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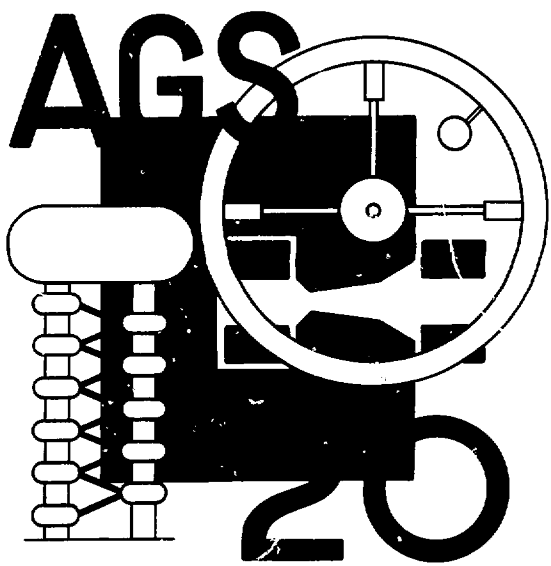

BROOKHAVEN NATIONAL LABORATORY
Associated Universities, Inc.
Upton, Long Island, N.Y. 11973

EDITOR'S FOREWORD

The planning and organization of this celebration was done by John Blewett, Ted Kycia, Vinnie LoDestro, Lyle Smith and Carl Thien, under the general direction of Ronald Rau and with the invaluable assistance of Kit D'Ambrosio. The logo which graces the cover of these symposium proceedings was designed by Per Dahl. The job of transcribing the tapes was done by Anna Kissel, and it was often a challenging one! I am to blame for the editing, which I hope has not distorted history too much. Joyce Ricciardelli has very ably produced the final manuscript and seen it through the complex process of publication. All of us took pleasure and pride in celebrating the AGS and in putting this book together, and we hope you enjoy it.

Preface

On March 17, 1960, a beam was first introduced into the newly constructed Brookhaven Alternating Gradient Synchrotron. On March 26, a hundred turns of circulation were achieved, and on July 29 the beam was first accelerated to the design energy of 30 GeV. Thus, however one defines the exact start of life during the series of steps by which a new accelerator is made operational, the year 1960 marks the start-up of the AGS, and in 1980 we celebrate the twentieth anniversary of that event. The AGS, together with the newly functioning PS at CERN, carried particle physics into a new world of higher energies and unanticipated discoveries. The AGS and the PS both embodied the new principle of strong focusing and demonstrated that, with its aid, a new era of particle accelerators had opened.

Since its start-up the AGS has been modified and upgraded almost continuously, so that today it is a very different and sportier device than the model of 1960. Most notably, in the period 1965 to 1972, it went through substantial changes in the so-called AGS improvement program which supplied it with a new injector, a new magnet power supply, all-external beams, a new experimental hall, and other features. Today it functions better than ever and is supplying particles for up to six simultaneous experiments, with a considerable backlog of new experiments waiting to go on line and further improvements in process of being made. The limitation today is the budget rather than the physical capacity of the machine. To date, the AGS has accelerated about a milligram of protons--more than any other high energy machine in existence. Perhaps its most important role is yet to come, for it is destined, as everyone knows, to serve as the injector to ISABELLE, thus entering a whole new phase of its service around the middle of this decade.

In the past twenty years, an array of important discoveries have been made at the AGS, discoveries which have changed our fundamental conceptions of matter and its interactions. The period in which the work was done was a golden age for particle physics, and because of these successes we have been led to higher energies where it is likely that many more exciting discoveries are yet to come.

On May 22, 1980, a symposium was held at Brookhaven to celebrate the 20th birthday of the AGS, to recall its beginnings, and to review major discoveries that have been made with its beams. The talks at the symposium are recorded in this volume.

In paying tribute to this historic instrument, it is fitting to note that two leaders who did the most to bring it into being have recently passed away. Leland J. Haworth, who was the Director of Brookhaven during the period when the AGS was conceived, built, and first operated, died on March 5, 1979. Leland also headed the project at its beginning and had a direct hand in much that was accomplished, including the winning of approval and funding for the project from the government. G. Kenneth Green was Haworth's deputy in the early days and became his successor as Chairman of the Accelerator Department in 1960. He died on August 15, 1977. Brookhaven and High Energy Physics owe much to these two men.

George H. Vineyard

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AGS EARLY HISTORY

J. P. Blewett

May 22, 1980

To give a decent beginning to the AGS story I have to go back and say a word about the Cosmotron. The group that started the Cosmotron was a raw, inexperienced group picked up, so to speak, off of the street. We made some daring decisions, flying often in the face of recommendations by our experienced competition at the University of California - until then the U. S. Number 1 accelerator lab. So we often lay awake nights. My sister, who came to visit me when the machine was half built, put it in a nutshell. She said "where will you look for a job if it doesn't work?"

But it did work -- here is a happy scene at an early test.

Clockwise from center: Ken Green (with cup), Al Wise, George Collins, Charlie Keenan, Gerry Tape, Stan Livingston, Marty Plotkin, Lyle Smith (partly hidden), Joe Logue and Irv Polk

Soon thereafter we heard of the formation of a new joint European laboratory (CERN) which was to send a delegation to visit us and ask our advice about building in Europe a scaled-up Cosmotron.

This led to the invention of A-G focusing. Most of you have heard the story, and will hear it from Courant, but I'll summarize it for those who have not. Stan Livingston, who built the first cyclotron and was the first Chairman of the Cosmotron Department, was visiting us that summer and made an effort to collect some good ideas for the CERN people. He thought, reasonably, that magnets could be run to higher average fields if some sections had back legs inside and some outside. There would be high alternating gradients as the magnets saturated and Stan asked Ernest Courant to see if this would damage the orbits. Ernest found, to his surprise, that it seemed to improve the focusing. Hartland Snyder recognized an analogy with optics where alternate focusing and defocusing lenses of equal strengths are focusing, no matter which comes first. Thus was AG focusing invented.

All unknown to us, these three had been preceded by a Greek elevator engineer in Athens who, for fun, read the Physical Review in the American Library and spent his spare time inventing accelerators. He visited the U.S. at the end of 1952, dropped in at the New York Library for a look at the latest Physical Review and saw Courant, Livingston and Snyder's paper. Since he had thought up essentially the same idea two years earlier he thought his idea had been stolen and he came out to Brookhaven to tell us so. At first we thought he was a phony nut but facts gradually emerged to support his claim. We speedily changed our mind about him and offered him a job which, later, he accepted. Nick spent a few years here and made many valuable contributions. His was the first calculation of linac drift tube configurations. To do this he had to learn for the first time about Bessel functions, which he did with an enthusiasm that I still remember. While here, he invented a new scheme for a fusion reactor -- the Astron -- and he left us to build a model at Livermore in California.

1) Ernest Courant before he retired behind a beard.
2) Stan Livingston, now retired and living in Santa Fe.
3) Hartland Snyder - formerly a student of Robert Oppenheimer.
4) Nick Christofilos

Shortly after the AG focusing invention a delegation from the CERN group paid us a visit.

Left to right: George Collins, then Chairman of the Cosmotron Department, Odd Dahl, Rolf Wideroe, and Frank Goward.

Odd Dahl from Norway was to be head of CERN's proton synchrotron group. He has a long list of achievements including flying airplanes for Amundsen at the North Pole and building Norway's first nuclear reactor. He also is famous for being Per Dahl's father. Frank Goward from England, who built the first working synchrotron, was to be Dahl's deputy. Rolf Wideroe who worked for Brown Boveri in Zurich came along on his own. He could be considered to be the founder of the accelerator art, having built the first working linear accelerator in 1928.

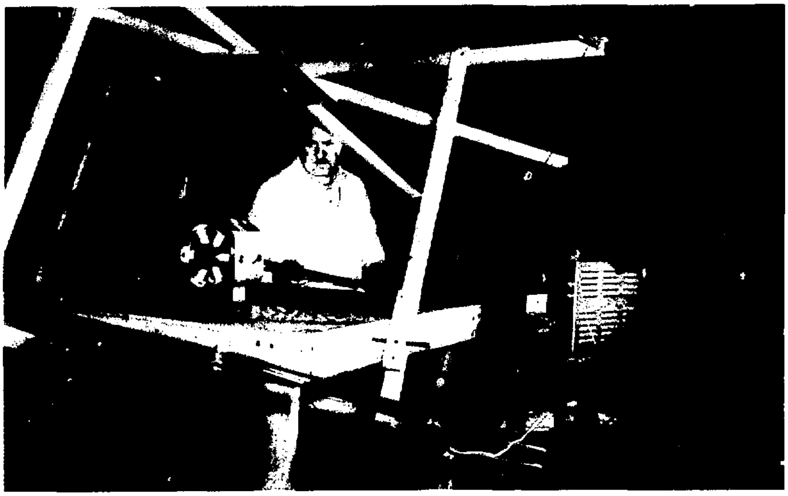
The CERN group heard of our new discovery with great enthusiasm and immediately scrapped their schemes for scaling up the

Cosmotron. Also they invited several of us to come to Europe and help them to get started. My wife Hildred and I accepted and spent a pleasant eight months partly in Norway with Dahl and partly in Geneva where the proton synchrotron group moved in September of 1953.

The CERN group was a small, but brilliant, collection of stars from England, France, Switzerland, Germany and Norway. One of our major contributions to CERN was to persuade some Englishmen to move to Geneva -- they felt that in leaving England they were leaving civilization. One of them is now CERN's Director General and presides over one of the most successful international efforts ever undertaken.

