completely. The heaviest ions for which this seemed to be reasonably efficient, at the energy produced by the AGS (used as the injector for RHIC), was gold ($Z = 79$, $A = 197$); thus, the focus was on building a gold accelerator. Possibly uranium is in the future.

Again it took several years to get the program approved. In 1991 construction finally began, starting with magnet prototypes. In early 2000 collisions between two gold beams at 100 GeV per nucleon were indeed achieved—the same total energy for the two gold nuclei as SSC would have had for two protons. Details of the machine are described in a review article by Harrison et al. (33).

5. POLARIZED PROTONS

5.1. Resonances

In 1961, Vernon Hughes invited me to join the Yale physics department on a part-time basis. I flew or drove across Long Island Sound once a week to teach and discuss physics with the Yale staff. (Among my students one year was the present Editor of this journal, Chris Quigg.) Hughes suggested that I look into the possibility of accelerating polarized protons in a high-energy machine. I discovered a field that has kept me engaged ever since.

Froissart & Stora (34), using the formulation of Bargmann, Michel & Telegdi (35), showed that in a static magnetic field the spin of a particle precesses according to the equation

$$\frac{d\vec{S}}{dt} = \vec{S} \times \vec{\Omega} = \frac{e}{m\gamma} \vec{S} \times [(1 + \gamma G)\vec{B}_{\text{tr}} + (1 + G)\vec{B}_{\text{long}}], \quad 5.$$

where $G = 1.7928$ is the anomalous magnetic moment coefficient of the proton, and \vec{B}_{tr} and \vec{B}_{long} are the components of the magnetic field transverse and parallel to the particle velocity. One way to look at this is to say that the anomalous moment effectively transforms proportional to the energy.

The effect is that, in a uniform vertical magnetic field, the spin precesses around the vertical axis at a rate of ν_{sp} times the orbital revolution frequency, with the “spin tune”

$$\nu_{\text{sp}} = 1 + \gamma G. \quad 6.$$

In a coordinate system rotating with the orbit, the ratio is γG .

But the actual magnetic field is not exactly uniform and vertical; the field in Equation 5 as seen by the particle also contains horizontal components. These arise from the focusing fields that govern vertical oscillations, as well as from possible alignment and construction errors. These fields will turn the spin away from the vertical. If these depolarizing fields have a frequency component γG (in the reference frame of the particle), resonance arises, and the spin can be substantially changed, leading to depolarization.

Because of misalignments and field errors, the horizontal fields produce a central orbit that deviates somewhat from the ideal orbit; the deviations of the closed orbit and, with it, of the field contain all integral harmonics of the orbit frequency. This leads to depolarizing resonances whenever $\nu_{sp} = k$ (k integral):

$$\gamma G = k \text{ (imperfection resonances).} \quad 7.$$

The frequency of vertical oscillations is the vertical tune ν_z multiplied by the orbit frequency, where ν_z is determined by the lattice (magnet configuration) of the ring. In an alternating-gradient accelerator or storage ring, the oscillations are not purely sinusoidal but are modulated by the overall periodicity of the lattice (12-fold for the Brookhaven AGS). Therefore, the resonance condition for spin depolarization by vertical oscillations is

$$\gamma G = kP \pm \nu_z \text{ (intrinsic resonance).} \quad 8.$$

(These resonances are called “intrinsic” because betatron oscillations inevitably take place in any ring.) Here k is any integer and P is the periodicity of the lattice structure.

As protons are accelerated, they encounter these resonances. The imperfection resonances are $Mc^2/G = 523$ MeV apart; there are two families of intrinsic resonances with spacing $523P$ MeV. On each traversal of a resonance, one may expect depolarization. What can be done to prevent this?

Froissart & Stora (34) showed that, if a resonance has a strength ε (defined as the normalized Fourier component of the perturbing field at the resonant frequency), then on traversal of this resonance the polarization is multiplied by a factor

$$\frac{P_f}{P_i} = 2 \exp[-(\pi\varepsilon)^2/\Delta] - 1, \quad 9.$$

where Δ is the rate of change of $\gamma G - (\gamma G)_{\text{reson}}$ per revolution. Thus, if the resonance is so weak that $|\varepsilon| < 0.0225 \sqrt{\Delta}$, the polarization is reduced by $< 1\%$, but—more interestingly—if the resonance is strong, with $|\varepsilon| > 0.733 \sqrt{\Delta}$, then the spin is reversed and the new spin is again within 1% of the old one. (This spin reversal is the same phenomenon as the spin reversal in “adiabatic fast passage” in the context of nuclear magnetic resonance.)

