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EARLY HISTORY OF THE COSMOTRON AND AGS AT BROOKHAVEN*

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In 1946 Brookhaven National Laboratory was established by the Manhattan Project - soon to be transformed into the Atomic Energy Commission - as the first new laboratory to be devoted exclusively to peacetime research; its mission was especially to make large research facilities available in the Northeastern part of the country. It was intended to focus on facilities too large to be supported by any university, specifically:

- (1) A nuclear reactor for research with neutrons;
- (2) A 700 MeV synchrocyclotron;
- (3) A particle accelerator to reach or surpass the then unheard-of energy of a billion electron volts.

The reactor was, I think, the first reactor built purely for scientific research purposes; it was decommissioned some years ago and replaced by a more modern one which is still operating.

The cyclotron was given up after a year or so of design work; instead the Nevis synchrocyclotron (somewhat smaller) was built by Columbia University. As for the proton synchrotron: McMillan and Veksler had independently discovered the principle of phase stability, which was first applied to the

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synchrocyclotron and the electron ring synchrotron. But for really high energy the cyclotron would become too massive to be attractive, since it requires the entire interior of the top energy orbit to be filled with magnetic field. The electron synchrotron only requires magnetic field at the outer radius, and thus is more economical; but the phenomenon now known as synchrotron radiation makes for very large power requirements at high energy, and seemed to limit the energy to some 300 MeV (a rather elastic limit, which has since increased considerably, but really only by simply applying massive radiofrequency power). The best way of getting to the highest conceivable particle energies was then, and is still, the use of protons.

In a proton synchrotron, unlike the electron synchrotron, the radiofrequency has to be modulated with high precision so as to track the magnetic field and at the same time keep the orbit at constant radius. But this complication is outweighed by not having to worry about synchrotron radiation.

Several proposals for proton synchrotrons appeared, notably one for a 1 BeV machine at Birmingham, England (Oliphant, Gooden and Hide)¹ and one for a 10 BeV machine by Brobeck at Berkeley.² Discussions between Leland Haworth at Brookhaven, Ernest Lawrence at Berkeley, and the AEC authorities led to the decision that both Brookhaven and Berkeley, instead of competing for the 10-BeV prize, would each build a smaller proton synchrotron, one around 3 BeV and one at 6. Haworth chose the smaller size with the hope of getting it finished faster; in later years he often said that this was the best decision he had ever made. You will surely hear more about the Bevatron, the 6 BeV machine at Berkeley, from other speakers.

An accelerator project was set up at Brookhaven under M.S. Livingston. In the spring of 1947 he, P.M. Morse (director of Brookhaven) and R.A. Patterson

(personnel director) visited Cornell, where I was a post-doc under H.A. Bethe, and invited me to Brookhaven for the summer. The next year I joined for good. 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 machines (named the Cosmotron).

Two important problems we tackled were: (1) The beam blowup and loss due to scattering by the gas in the vacuum chamber. We worked out criteria for this and concluded that a vacuum of 10^{-6} mm was desirable if not altogether necessary.³ (2) 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. non-circular orbits.⁴ (Dennison and Berlin⁵ had tackled a similar problem at Michigan; I think 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) that the amplitudes of oscillations are modulated, and (c) that 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 be avoided.

The more practical people worked hard on the magnets, vacuum systems, rf etc., and by the spring of 1952 the machine was finished. On May 20, 1952 a beam was injected into the Cosmotron and accelerated to 1.3 BeV - by far the highest energy in the world ever attained by artificial acceleration. Soon we

surpassed that record and got to 2.3 BeV in June, and to 3 BeV (the design energy) the next year, when pole-face correcting windings were installed.

Soon important physics was done with this new facility. I shall only mention the verification of the Pais - Gell-Mann hypothesis of associated production of hyperons, which was accomplished by Fowler, Shutt, Thorndike and Whittemore in 1953⁶ and was described by Fowler at this Symposium.

In the meantime; CERN was being formed in Europe. A delegation of Europeans (Dahl, Goward and Wideröe) was expected to visit us to see whether they could pick up some good pointers from us - they were planning, as the centerpiece of their new international laboratory, to build a proton synchrotron even bigger than the Bevatron, around 10 GeV.

As I recall, Livingston set up a study group especially to enable us to tell them not only what we had done but also what one might do better. One problem that bothered him was: The magnets of the Cosmotron all faced outward; therefore negative secondary beams are easily obtained, but positive secondaries will 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 would 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 have a drawback: The focusing gradients might easily be different in the inward and outward sectors, especially in the fringing fields. Because of my earlier work 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 saw at once that this was really something fundamentally new, and that the focusing could be pushed to make it much stronger so as to make it possible to make the magnet aperture really small. That, in turn, makes the magnets - and other components - much cheaper, and so one can go to higher energies than without "strong" focusing. We published a design with 1 inch aperture for 30 GeV.⁷ Snyder explained the new results in terms of optical principles, and we and Blewett⁸ saw that the same principle can be used without bending magnets - e.g. with just quadrupoles - for focusing of beam lines, and to replace the grids that were then though necessary in proton linear accelerators. So we had something to tell our European visitors when they came!

Two difficulties soon became apparent: (a) Imperfections of the magnets, differences between supposedly equal units, could lead to resonant beam blowup. This was pointed out especially by Adams, Hine and Lawson⁹ in England, and for a while led to great pessimism. But we soon saw that resonances could be avoided by staying between them - albeit at the cost of tightened tolerances and somewhat enlarged aperture allowances. (b) The transition energy, i.e. the change in the position of phase stability first found as an academic curiosity in the studies on straight sections, now came right in the middle of the interesting energy range. Fortunately we saw right away - thanks to the 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 point. 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. It turned out that an engineer named Nicholas Christofilos in Athens had thought up essentially the same scheme after reading about plans for the Bevatron; we soon saw that he deserved full credit¹⁰ - and we hired him at Brookhaven. He worked on the electron analogue (see below) and on linac focusing; later he moved to Livermore to work on fusion and on weapons ideas.

Because of the worries about resonances and about transition energy, we proposed an "electron analogue" accelerator that would also go through transition. (R.R. Wilson built a 1 BeV strong-focusing electron synchrotron at Cornell, but that one did not have the transition problem and therefore could not model it.) The energy of the analogue was just a few MeV rather than GeV; it used electrostatic rather than magnetic guide and focusing fields. It was built in about 18 months, and met our fondest expectations. We saw that the transition problem could be managed just as per theory. It also demonstrated very beautifully that resonances existed, and that low order nonlinear resonances were more important than had been thought - but that nonlinearity could also stabilize beam blowup.¹¹

On September 9, 1953 Haworth formally proposed to the AEC that the AGS be built - including the analogue. The project was approved early in 1954. The formal proposal was a five-page letter (plus a few graphs) rather than the 500-page books customary nowadays; approval took four months rather than four years. In the meantime CERN went ahead with a similar machine. They did not take the detour via the analogue; this may (or may not) be the reason why the CERN PS had a beam in 1959, a year or so before the AGS in 1960. But it was always a

friendly and collaborative rivalry between us and CERN - and remains so to this day.

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