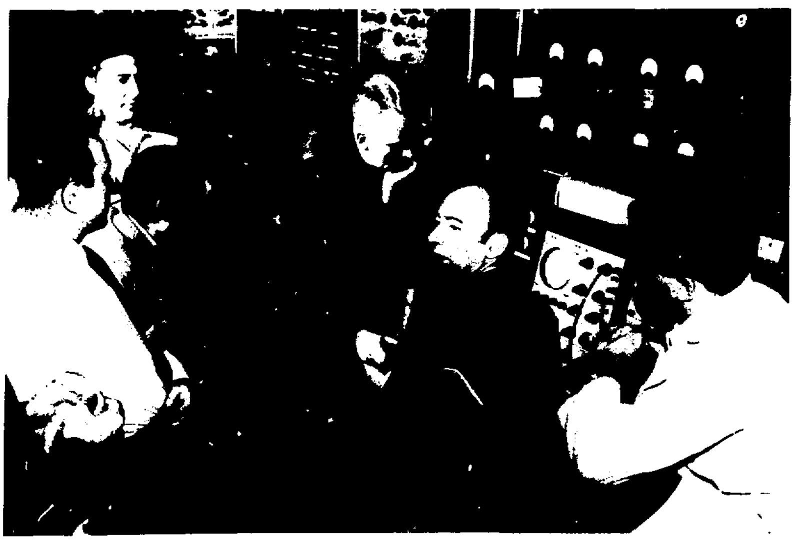
The CERN Council was a little skeptical about the new ideas and decided that the PS group -- the proton synchrotron group -- should have a public examination. This came off late in '53 with invitees from accelerator groups all over the U.S. Stan Livingston and Ernest Courant came over and we took them for a drive in the Alps. Stan, after his first look at Mt. Blanc, commented, "Mt. Blanc would really make Long Island."

No sooner had we turned our back on Brookhaven to go to CERN than the local accelerator development group decided that a model test was needed to demonstrate that one could pass through a discontinuous phase shift that had to happen in most AG focused machines at an energy of a few GeV. I took a very dim view of this project. The theory said it could be done and I believed the theory. But it seemed to be a political necessity, and the project went ahead. At Lee Haworth's suggestion the AG focusing was electrostatic. The accelerated particles were to be electrons and the device was called "The Electron Analog," and is quite beautifully described by Plotkin in the Brookhaven Bulletin of May 6. It was built in a huge wooden barn back of the Cosmotron building -- a barn known as "the test shack" because the Cosmotron magnet blocks were tested there.

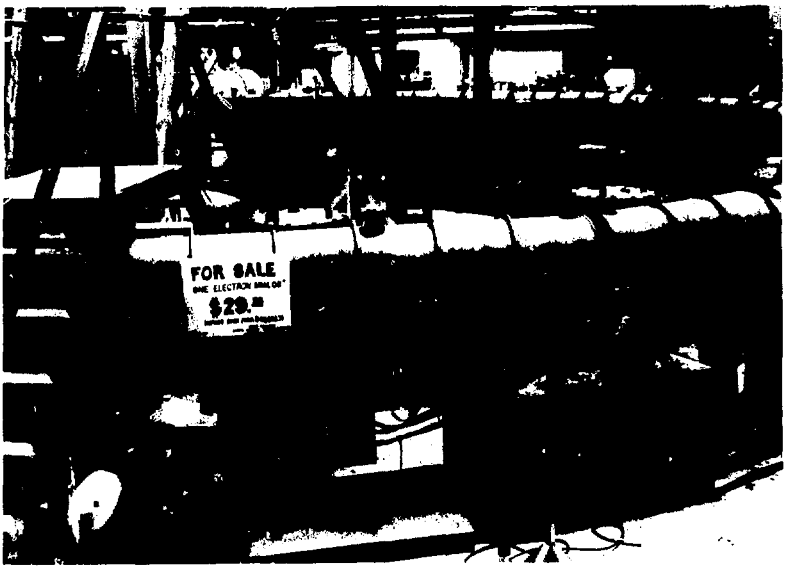


Analog Lens

The Analog, after some trouble with eliminating ferromagnetic materials, finally worked just as the theory said it would.



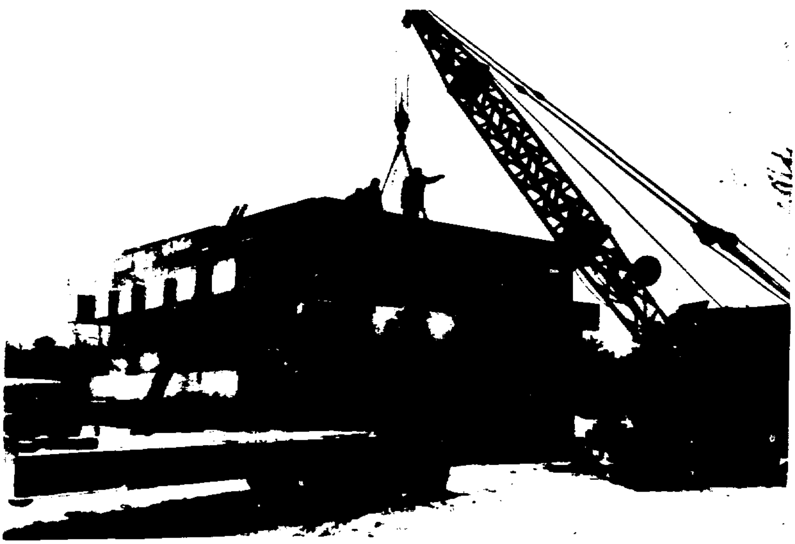
Left to right: Gary Cottingham, Julie Spiro, Marty Plotkin, Nick Christofilos, Hartland Snyder, Ken Green, Gene Raka.



The Analog after successful tests. The sign explains that the low sale price also includes one physicist, slightly used.

In the meantime work was proceeding on the AGS. People were confident enough that the Analog would work that we hired a good Architect/Engineering firm -- Stone and Webster -- to design our buildings and went ahead with component design. With this team Jack Lancaster supervised building construction and soil tests. We were much concerned about the stability of the ground on which the machine was to be built because the theory predicted rather close mechanical tolerances that would have to be met for the machine to work. We dug holes to see what was under the surface sand and found...

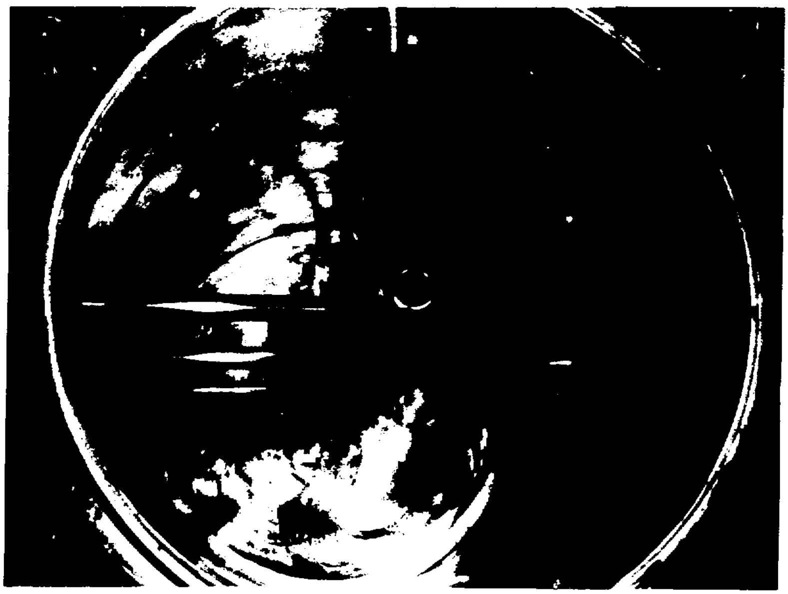
mostly just more sand, but occasionally there were sheets of clay. We did a soil loading test where we piled several hundred tons of Cosmotron concrete shield blocks and measured how much the earth sank, then unloaded the ground and then piled the blocks on again.



Finally we decided to be extra safe and support the magnets and the linac injector on steel I-beam piles driven 50 feet into the ground. It worked very well but now we are more relaxed and probably we would not do that again.

Hildred presided over the design of the AGS magnets and Cal Lasky was in charge of their manufacture. With a team of inspectors he spent most of his life at the factory measuring, revising welding procedures and instructing the factory staff. Similar efforts went into the magnet coils. Finally they began arriving. All were stacked on the floor of the new target building and were carefully measured, then distributed around the ring in such a fashion as to minimize the effects of their small deviations from mean values of such parameters as remanent field.

We went through tests of several novel types of linac. Some were really quite ingenious but finally we went back to a sophisticated version of the Berkeley linac built by Luis Alvarez. Beside his drift-tube calculations, Nick Christofilos made many other contributions to the linac design. We decided to build a three drift-tube-model of drift tubes appropriate for use at about the 30-MeV point. Gary Cottingham presided over this operation.

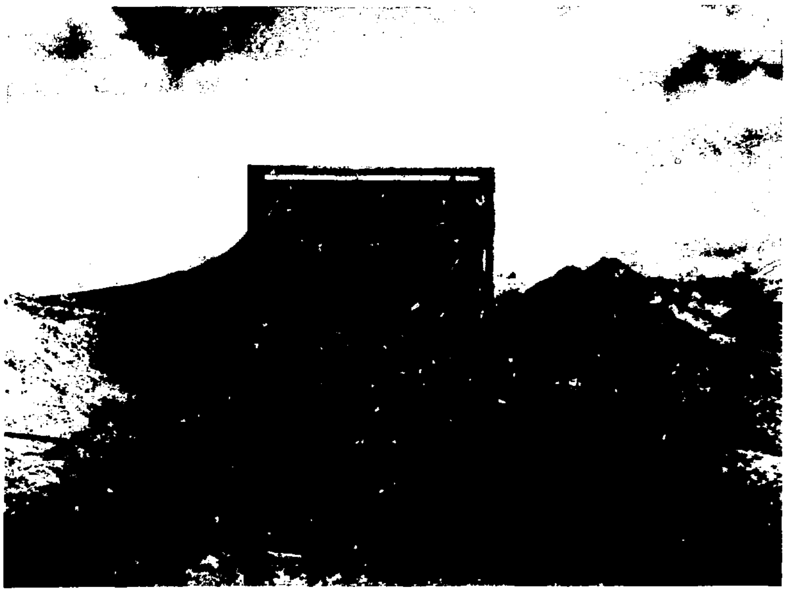


Three drift tube Linac model.