I came to the conclusion (36) that depolarizing resonance could most easily be avoided in a weak-focusing machine such as the proposed Princeton-Penn accelerator, where there were 16 identical periods, so that the intrinsic resonances



Figure 19 At the 1974 spin symposium: Alan D. Krisch, Louis Michel, P.A.M. Dirac, J.D. Roberts.

would lie above the acceleration range. On the other hand, resonances in strong-focusing machines such as the Brookhaven AGS appeared likely to be troublesome.

At Argonne National Laboratory, the 12-GeV Zero Gradient Synchrotron (ZGS), a weak-focusing accelerator, began a program of accelerating polarized protons. The ZGS physicists managed to get through the intrinsic resonances by rapidly jumping over the resonances with the help of pulsed quadrupoles; imperfections were corrected sufficiently to make their effects very slight. The ZGS team succeeded in obtaining polarized protons up to the full energy of 12 GeV (37).

In 1974, Alan Krisch initiated a symposium at Argonne to discuss high-energy spin physics, including acceleration of polarized beams. This was the first in a series of biennial meetings on the subject that have continued to the present day. The highlight of this meeting was a talk by an honored guest, P.A.M. Dirac (Figure 19). He fascinated us with his lecture “An Historical Perspective of Spin” (38), in which he recounted the discovery of the Dirac Equation.

5.2. Siberian Snakes

At a workshop in Ann Arbor in 1977, I came across a preprint by Ya.S. Derbenev & A.M. Kondratenko from Novosibirsk (39). They showed that if a device is inserted in an accelerator ring that rotates the spin by 180° about a horizontal axis, then depolarizing fields will perturb the spin in opposite directions on alternate

turns, and as a result the spin tune—ratio of overall spin precession frequency to orbital frequency—is $\frac{1}{2}$, *independent of energy*. Thus, as a particle is accelerated, it no longer encounters energies at which the spin tune resonates with the orbit. Moreover, they showed that such a rotator could consist of a sequence of several transverse deflecting magnets, deflecting the beam alternately in the vertical and radial directions, with no net deflection after traversing the whole sequence. And, since the deflecting fields are transverse to the motion, we see from the transverse-field term of Equation 5 that the spin rotation produced by a given transverse field is essentially independent of the energy, so that the fields necessary for the rotation are constant.

However, the transverse fields, in addition to rotating the spin, also deflect the orbit. Within the rotating magnet sequence, the orbit moves up and down and sideways in a sinuous or snake-like manner. Therefore, I dubbed this device a “Siberian snake” (40), and that name has stuck.

Siberian snakes promised to eliminate depolarizing resonance—so now there seemed to be no obstacle to going to high-energy polarized beams!

But . . .

The magnets needed for a snake to rotate the spin by 180° add up to about 30 Tesla-meters. Even with 4-T superconducting magnets, a snake would have to be at least 7 m long. The straight sections of the Brookhaven AGS and the CERN PS were at most 3 m long; thus, snakes would not be practical for these machines. Only the much larger machines of the future might have room for snakes. Also the beam deflection within the snake, and therefore the aperture requirement, is large at low energy (the angle of deflection is $1/\gamma G$ times the angle of spin precession). So, although Siberian snakes were first proposed around 1977, we were to wait 20 years before full-fledged snakes were actually built.

Even though Siberian snakes did not promise to be useful immediately, they made it practical to consider polarized beams as features of the largest new proposed machines. Studies on how they might be built and used continued. A variety of optimized configurations were devised over the years by various people, including myself, Klaus Steffen of DESY in Germany, S. Y. Lee, David Underwood, and others.

It soon became evident that if one snake is good, two are better. With two snakes, one rotating the spin about a longitudinal axis and one about a radial axis, the overall spin tune is still $\frac{1}{2}$, so that resonances are avoided; the advantage is that now the spin is vertical (alternately up and down) everywhere in the arcs, whereas with a single snake the spin would be in the horizontal plane, rotating many (γG) times per particle revolution. And for a really large ring, it appeared that even more snakes would be even better because the perturbations between snakes in one sector would tend to cancel out those in the next sector, where the spin direction was opposite (41). So we incorporated 26 “snake pits” in the huge SSC ring layout.