We estimated that about 125 kilowatts would power it and we set it up on the second floor of one of the seven barracks buildings that housed the AGS project. It had a pulsed power supply and various measuring devices that told us when it had reached the design rf accelerating field. As I remember there were supposed to be about 700,000 volts between drift tubes. Unfortunately we didn't then appreciate the need for extreme cleanliness in high powered rf systems and we left some films of machining oil inside the drift tubes. This oil spread itself around on the surfaces of the drift tubes in thin film which, under high fields, emitted electrons in copious quantities and, in turn, generated lots of 700,000 volt X-rays. Finally it took about 500 kilowatts to bring the model up to full field. Of this, 375 kilowatts were going into X-rays and it wasn't safe to be anywhere in the building!

We also did a good deal of work on permanent magnet quadrupoles for the linac. We discharged a big condenser bank into a stepdown very high current transformer which powered a four turn magnetizing coil inside the ferrite rings that we hoped to make into permanent quadrupoles. After the magnetizer blew up several times, blasting pieces of ferrite through the walls of the barracks, we finally resorted to wrapping it tightly with piano wire. The permanent magnet idea was a good one and is being resurrected at Los Alamos and elsewhere, but we lost our nerve finally and installed pulsed electromagnet quads capable of having their fields varied from outside.

So we plodded on and on. The piles were driven and the ring tunnel was constructed.



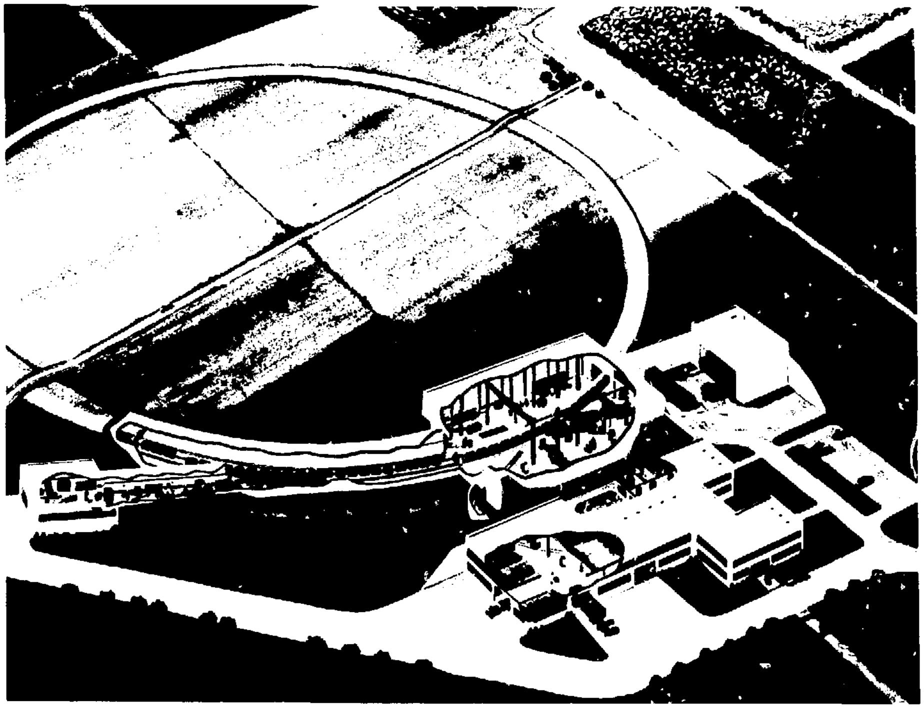
Finally, in May of 1960 we were ready for a first test. The linac, after heroic pressure from Sal Giordano, Frank Toth and Vinnie Racaniello, had produced a 50-MeV proton beam, the magnets were in place and aligned precisely, the magnet power supply had been tested on magnet pulses. Enough controls were installed for a first test. The rf system was not yet quite under control so the first test was to be injection into the ring hoping that, as the magnetic field increased, the beam would make a number of revolutions spiraling gradually inward and finally being intercepted by the inner wall of the vacuum chamber.

Happily, after some adjustment, that is exactly what happened.

Happy operators after spiralling beam test. Left to right: Arie vanSteenbergen, John Blewett, Ralph Kassner, Ken Green, Frank Toth and Irv Polk.

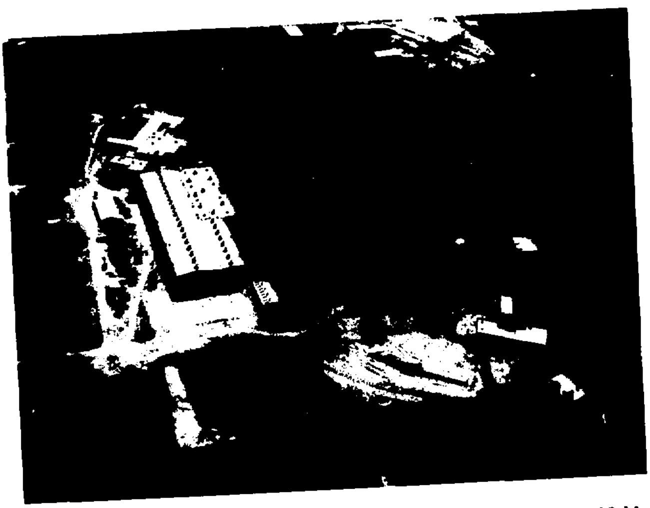
A couple of months later the rf was in operation and, in July, the beam was taken through the dreaded phase transition with no difficulty and accelerated to 30 GeV.

Even happier operators after first 30 GeV operation. Left to right: Ralph Kassner, John Blewett, Julie Spiro, Ken Green, Henry Halama, Eric Boerner and Ray Rheaume. (Lee Haworth behind Ken).



The AGS as planned in 1954.

Under construction in 1957.



As it appeared in 1961. Note the 80" bubble chamber building, lower right.

Since the first operation we have continually cast about for possible new accelerator projects. Already in 1961 we ran a design study on accelerators for 300-1000 GeV range. This was to be a collaboration with the Russians, but they failed to show up for a discussion of the project and it came to nothing. We took a leading position in the competition for the "200 GeV" accelerator in the late sixties. That was a real scramble. Some 200 sites were offered all over the U.S. -- many completely unsuitable. I was invited to be a consultant to the State of Louisiana which was offering a site on silt brought down by the Mississippi River. Life magazine published a cogent cartoon.

We were one of six sites finally chosen by a site committee that worked long and hard to eliminate all of the other sites. They were California, Denver, Madison (Wisconsin), Chicago, Ann Arbor (Michigan) and BNL. Evidently we were outnumbered by the Middle West and eventually that was where it went -- to what is now Fermilab in Batavia, Illinois.

So we bravely bit our lip and went back to work -- laying out pictures of 2000 GeV accelerators on the Brookhaven site with experimental beams crossing 40 ft under route 25 to reach experimental areas on what was our North Tract before President Nixon gave it away. Also we did a decade and a half of pioneering on superconducting magnets, and on storage rings in general.

This work has finally paid off and we are in the midst of two fine major projects -- the National Synchrotron Light Source and ISABELLE. I take great pleasure in the fact that I have played a part in the initiation of both projects and I am confident that under the capable direction of Arie vanSteenbergen and Jim Sanford both will be splendidly successful. Also in the fact that I have worked with many great men -- physicists and engineers -- for example, Stan Livingston, Dave Jacobus and Nick Christofilos -- there have been very many others.

Finally I should like to salute two of the finest people with whom I have ever been associated.

Ken Green and Lee Haworth

Some Recollections on the Early History of Strong Focusing

Ernest D. Courant

The AGS had its genesis in a study group we had here in the summer of 1952, when M.S. Livingston, H.S. Snyder, J.P. Blewett and I considered what one might do differently if one designed an accelerator like the Cosmotron over again.

How did this come about?

In the summer of 1947, I had come here at Stan Livingston's invitation to work on the project for the first billion-volt accelerator, the Cosmotron, and in 1948 I joined the project for good. My particular task was to analyze the properties of proton orbits in the machine.

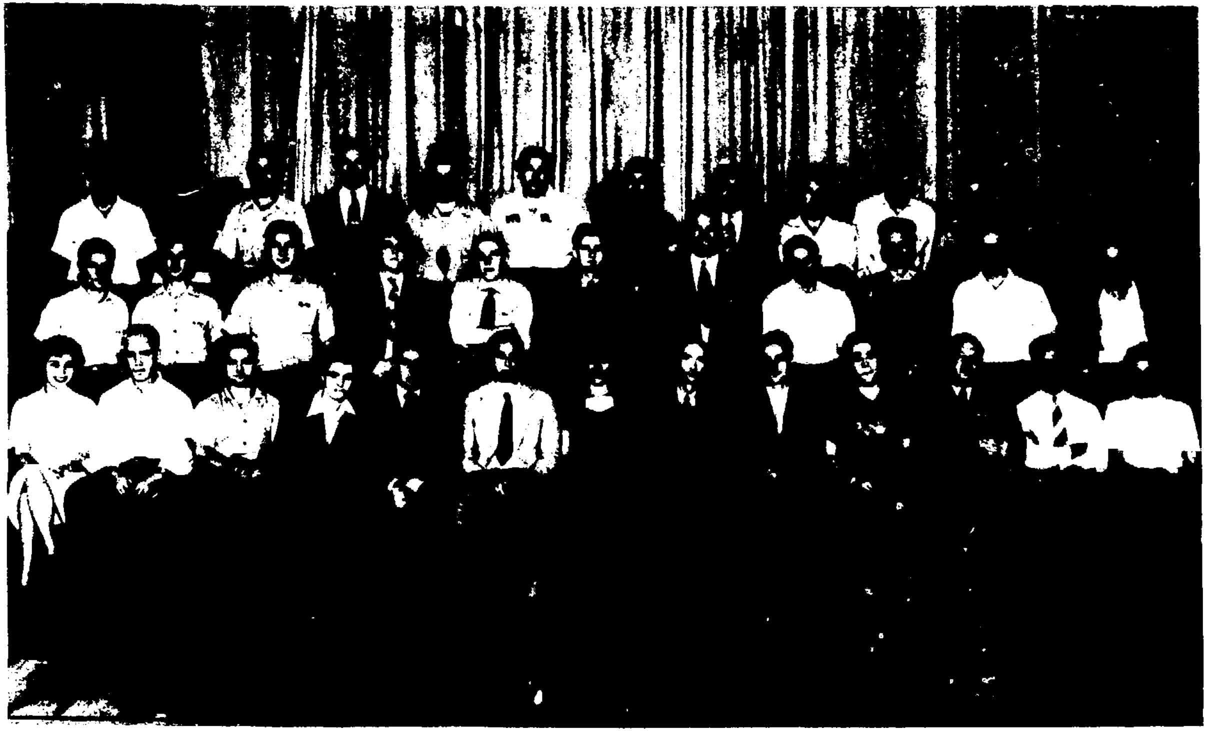
In circular accelerators the particles have to go around a circle many times, and stay on or near the right orbit. About 1931 Lawrence and Livingston had started the cyclotron. They found that to ensure vertical focusing, the field had to decrease with increasing radius. Unfortunately this leads to a loss of synchronism and limits the number of times the particles can go around. Veksler and McMillan had shown how synchronism can be maintained anyway using the "synchrotron" or "phase stability" principle.