In 1989, Alan Krisch and his group wondered how the Siberian snake concept could be tested. (I was then, and continue to be, an adjunct professor in Krisch’s group in the University of Michigan physics department.) We were aware that

the Indiana University Cyclotron Facility had a large “cooler ring” that stored protons of 100–200 MeV in a storage ring with long straight sections. An old superconducting solenoid magnet was installed in that ring and energized so as to produce 180° spin flip at 108 MeV ($\gamma G = 2$). The resonance was indeed suppressed (42), confirming the theory of Siberian snakes as a cure for depolarizing resonances. (Strictly speaking, the solenoid should not be called a snake, since it does not entail snake-like transverse motion like the snakes with transverse fields; however, the term is generally used to include solenoids used as snakes.)

Work on spin manipulation at IUCF continued until last year, when the facility was closed.

5.3. Polarized Protons at Brookhaven

In 1977, the ZGS accelerator at Argonne was shut down. It was the world’s only accelerator producing polarized protons in the 12-GeV energy range, so the people involved with high-energy polarized beams were left high and dry. Alan Krisch, the leader of the ZGS polarized experimental program, and others pushed a program to accelerate polarized protons at the AGS. The task was complicated by the fact that, as we have seen, Siberian snakes were not feasible at a machine like the AGS, and the depolarizing resonances in the strong-focusing AGS were expected to be considerably stronger than at the weak-focusing ZGS. I had worked out a computer program called DEPOL for calculating the strength of the depolarizing resonances using the detailed orbit properties of a given lattice (43). Figure 20 shows the strength of the resonances in the AGS, with rms magnet misalignment of about 0.5 mm, as calculated by this program.

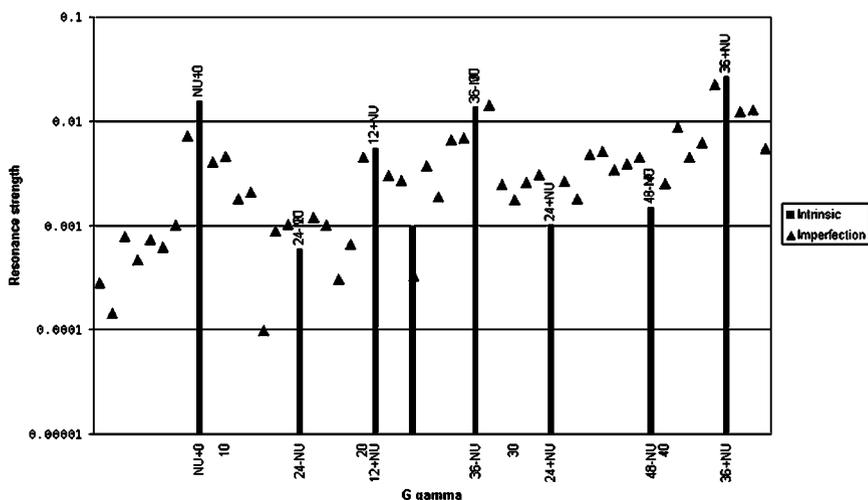


Figure 20 Depolarizing resonances, intrinsic and imperfection, in the AGS.

It was decided to proceed with the effort to accelerate polarized protons at Brookhaven. One of the key accelerator physicists who had made the polarized ZGS work, Larry Ratner, joined the Brookhaven staff. He, Alan Krisch, and many others undertook to tame these resonances. For the intrinsic ones, they installed a set of pulsed quadrupoles that turned on in a few microseconds during the acceleration cycle whenever γG approached a resonance (Equation 8), and rapidly increased or decreased v_z to jump across the resonance; the quadrupoles were then turned off until the next resonance was approached. As for imperfections, at every imperfection resonance $\gamma G = k$, correction windings were energized to control the k -th harmonic of the perturbing field. The process was unbelievably time-consuming, painstaking, and elaborate, but it worked. Polarized protons up to ~ 22 GeV were obtained (44). Later, as described elsewhere (33), a “5% partial Siberian snake,” i.e., a solenoid rotating the spin by 9° , was inserted. This makes all imperfection resonances strong enough for almost 100% spin flip (see Equation 9), so that polarization is preserved. Intrinsic resonances are now tamed by a related technique, developed by Bai (45): As the resonance is approached, excite coherent betatron oscillations with an rf dipole so as to make the resonance strong enough for 100% spin flip, then de-excite the oscillations.