As for focusing forces, Kerst and Serber[1] showed in 1941 that vertical and horizontal focusing were antithetical: if the vertical focusing from decreasing field is made too strong, horizontal stability disappears:

$$\nu_x = \sqrt{1-\nu_z^2}$$

The Cosmotron - and its sister accelerator, the Bevatron at Berkeley - differed from earlier synchrotrons in that there were straight sections between the magnet sectors. What difference did this make to the stability problem?

A similar "racetrack" configuration had been suggested by H.R. Crane[2] at Michigan, and the stability problem was first



Cosmotron group, Sept. 1948

analyzed by Bob Serber[3], David Dennison and Ted Berlin.[4] Here at Brookhaven I set out to investigate this more fully, together with another young theorist, Nelson Blachman[5] (who is now with GTE in California, working on communications theory). We found that adding straight sections would do three things:

Change the focusing frequencies ν_x and ν_z, and therefore affect resonances with field errors.

Produce a modulation in the oscillation amplitude, i.e. the amplitudes would be different in different parts of the machine.

Affect the mechanism of phase stability: If the straight sections were too long, the stable phase of the accelerating voltage would become unstable at a certain "transition" energy, but at that point another phase would become stable.

We found that, with the relatively short straight sections at the Cosmotron (or Bevatron) none of these effects would be serious. In particular the transition energy would not occur. But we had developed the matrix algebraic formalism for handling variations of the fields along the orbit.

Came the summer of 1952. We had succeeded in building the Cosmotron, the world's first accelerator above one billion volts. We heard that a group of European countries were contemplating a new high-energy physics lab with a Cosmotron-like accelerator (only bigger) as its centerpiece, and that some physicists would come to visit us to learn more about the Cosmotron. They were Edouard Regenstreif, Frank Goward (who had built the world's first electron synchrotron in England), Odd Dahl, and Rolf Wideroe, who had in 1928 originated the whole concept of resonant RF acceleration.

To prepare for their visit, Livingston organized a study group to consider what advice we should give them: if we were to build a bigger and better Cosmotron, what would we do differently?

Stan suggested one particular improvement: In the Cosmotron the magnets all faced outward. This made it easy to get negative secondary beams from a target in the machine, but much harder to get positive ones. Why not have some magnets face inward so that

M. S. Livingston

H. S. Snyder

J. P. Blewett

E. D. Courant

Soon thereafter we heard of the formation of a new joint European laboratory (CERN) which was to send a delegation to visit us and ask our advice about building in Europe a scaled-up Cosmotron.

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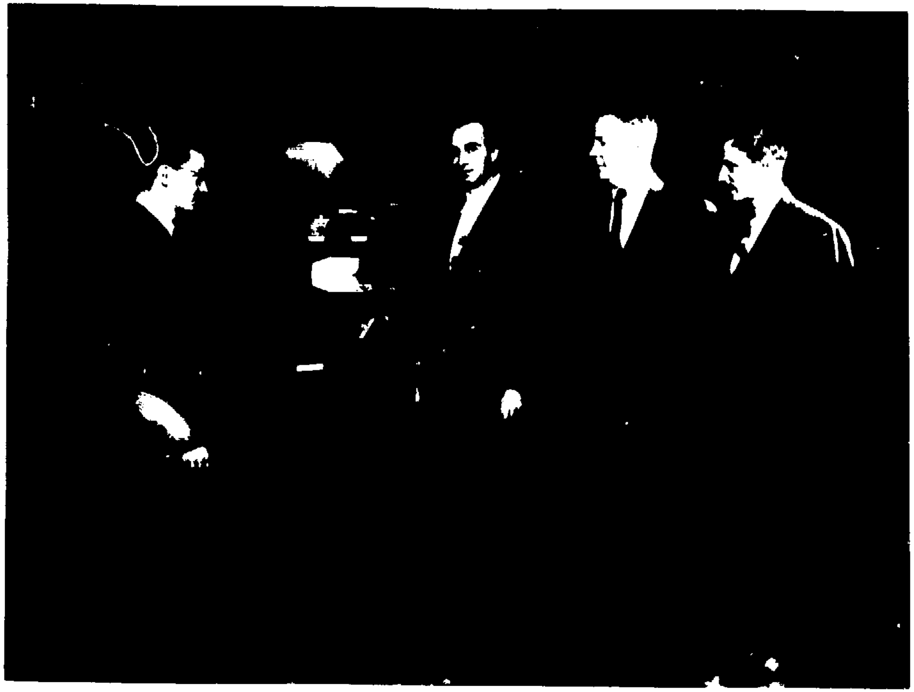
positive secondaries could have a clear path to experimental apparatus inside the ring?

I had one misgiving: As the magnetic field saturates, the field shape (and the index n which governs focusing) deteriorates; this change would now alternate instead of being uniform. Would this limit stability more severely?

Because of my earlier work with Nelson Blachman on straight sections, I knew how to do this calculation with the matrix algebra method. I did the calculation and found to my surprise that the focusing would be strengthened simultaneously for both vertical and horizontal motion. In the constant gradient case, if vertical focusing was strengthened only a little by increasing the gradient, horizontal stability would disappear; now one could make both kinds of focusing strong at the same time. Soon we tried to make the gradients stronger and saw that there was no theoretical limit -- provided the alterations were made more frequent as the gradient went up. Thus it seemed that apertures could be made as small as one or two inches -- against 8x24 inches in the Cosmotron, 12x48 in the Bevatron, and even bigger in higher energy machines as we then imagined them. With these slimmer magnets, it seemed one could now afford to string them out over much bigger circles, and thus go to 30 or even 100 billion volts.

Hartland Snyder explained the new effect in terms of optics: A sequence of alternating focusing the defocusing lenses of equal strength produces a net focusing affect. This way of looking at it led to the invention of quadrupole lenses: just leave out the bending field and retain the gradient. John Blewett then saw that quadrupoles could solve a major problem for linear accelerators: Previously the beam in proton linacs had had to be focused by grids which necessarily absorbed some of the beam, limited intensity, and caused a lot of radioactivity; now with quadrupoles in the drift tube one could have a clean and efficient linac.[6]

But another problem appeared: the "transition energy", which Blachman and I had discovered in the straight section theory,



Courant, Livingston, Snyder and Blewett comparing size of scale models of Cosmotron and "strong focusing" magnets.

reappeared just in the middle of any reasonable acceleration ..ge; furthermore the acceleration frequency would have to be fantastically accurate. Fortunately the old calculations showed that, as the transition energy is approached, the beam tends to bunch sharply. Therefore if the tolerance problem is taken care of by a feedback system, the phase of the accelerating rf field can be switched at the transition energy to the new stable phase, and it would be possible to continue the acceleration process. All this was included in the paper by Livingston, Snyder and myself which we sent to the Physical Review.[7]

The European visitors were astounded and delighted when they came in the middle of all this, and went home to start working on a real design for 25 to 30 GeV instead of the 10 they had counted on — and we started to plan for the same thing here. A friendly competition ensued, and the Europeans won the race when the CPS worked about a year before the AGS here.

But it was not all smooth sailing. Adams, Hine and Lawson[8] in England asked — what if the magnets are not perfect? They found the new scheme very sensitive to magnet errors — the orbit deviations due to magnet errors threatened to be much bigger than those intrinsic to the beam, and might even grow indefinitely! Very soon we saw that this defect was serious but not fatal — if "resonances" could be avoided, the effect of magnet errors could be kept in reasonable bounds.[9] But this did mean that the one inch aperture cross section we had proposed in our initial euphoria was too small, and the magnets would have to be a good deal fatter — but still much smaller than without "strong focusing." Within a few months we understood the problem quantitatively, and BNL and CERN both settled on about the same parameters for our two projects. In December we had a dedication ceremony for the Cosmotron here, together with a small conference on the new ideas (where, incidentally, we met Kjell Johnsen for the first time), and we all agreed more or less.

Another startling development came up. About a year or so earlier, the AEC (I think) had sent us some patent applications for a new proton accelerator from an unknown man in Greece, said to be an elevator engineer without any particular academic credentials. His design was similar to the Bevatron but contained some errors which would have made it impractical if not impossible, and we forgot all about Nicholas Christofilos. But now a letter came from Berkeley, with the news that this same man had sent them, two years earlier, a proposal for an accelerator system that was startlingly similar to the AGS principle. At the time they had looked at it superficially, concluded that this was just another mad inventor who need not be taken seriously, and filed it away. Now they looked at it again, and found that what he had said in this proposal was correct. Nick's system[10] was a bit different from ours in detail, but the basic principle was identical. Subsequently Nick came here to Brookhaven and worked on our project for several years; later he went to Livermore to work on fusion and weapons, and he died a few years ago.

Actually strong focusing had also been anticipated by L.H. Thomas[11] in 1938. As we know, the cyclotron required fields decreasing with radius to give vertical focusing, while for synchronism an increasing field would have been desirable. Thomas showed that if the field varied azimuthally, one could retain vertical focusing even with a field that, on the average, increased with radius, i.e. synchronism and vertical focusing could be made compatible. Most people thought that this was too complicated to be practical, but we saw in 1952 that our work was in a way an extension of Thomas's.