When the RHIC program got under way, some of us started to think about incorporating polarized protons into it. Some people at the DOE were opposed to protons in RHIC—“This would be a back-door way of reviving ISABELLE!” But studies got started.

What kind of snakes should be used? The depolarizing resonances, especially at high energy, would be stronger than in the AGS (Figure 21). But according to the estimates of Reference 41, two snakes should be adequate to cope with them. The RHIC lattice had room for snakes up to 12 m long; this would be adequate for snakes made up of superconducting magnets with 4-T fields. Many people worked out snake configurations with strings of horizontally and vertically deflecting magnets so as to minimize the orbit excursions. It turned out that most configurations would require orbit excursions within the snake magnets of the order of 5–10 cm at an injection energy of 25 GeV; this was uncomfortably large.

I had been intrigued by the idea of helical wigglers for synchrotron-light machines, and looked to see what such configurations would do to spin. I found that having the transverse deflecting field undulate continuously between horizontal and vertical could rotate the spin with considerably smaller transverse orbit deflection than the discrete jumps of the conventional snake (46). But some conventional deflecting magnets would still have to be added to bring the net orbit deflection to zero.

A number of hybrid configurations were devised, combining helical and conventional deflecting magnets. But none caught on, until Ptitsyn & Shatunov (47) (again two Siberians coming to the rescue!) observed that with four helical magnets, two 4-T and two less, each twisting the field through a full 360° , one could achieve 180° spin rotation about any desired axis, with zero net deflection and only

Intrinsic resonance strengths, protons in RHIC

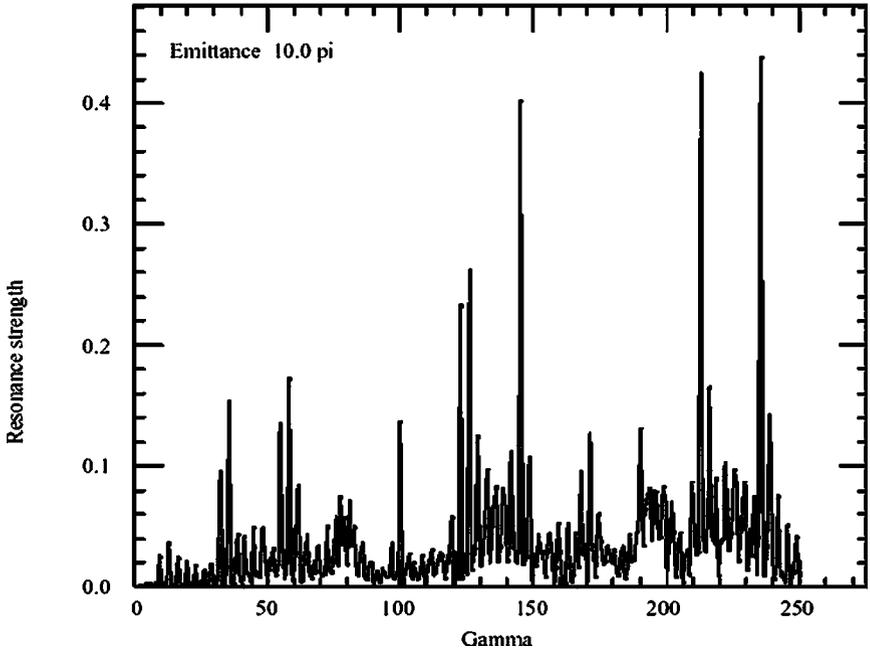


Figure 21 Intrinsic depolarizing resonances in RHIC.

3 cm maximum orbit excursion within the snake. (Ptitsyn is now at Brookhaven; Shatunov is a frequent visitor.)

Our team of magnet experts at Brookhaven managed to design, build, and install these helical magnets, and at the end of 2001, polarized protons in the two counter-rotating beams of RHIC were accelerated to 100 GeV and collided. The polarization appeared to be preserved during the acceleration process (see Reference 33 for details). So, 25 years after Siberian snakes were first proposed, they achieved their purpose. Figure 22, which also appears in Reference 33, shows the layout of the AGS-RHIC complex.

In the future, the RHIC proton energy will be increased to 250 GeV. The Siberian snakes will also work with polarized ^3He ions. These would essentially make it possible to study the behavior of polarized neutrons as well as protons.