In fact, unknown to the open physics community at the time, a project was under way at Berkeley to build a large Thomas cyclotron for high intensity beams, which would make lots of neutrons for plutonium production. This project was aimed at the weapons program, and was classified secret. Therefore the AEC was inclined to classify our project as well; it took a lot of persuasion on the part of Leland Haworth to get permission to keep our project in the open and to let us publish (probably because we had already talked to the Europeans before most of us knew anything about the secret project).

So we went ahead. In early 1954 Haworth made a formal proposal to the AEC, in the form of a six page letter (rather than the thick books customary nowadays), for a 25-30 GeV machine to be built here -- the AGS.

In the meantime, R.R. Wilson at Cornell had just gone ahead and built a strong focusing electron synchrotron for 1 GeV-- the first such machine anywhere.

One aspect that worried us was the transition energy problem. Theory showed that it should be easily manageable. But seeing is believing-- so we decided on a demonstration model, the "electron analogue." This was an electron accelerator, with electric instead of magnetic fields, and very low energy (only a few MeV). It was built very quickly, and showed that indeed the transition energy phase jump was no problem --but it also showed us, what we had not expected, that nonlinear resonances could be more important than we had expected, and made us more careful about these than we might otherwise have been.

Very soon people began to think about even higher energies. I think it was Matt Sands who first proposed piling even larger synchrotrons up in cascade -- a small one injecting into a second one, etc., as is now done at Fermilab and CERN. In 1959 there was a workshop at MURA (Madison, Wis) where the preliminary ideas were worked out.

As for beam intensity: We were clearly too modest in our estimates. We only claimed the AGS would accelerate 10^{10} protons per pulse. A general feeling developed that strong focusing would produce weak intensity and vice versa; hence the ZGS at Argonne which was supposed to produce much higher intensity than the AGS. Actually the AGS went from the initial intensity of 10^{9} on the first beam day to 10^{11} in a year or so, and then rather quickly to

Cottingham, Plotkin, Christofilos, Snyder, Green, Raka at Electron Analogue control.

$3x10^{12}$, and now runs close to 10^{13}. The main reason we did not know our own strength was that we were too pessimistic about linear milliampere and felt that we were being daring (no linac had exceeded some tens of microamperes up to that time); as a joke someone said "why not 5 milliamperes?" Nowadays linacs routinely go to 30-100 mA and even higher.

I can only hope -- but do not dare predict -- that our current ideas on achievable performance in ISABELLE are analogous to what we thought about AGS capabilities twenty years ago.

M. H. Blewett and M.G.N. Hine near Geneva in 1953.

V. Vladimirski, R. Hofstadter, G. K. Green in Geneva, June 1956.

L. Smith and H. Hereward at the PS construction site.

S. Kheifetz at Dubna conference, Sept. 1963.

Yu.Orlov at Dubna conference in Sept. 1963.

S. Kapitsa and V. Veksler at Dubna conference, Sept. 1963.

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The AGS and the CERN PS: Recollections from the Early Years

Kjell Johnsen

I have a feeling that we are approximately in the situation we have all encountered occasionally: when reading a novel, we discover that it is divided into three parts. Each part is the same story but told by three different people.

Let me start by making a confession. I have some difficulties these days in identifying where I belong. At this moment, I have put on the hat marked CERN and more particularly the CERN Proton Synchrotron. Tomorrow I will put the hat marked BNL back on.

I'm going to give a personal description and not try to give a balanced history of the AGS and PS development. We are celebrating 20 years, but it turns out that neither I nor the previous speakers really want to talk about the last 20 years. We want to talk about what happened before that. Now I don't think this is too bad. I think that often, for instance, when we celebrate birthdays, it is more exciting and more enjoyable to dwell on the love affair before the birth. It is the pre-birth, pre-20 years period that I will spend time on.

When did it happen? When did I first see the girl? I came from a very different field. I was asked in 1948/49 to start looking at accelerators and I began, of course, by looking a little in the literature and here I saw, - it must have been late '48 or beginning of '49, - an artist's view of something fantastic. It was called the Cosmotron. I was overwhelmed by the dimensions, by all it presented, and I must admit I thought the nuclear physicists must be quite a bit crazy. (I don't know if I have changed my mind very much since.)

A few years later, I had been away from my Institute for a year. (This was the Institute led by Odd Dahl, a Norwegian accelerator expert, well known in this country at the time.) The first assignment on my return was the following. Odd Dahl said:

"I'm sorry, I have to be out of the Institute, and there is a Frenchman coming tomorrow to stay for a few days and he is going to talk about a project I'm going to be involved in, namely, an European accelerator project. He wants to discuss this with us. I'm sorry I am out of town. Can you look after him?"

This visitor, Ed Regenstreif, who later became our good friend, and on this occasion gave me the first vision of what this future European laboratory would be, the laboratory later known as CERN. But he also gave a glowing report from a recent visit he had made to this country where he had seen the work on, in particular, the Cosmotron and the Bevatron, the Cosmotron being the more advanced of the two.

The report he gave on the work here at Brookhaven was really interesting, and I think it did influence considerably the later approaches that were made to this laboratory from Europe. This point has been mentioned by previous speakers. The next milestone, seen from my side, was when Odd Dahl and Frank Goward went to Brookhaven to learn how to build a scaled-up Cosmotron, joined forces here with Wideroe, and ran straight into the Courant, Livingston and Snyder discovery of strong focusing. Due reference has been also given by the previous speakers to Christofilos for this discovery. I was sitting in Bergen quite ignorant of this exciting development and it was quite an experience having Odd Dahl return to his Institute from America, beaming, and announce, "Drop all that you are doing, change straightaway, because there is a beautiful new focusing principle and this is absolutely a breakthrough. We are going to lift the planned energy from 10 GeV to 30 GeV straightaway and we are not going to work any more on the old fashioned ideas." I tried to protest a little, I regret to admit, because I felt there must be limits to changing things quickly. On the other hand, I also knew Odd Dahl. I knew his intuition and we followed. I personally believe that this was the most courageous and most important decision that was ever taken for CERN. If CERN had gone the Dubna way, I don't think CERN would have been the institution it is today. It did show intuition. It also showed, however, another thing: it radiated confidence in the team Odd Dahl and the others had met over here at Brookhaven, and it counted on a strong hope for the future good collaboration. As most of you know better than I, there were in those days classified activities at the Brookhaven Laboratory, which imposed restrictions on the flow of information from the Laboratory. Lee Haworth made sure that BNL was as open as possible within those restrictions, and we are extremely grateful for all the information we were able to gather. This was the start of about the finest informal collaboration that I have seen.

So we established plenty of contact. Ernest Courant, John Blewett and Hildred Blewett came to Paris early to advise and help at a meeting in '52. I myself came to Brookhaven for the very first time in December '52 to participate in the "second" running-in of the Cosmotron and was duly impressed. We spent much time discussing the alternating gradient principle and I learned much. It is strange what memories occasionally pick up. In general, I was impressed, but one thing I remember very clearly was one day when we came walking along the corridor in the Cosmotron building that I've learned to know so very well the last eight months, the same corridor in which I have my office now. We were in a group which included Hartland Snyder. In the opposite direction came Ernest Courant with his, you know, apparently shy smile. He just stopped at Hartland Snyder and said, "You may not know it, Hartland, but you have just written a paper". This impressed me. This must be a fine way of writing papers, I thought, but also I didn't know who was most proud of the two to be a co-author with the other author at that moment. That I leave to others to guess.

I remember very well that during that period I also met Ken Green, George Collins, Dave Jacobus, Lyle Smith and others in addition to those that I mentioned before. This was extremely important for the future collaboration; so was the visit for more than a half a year of Hildred Blewett and John Blewett to the Proton Synchrotron Group of what now had been named CERN. They stayed with us first in Bergen for some months and then in Geneva. They tried to put us on the right track. We had kind of a public examination in October, 1953 where we to some extent felt like students, you know, who had to stand on podia like this and here were Livingston and Green and God knows who, sitting in the audience. I believe they had even been asked to give a secret report on us.

Solid contact was established, and from now on I doubt that any major technical decision was taken on the two machines without mutual consultation. There was one decision that I'll comment on as seen from our side, and that was the electron analog here in Brookhaven. The electron analog was also a bit of a problem for us at CERN, because we had studied the question of whether we should build a similar analog, and we concluded that we just simply couldn't afford it. It would mean reducing the energy of the machine we wanted to build, and in addition we couldn't spare the people. So we decided to go ahead without an analog. Of course, we were pleased when we found Brookhaven took the opposite view because we knew that through the close collaboration that was established we would learn as much from their analog as we would have from our own. May I nevertheless confess another thing. We also felt that a good byproduct from this was that we at CERN then got a good head start on our machine and we kept it up until the end, which gave us a bit of self confidence, perhaps needed by some of us.

John Adams and Mervyn Hine came over here, I think for the first time, during the running-in period of that analog and learned much. And in this way the good cooperation continued to the very end of the construction and the running-in of the PS in 1959, so vividly described in a CERN Courier article by Hildred Blewett, who came to CERN to participate in the start-up phase. I believe she was able to bring back experience that was valuable for the running-in of the AGS the year after.

I would like to make a few general comments on this very informal and very fruitful collaboration between the two laboratories. Often we found that when we consulted each other on technical issues we ended up with a common design even if the first approaches were different. One party discovered perhaps that he had overlooked something and therefore changed course. Sometimes perhaps we took the same decision because we found it easier to share the risk. A good friend of mine reminded me the other day in a letter that it was in some respects a very simple-minded approach we had in those days to the design of our machines. On the other hand, I think that for that reason we had to stretch our minds, our intellect, to the limit. It is perhaps worth reminding ourselves that at least for the PS no design parameter ever was established by a computer. The young accelerator physicists don't grasp that nowadays.