6. CONCLUSIONS

I have neglected to mention my teaching activities. At several stages, I spent time teaching at universities. I took a leave to teach at Princeton in 1950–1951. In 1956, I spent some months at Cambridge University on a Fulbright grant. In 1961–1967

I taught part time at Yale, where, as mentioned above, I became interested in spin problems. In 1967, C.N. Yang invited me to join the Institute of Theoretical Physics at Stony Brook, on a part-time basis, because he felt accelerator physics deserved a place in the University. I taught courses in accelerator physics, as well as mechanics and electrodynamics. This brought several students into accelerator physics, notably Alex Chao and my PhD students Ron Ruth, Steve Tepikian, and Jie Wei.

I retired in 1990 (though not completely). For over 50 years, I have been fortunate enough to be a part of the exciting evolution of high-energy physics and to make some contributions. I am grateful to Brookhaven National Laboratory for making this possible. The following quotation is a from an after-dinner speech my wife gave at my retirement dinner in 1990:

Brookhaven deserves our sincere appreciation for recognizing that Ernest is a rare bird without obvious distinguishing features. It is wonderful that . . . Brookhaven let him alone to do his work. No one attempted to mold him into a division chief or department head. He has remained uncompromised and was allowed almost complete freedom to follow his own instincts and to work unhampered by petty concerns. Brookhaven was and continues to be a happy place for him.

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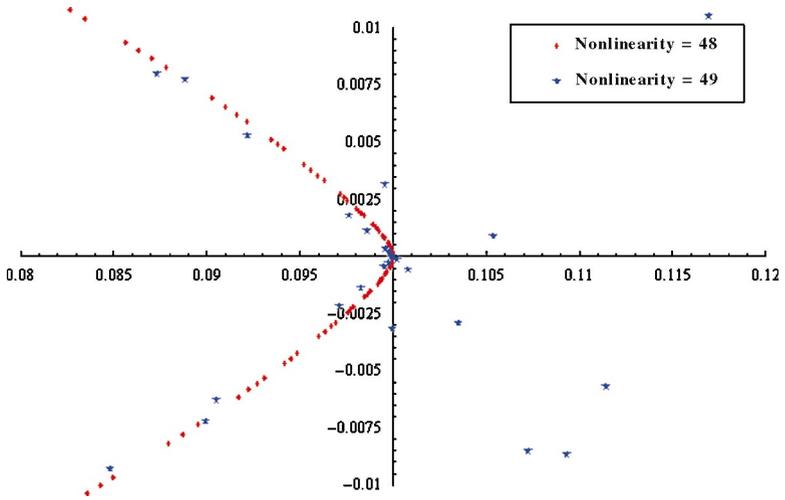


Figure 10 Phase plots for a simplified alternating-gradient lattice with nonlinearity, showing smooth curve for nonlinear coefficient $\alpha = 48$ (red) and chaotic behavior for $\alpha = 49$ (blue).

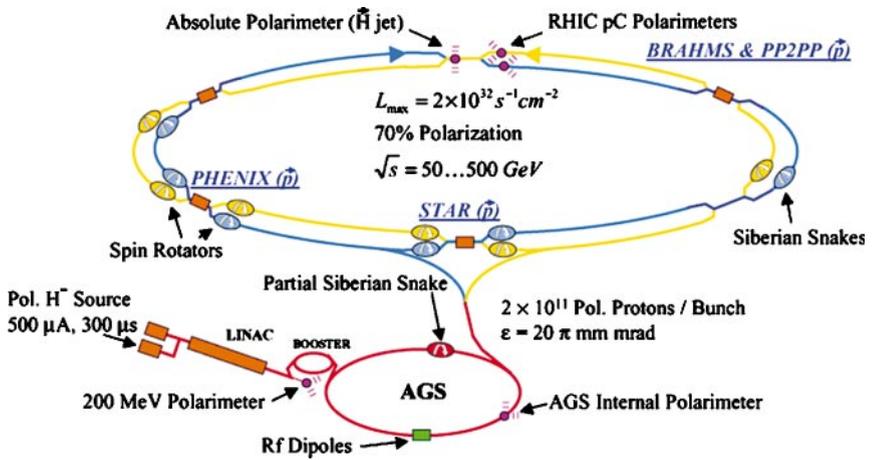


Figure 22 Layout of the Brookhaven accelerator complex with expected performance goals.