However, equally often we took different decisions and opted for different designs. In those cases we knew it perfectly well and did it with open eyes. There were various reasons. For instance, we had different situations with industry. I can mention as an example the linac, where we did not think we could take responsibility for using copper clad steel with the state of the European industry at that time. In America, one could. With the vacuum technology also, you could be more advanced. In general, BNL was perhaps a little more advanced and more daring in decisions on technical detail. In other cases we took a slightly different course for the reason that we had different experiences and different background before decisions were taken. For example, although the designs of the magnets were superficially rather similar, they were different, and I think those differences came from such difference in background. Then we had occasionally genuinely different opinions and these are perhaps in some cases the most interesting to look back on. As an illustration, the small example of the acceptances built into the linacs. They were different to such an extent that on the PS, as we moved up in intensity, we could just pull more and more particles in and we were very proud of beating the AGS in intensity for several years--til we leveled off when we had filled the acceptance. By this time the AGS people, who had been, if I may say so, slightly red-faced for a while on that particular point, had developed multi-turn injection and made that very efficient indeed. Here they came from behind us to sail past and well in front of us, and that was that. Now I do not really know which of those two decisions was the right one. Under the circumstances, I think it illustrates that many, many problems have many different solutions that are all good. This is something to keep in mind sometimes when we criticize other solutions than the one we favor.

Our collaboration was surprisingly free of mutual criticism. We may have a lesson to learn from this nowadays. We at CERN got only encouragement from BNL, even when we differed. We had it out with each other, we discussed, and we got only encouragement. In return we also offered little negative criticism, I believe, when we differed. Accepting different approaches may overall be equally good, and that turned out to be the case.

There was also another aspect, which I think has been true in the accelerator field all the time, so that is not special for CERN and BNL: the surprising openness. To my knowledge, no idea was ever hidden from the other party, however good we thought it was.

On the contrary, for us at CERN, our BNL partners were the first ones on which we tried and wanted to try our ideas. And we developed a sense of security out of this, so we got something back. Again, I would take a very small example. I remember we had an idea, it's not worth bothering you with what the idea was, and it so happened that we came here a few weeks or a few months afterwards, and, of course, mentioned our idea. What do you think about it? Our friend John Blewett said, "Nonsense, it doesn't work." I said, "I think it works." "No, no, no, it won't work." Then John Blewett disappeared. I don't know where, but we were in good company. After awhile he reappeared smiling and said, "You were right, it did work. We have tried it in the lab." I don't remember if that thing was implemented on this side of the Atlantic. I know it was implemented on the other side.

What was the result of all this good contact we had? Well, the result was that we constructed two unbelievably reliable and at the time very advanced work horses for nuclear physics. The AGS has produced 20 years of very fruitful physics and is still going strong. In addition, it has a new future in front of it as an injector for ISABELLE. That is not a bad performance after 20 years, to be able to look forward to many more years of advanced utilization. The PS did about equally well, being equally old. It did physics that I hope is comparable with what was done here. It has worked as an injector for the ISR, for the SPS and will work as an injector for the $p\bar{p}$. This is a remarkable record for machines that were based on a completely novel idea and implemented only weeks after the invention.

There was another result. We managed, or it came naturally, to build up a very, very warm feeling for each other's laboratory. I think it is a feeling that has not been surpassed by any other informal or formal collaboration between labs. We oldtimers who were involved in the early days cannot forget this. Some of us developed in those days a soft spot for our sister laboratory that time has not been able to wear off. Good luck for the future task for the AGS, and all our gratitude for the past. Thank you.

Finding the Second Neutrino

Melvin Schwartz

Well, I must say it's really a great pleasure to be back here and to see so many familiar faces. In fact, the amazing thing is to see so many of the people I knew back in the days when we were setting up on the floor of the AGS. Incidentally, the picture that John showed, the 1961 view, is actually 1965. In 1961 it looked really a lot different. It was only one little building and was just a horrendous job to try to mount a major experiment.

But let me tell you a little bit about the history of how that whole thing came about. It's a history which has a certain element of physics in it and, as all histories have, a certain amount of politics, maybe a certain amount of personal involvement. After all, these experiments, every one that you know about or hear about, are really personal efforts on the part of individuals for whom these experiments constitute a major portion of their lives. So much of what one sees in the experiments is in fact an interaction of one's self with equipment and other people. Basically, the whole notion of doing neutrino physics in this area started several years before the experiment actually was done. Back in 1959 I guess, at a coffee hour at Columbia, the question was raised how one might possibly investigate weak interactions at high energies. In those days no one conceived of electron-positron rings with 100 Gev beams going in each direction as having weak interactions, which is in fact the way one will really, in the end investigate weak interactions at high energies. That was still probably twenty years off in the future and that evening it came to me that one might indeed investigate weak interactions at high energies by using neutrinos. Now neutrinos are of course, as most of you know, particles which really have no interactions other than weak interactions and so they are an ideal probe for studying the weak interactions.

When you hit them against things, you essentially see what weak interactions do, and since the energies of those times were considered to be very high, several GeV, that was weak interactions at high energies. In fact, much of the history of that time is related in conversations during the next year between myself and Lee and Yang, who had many of the most profitable theoretical notions related to this area. I should also mention that the idea of neutrino experiments occurred to several other people before myself. In particular, the earliest record is a published paper in the Bulgarian Journal of Physics some years before that. It was a name I really can't remember. Of course, Pontecorvo, who has played an immense role in the history of neutrino physics, had many of these same ideas at about the same time.

In any case, the first question that arose was whether the electron neutrino and the muon neutrino were really the same animal. And indeed there was only one very uncertain piece of information on this question at that time and that was the absence of the decay of the muon into an electron plus a gamma ray. The question was why this decay was never seen, because after all here are two leptons and one might expect in a sort of natural way that one will decay into another unless there was some property about the muon, some quantum number related to the muon which was different from that of the electron. Now, in fact there had been some theoretical work before that point which had indicated that if this thing called an intermediate boson existed, then this decay must occur unless there really were two types of neutrinos. The key contribution in the thinking that was made at this point by Lee and Yang was the observation that in fact this must always go if there is any kind of size structure to the weak interaction or else the so-called unitary limit would be reached and there would be really serious problems in any case. So, it was really very, very hard to avoid this unless there were two types of neutrinos.

Of course, given this history, it was obvious at this point that one ought to investigate neutrino physics. How did one investigate the question of a different quantum number? Well you start of course with a π which decays into a mu plus a neutrino; you don't know yet whether this neutrino is the same as the one that comes from a β decay, but it is an interesting question to investigate. Neutrinos of course can go very long distances in matter, a million miles of lead, or something like that, at these energies, before interactions. So you can filter it through a wall and then look at the other end with a large detector and presumably, if the neutrino that came along with the muon was the same kind as that which came along with the electron, you would anticipate seeing in this chamber as many electrons produced as muons. The reason for that is a little bit theoretical in nature but relates to the fact that in every way, as far as weak interactions were concerned, electrons and muons appeared to behave identically, so that if there really was only one type of neutrino it should make a muon quite as often as it made an electron. There should be the same number of each. And the interaction one would look at would be typically say $\nu + n \rightarrow \mu^- + p$ or alternatively $\nu + n \rightarrow e^- + p$. If these happened at the same rate, then there was one type of neutrino. If the second one didn't happen at all, then of course it would mean that there had to be two types of neutrinos. So that's the basic background of that original experiment. Needless to say, we worked very hard at thinking up ways of doing it.

As I said earlier, the key notion in the thing is to make a large number of neutrinos, and the second part of it is to make a very big detector. And so I would say the first six months of our effort were devoted to trying to figure out how to make a very large detector. We had notions of huge masses of iron and scintillator -- remember, it has to be a detector which not only is heavy but also can detect the difference between a muon and an electron. It doesn't do any good to have a mountain of scintillator if you can't see what's happening inside of it. The difference between the two as far as material is concerned is that one travels very long distances without doing anything, that's the muon, whereas the other one, the electron, makes so-called electromagnetic showers, and after typically 8 or 10 inches of aluminum has pretty much dissipated itself. So the two of them appear very, very different in material. We went through a whole variety of kinds of instruments and then as very often happens in this business somebody invented just the right machine for us. Some Japanese physicists at that time had developed a so-called spark chamber, a device which was made up of a collection of thin plates in neon gas, and if you pulsed those plates with a high voltage right after a charged particle passed through, you would see a track. This was obviously the ideal instrument for doing this type of experiment. We heard about this indirectly -- I think it was Irwin Pless who told me one day at the Cosmotron, and the next day we ran out to see the machine that had just been built by Jim Cronin, and as soon as we saw it we knew this was exactly the instrument. But of course nobody had ever built one which weighed 10 tons. That was the second problem.

Now in planning for experiments, I tend always to be an optimist (most people do, I guess), and when we first sat down to do the figures we said, "Well, we ought to get one event per ton per day." That was the number we worked with. Actually it turned out to be a factor of about 10 to 20 smaller than that, but fortunately we built a detector which was 10 tons in size. Incidentally, by this time we had formed a group that consisted of Gordon Danby, who was very instrumental in the construction of the chambers in the early days and in the operation of the machine for us and in working with us on all the machine parameters, Jean-Marc Gaillard, two other graduate students, Nariman Mistry and Dino Goulianos, and of course, Leon Lederman, Jack Steinberger and myself. I might interject: there was a little piece of politics that happened just before this and that was the question of CERN. There was after all a machine that had a head start on us at CERN, and it was clear that they would be at least six months ahead of us, maybe even a year, and Jack actually went off to join the other side for a while. (Some people think we sent him as a saboteur, but it's not true.) But in any case, the question then was: Would CERN beat us? And indeed the entire experiment was designed at CERN and was almost ready to mount and then we heard the great news. (In fact there were two great pieces of news. The first one was of course when we heard this and the second one when we got the first event.) We heard the great news that Von Dardel had discovered a mistake in the calculations and indeed it turned out that the beam as it was planned for CERN would give very low intensity because the beam was planned for a 5-foot straight section and the defocussing effect of the magnets right after the straight section would have essentially demolished the beam. I should point out that in all these cases the target was in a straight section of the machine and of course the pions would come out and then the wall would begin somewhere and then finally the detector. So the idea was that you had to have a very good intense beam of pions aiming more or less in the direction of the detector in order to get the neutrinos.

Well, in any case the mistake there of course was that they had set it up for a 5-foot straight section. The other mistake, which is in a sense much deeper and is really an indication of the very real difference in physics philosophy in this country compared to what it was there, was that rather than saying this experiment is so important let's move it to the 10-foot straight section, the hell with all the other junky little experiments that are going on, they cancelled the experiment, and of course we knew they would do that because that was just the style. So I guess that was the most cheerful thing that happened at that time. Remember that was early in 1961 and we really were way,

way behind because we had a detector but no machine. The big 10-ton spark chamber was completed here in the summer of 1961 and worked extremely well. It was the first chamber of that size that was built and I would say it worked probably better than we ever had any anticipation of seeing.

And then we began looking at pictures and I would say for a period of two months it was nothing but junk as we improved the shielding wall little by little. In the end there was a residual level of junk which I suspect in retrospect was probably neutral current type events. But you have to remember that it would have been quite impossible with this setup to see neutral currents because, you see, the beam was unlike current neutrino beams which are nice clean, highly collimated, very energetic beams. Neutrino interactions were taking place on all sides of us and so neutrons were indeed (from neutrinos alone) coming in and the flux of those was quite comparable to the number of neutral current interactions that one might see and hear and there were also neutrons that were coming in through cracks in the shielding and things of that sort. So I don't feel all that bad that we didn't have any interpretation for the little junky things that occasionally occurred.

But in any case I still remember quite well one day, I guess around November or so of '61, when Leon and I were at Columbia and we got a call from Dino Goulianos, who was doing his thesis on this, saying that they had seen the first event, and it was one spectacular event. It was a muon that made its way all the way out, with, in fact, a large shower on one side of it, but it was so characteristically what a neutrino event should look like that it was absolutely clear that this was the first of these. Subsequent to this we got about 29 or so others that had just a muon, plus a large number that had a muon and other tracks associated with it. But in any case the rate ended up being something like one per day which is about a factor of 10 less than we had anticipated, but enough to be able to do an experiment.

Two other amusing things in those days. One is that unlike today there were no committees to decide whether you run or not. It was just Maurice and he was very generous and I must say that his great wisdom prevailed. Also it was a very informal type of organization: the entire experiment took less than a year and only 7 people were involved in it, so it was a very different type of thing from what you see now. But it had a number of very unique features. In any case that's the brief thumbnail sketch of that short period in my own association with this lab. I do wish the AGS and of course ISABELLE as it is coming up a very great future. Physics is not exactly the same as it was in those days, but I think in many ways it is just as exciting as it has ever been. Thank you.

A Discovery in Inner Mongolia

Val Fitch

I'm sure we are not gathered here to celebrate a dumb (non-talking) machine but rather to reflect on the people who invented the machine (and what a marvelous invention), who built it, and ran the experimental program, and such an extraordinarily fruitful program it has been. I was asked to talk about CP violation and so I speak for the little group of Christenson, Cronin, Turlay, and myself. It is obviously an occasion to get absolutely nostalgic about the good old days and I'm not very keen about this kind of thing. You probably remember a black baseball pitcher by the name of Satchel Paige who, at the age of 60, was finally allowed to play in the major leagues. All the reporters went around to ask him how he managed to do so well for so long and he said he never looked back, something might be gaining on him.

With this in mind, at some risk in view of the above story, I did go back to our data book to recall just what went on. I even went back to the experimental proposal. It's interesting to compare the way we lived and what we did in those days with the way we live and what we do today. Many things have changed and many things have not changed.

First of all, the proposal. It was double spaced on purple ditto and it goes to a page and a half. Today, of course, an experimental proposal is apt to be an enormous brochure with colored pictures of models and so on. By current standards ours was a modest proposal indeed. I have a copy of it here in front of me. It's so short I will go over the whole proposal.

The title is "Proposal for K^0_2 Decay and Interaction Experiment" and the introduction: "The present proposal was largely stimulated by the recent anomalous results of Adair et al., on the coherent regeneration of K^0_1 mesons. It is the purpose of this experiment to check these results with a precision far transcending that attained in the previous experiment. Other results to be obtained will be a new and much better limit for the partial rate of $K^0_2 \to \pi^+ + \pi^-$, a new limit for the presence (or absence) of neutral currents as observed through $K_2 \to \mu^+ + \mu^-$. In addition, if time permits, the coherent regeneration of K_1's in dense materials can be observed with good accuracy." So that was the introduction.

"EXPERIMENTAL APPARATUS

Fortuitously the equipment of this experiment already exists in operating condition. We propose to use the present 30° neutral beam at the A.G.S. along with the di-pion detector and hydrogen target currently being used by Cronin et al. at the Cosmotron. We further propose that this experiment be done during the forthcoming μ-p scattering experiment on a parasitic basis."

This is a bit of strategy still used. If you can show that you don't cost anybody anything they'll let you in. I started reflecting on the μ-p scattering experiment since it would be the prime user and have all the priorities and I seem to remember that Leon Lederman was involved, controlling the beam. This is one of the things that hasn't changed at all.

"The di-pion apparatus appears ideal for the experiment. The energy resolution is better than 4 Mev in the m* or the Q value measurement. The origin of the decay can be located to better than 0.1 inches. The 4 Mev resolution is to be compared with the 20 Mev in the Adair bubble chamber. Indeed it is through the greatly improved resolution (coupled with better statistics) that one can expect to get improved limits on the partial decay rates mentioned above.

"COUNTING RATES

We have made careful Monte Carlo calculations of the counting rates expected. For example, using the 30° beam with the detector 60 ft. from the A.G.S. target we would expect 0.6 decay events per 10^{11} circulating protons if the K_2 went entirely to two pions. This means that one set a limit of about one in a thousand for the partial rate of $K_2 \to 2\pi$ in one hour of operation. The actual limit is set, of course, by the number of three-body K_2 decays that look like two-body decays. We have not as yet made detailed calculations of this. (Today they'd send this back and say make those calculations.) However, it is certain that the excellent resolution of the apparatus will greatly assist in arriving at a much better limit.

"If the experiment of Adair et al. is correct, the rate of coherently regenerated K_1's in hydrogen will be approximately 80/hour. This is to be compared with a total of 20 events in the original experiment. The apparatus has enough angular acceptance to detect incoherently produced K_1's with uniform efficiency to beyond 15^{o}. We emphasize the advantage of being able to remove the regenerating material (e.g., hydrogen) from the neutral beam."

"POWER REQUIREMENTS

The power requirements for the experiment are extraordinarily modest. We must power one 18-in. x 36-in. magnet sweeping the beam of charged particles. The two magnets in the di-pion spectrometer are operated in series and use a total of 20 kw."

So what else has changed? First of all there is the magnet power, only 20 kilowatts, and now it is more apt to be two megawatts. Furthermore, we used homemade electronics. There wasn't the big accessory industry that is associated with the field now producing absolutely superb apparatus to do counting work. Rather, we had to do our own. In our case we even had a homemade NMR to monitor the magnetic field. That really wasn't necessary but constructing it had been a good exercise for the student.

The time scale of the experiment: This proposal that I just read to you is dated April 10, 1963. We were taking data on June 10 in 1963 and we started the CP part of the experiment on

June 22, 1963. In late October we had finished all the measurements and we had this suspicious-looking hump for which we waited to go away. It was still there over Christmas and came time for the Washington meeting — you know the Washington meeting is in April every year but you normally have to submit abstracts for contributed papers by early February and as usual we constructed an abstract and sent it in at the very last minute, just before the deadline. Well, we weren't really sure we wanted to talk about this yet but the Washington meeting was a long way off so we sent in a noncommittal abstract. It was sent back to us! A new rule to the effect that all abstracts had to be typed in one paragraph had just been established and ours was in two paragraphs. By the time we got it back it was too late to resubmit. So the paper missed that meeting except that we did talk rather quietly about it in a postdeadline session and tried to de-emphasize that funny bump. The main observation now is the incredibly short time between the proposal and the results, impossible now.

Now to the life style. This talk is entitled "A Discovery in Inner Mongolia." Inner Mongolia is a local term devised by Ken Green denoting the area inside the magnet ring. Doing an experiment in Inner Mongolia had certain advantages. There is no experimental activity at all there now but we were there and visitors coming through the AGS would seldom spend the energy to climb the stile over the ring and come down the other side. So we were largely left alone. We did our own thing over there. No one came around and asked any questions. We had our electronic equipment set up on the floor of the AGS just inside the big door in the original experimental hall. It was set up right next to the beam line. The circulating beam intensity in those days was about 10^{11} so there was no radiation problem. We just sat there with our electronic equipment right beside the beam and watched the mesons go by. Of course it was terribly hot or cold and also noisy, especially with those roof fans. If you have been in the AGS when those roof fans go you know what it is like. Of course now one has the elegant comfort afforded by one of the Portacamps or trailers, all air-conditioned, etc. That was not for us. The amenities have changed a lot and much to the good.

What has not changed? As I said, I thumbed through the data book and I saw comments like,"Lightning struck ending this run prematurely. All scalers went off with their information. All magnets off." A couple more pages: "Weather bad, write down data often." We also had trouble with the helium bag leaking. Actually, it didn't happen fast enough to make for any real concern but it was something to worry about. We still have all these kinds of difficulty.

Here's another comment. "Pickup from roof fans makes the NMR setting impossible." "Beam off. Mg. set overheating. Hot day, 90-95°." "Fiducial relay jammed. Replace." "Started flipping F-10 at 1700. 20" bubble chamber. They are taking 25% of the beam. Exclamation mark." Counter people are still jealous of any beam going to a bubble chamber. "Sweeping magnet voltage showing some oscillation. Watch it."

I suppose the greatest, the most important, thing that has changed is the time scale. I indicated that between the proposal dated April 10 and the end of the year we actually had results. The time interval between the proposal and collecting data, really doing something, was amazingly short. Of course, that also represents the enthusiasm of the people that we had here to help us handle the work. We're so completely aware of the splendid cooperation from the BNL staff that made this experiment go.

There was one other aspect of this short time scale. We submitted the paper July 10 and PHYSICAL REVIEW LETTERS actually had it in their July 27 issue, one year following the data collection. We had tried to kill the famous effect for about six months and then finally gave up and published it. This started a flood of theoretical papers. Certainly the first came from the man right down here in the front row (Wu and Yang) and the standards that he established for analyzing are still used today, defining parameters like $\eta_{\pm}$, η_{00}, K_L, K_S and so on.

One amusing thing that came out of this: One of my graduate students got his degree and went off, became an assistant professor, I believe, at Berkeley and received one of the standard forms from the American Men of Science asking, "When were you born? etc., etc., and finally, what is your special interest? He wrote down "CP violation in neutral K decay." When the proof came back, it was "carbon-phosphorus violation in neutral potassium decay."

The other discovery I would like to discuss has never been published and very few people know about it. This might be a good time to say what it is. You saw those pictures of the AGS earlier. There is that lovely grove of pine trees in the center of the ring. In that grove of pines we discovered wild orchids growing. I have always considered myself the guardian of those orchids. I went out there just before lunch today and in fact some of them were in bloom. I must say I'll always regret telling you if in fact they disappear now.

From Ω to Charm - One Great Picture
is Worth Another

N. P. Samios

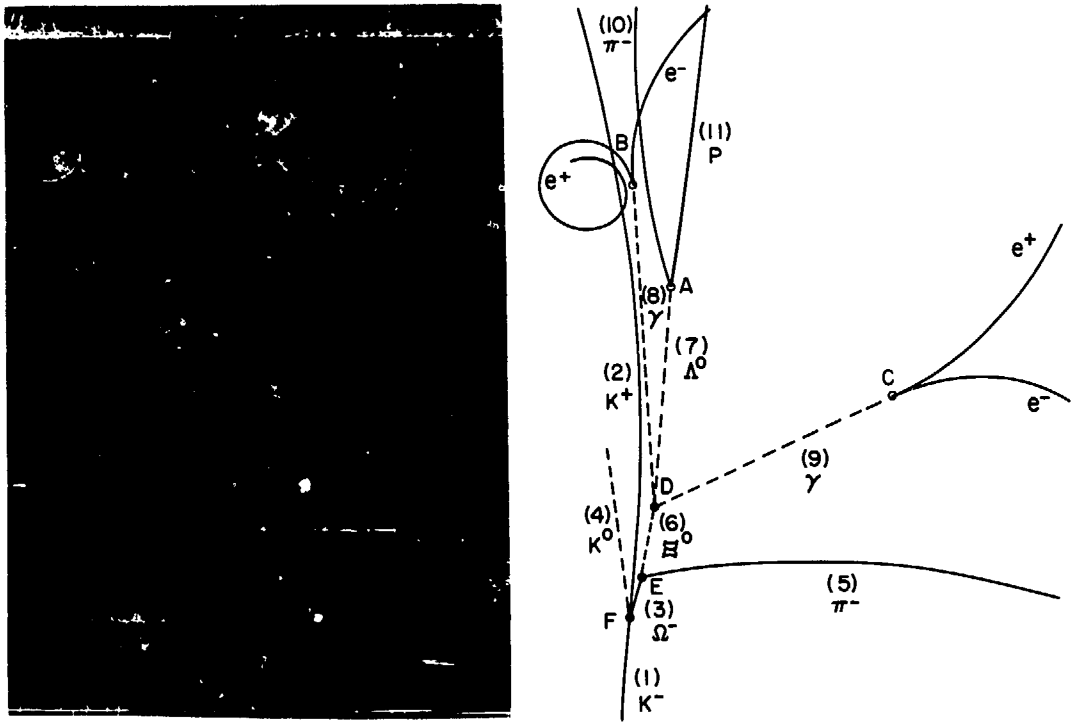
Today is a time for philosophizing and reminiscing, for seeing friends and discussing the good old times. I thought I would use the minutes I have available to illustrate the point that the paths of physics are not as straightforward as the textbooks sometimes lead us to believe. Many experiments have been and continue to be performed at the AGS, most of these adding substantially to our body of knowledge. However, most accelerators are distinguished not just by the totality of performed experiments but by the few outstanding results which alter fundamentally the way we look at nature. This historical trend has indeed been repeated at the AGS with the further caveat that none of these major results had been even remotely anticipated at the time the construction of the AGS was proposed. We heard earlier about the Haworth letter, six pages in length, providing the request and justification for a 30 GeV accelerator to be built at BNL. The new technical idea was strong focusing, but the physics motivation was contained in a few sentences from which I quote, "...the Cosmotron has, during its relatively short operational use, yielded much new data on meson yields, and on the energy dependence of π meson and fast neutron cross sections and has even led to the observation of certain hitherto unobserved heavy meson phenomena. That extension of the available energy would yield many fruitful results seems unquestionable; indeed, it is already possible to visualize many useful experiments requiring considerably higher energies. Although many of these will be made possible by the 6 Bev soon to be available at the University of California Bevatron, still further extension seems highly desirable, for specific and predictable reasons as well as on the general grounds that past extensions of energy have always proved highly profitable." And that's it. It does my heart good to see a major accelerator justified on such correct and simple grounds -- new technology and a màjor increase in energy, allowing a look into a new area for new phenomena. Nowadays one must produce a fat proposal which costs the equivalent of the expenditures to construct the whole AGS.

What I'd like to do now is to touch on two major findings with which I was fortunate enough to be involved, namely the discovery of the Ω^- hyperon and the Λ_c^+ charmed baryon. I am not going to discuss their significance as their impact on understanding the spectroscopy of fundamental particles is well known. Instead I will touch on a few of the difficulties encountered on the way to establishing the existence of these two particles -- on the basis of one picture for each. Figure 1 shows the Ω^-. Believe it or not, the 2 connected γ rays were not immediately noticed. Only when the event was being measured was one and then the second e^+e^- pair found. The unusual features first noted were the large transverse momentum of the decaying pion and the nondirect association of the Λ^o from the decay vertex, i.e., the Λ^o originated from another point which ultimately was shown to be from a Ξ^o decay. Upon measurement and analysis, this event was shown to be the decay of a Ω^-. What was not presented was the full photographic picture. This is now displayed in Figure 2. What one notes is that roughly one quarter of the chamber is not visible. This resulted from a problem that occurred in the optical system. Since the 80" bubble chamber had one window, 8" thick, one had to devise a retrodirective system to illuminate and photograph the bubbles from the same side. This system is shown in Figure 3, consisting of 80 vertical slats of plastic, so-called coat hangers. What had occurred in the early part of the run is that 15-20 of these hangers had fallen, in fact had hit the glass window. This was a moment of truth, for there were 1,000 liters of liquid hydrogen in the chamber and a chance that the front glass window had been damaged. There were four of us present on that midnight in December; Shutt, Palmer, Fowler and myself. We each took turns evaluating the possible damage and risks involved in continuing. We each gave our opinion; however, it was Ràlph Shutt's decision. He said to expand the chamber. And so we expanded once and looked in. Nothing happened. We expanded the chamber again and again it was okay. In fact, the run ensued with the chamber in this condition and the Ω^- was found. One further point is worth mentioning. If one looks closely in Figure 3, one will note a horizontal wire which was added after the fix up to catch any coat hangers that fell. In fact, during all the long history of the 80" chamber, 11.5 million pictures, not another coat hanger fell.

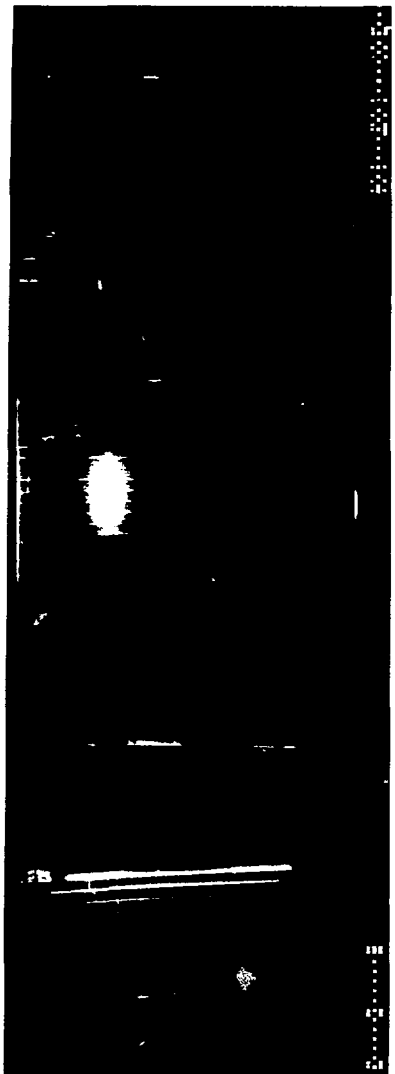
Two other difficulties that were overcome are illustrated in Figure 4 and Figure 5, one obvious and the other not so obvious. The large honeycomb cylinder, with clear separations, is the piston used to activate the chamber. With these splits it wouldn't work very well. There was clearly an engineering problem which had to be solved, and it was. The more subtle difficulty involving the beam is shown in Figure 5. In order to prepare a beam of K^- mesons for the chamber, a series of 25 magnets with appropriate beam separators was utilized. This equipment was turned on and the profile of particles was examined near the chamber. There was indeed a beautiful peak where the kaons should be; however, a Cerenkov counter placed to examine the character of the peak said it consisted of pions, not kaons. We proceeded to systematically vary all possible parameters to resolve this difficulty, all to no avail. Finally, one morning at 2 a.m., Bob Palmer and I determined that there had to be a secondary source near the target, which by accident imaged on the kaons. We therefore asked that the AGS be turned off and we went inside the tunnel to look for any peculiarities. I still remember walking down the ladder with Palmer when the small dimensions of the snout became apparent. Palmer had brought a ruler along, placed it on the beam exit port, measured its dimension to be one inch, and thus unearthed the secondary source. This was quickly fixed by placing a collimator inside the straight section.

The second physics topic to be discussed involved the neutrino program with the 7' chamber. The volume of this device was an order of magnitude larger than the 80"; namely, 10,000 liters of hydrogen or deuterium. In the process of making such a device operational, there were many grueling and memorable incidents. The accidental triggering of the foam safety system in the midst of the expansion system of the 7' chamber is shown in Figure 6. This is the only time it was activated during the lifetime of the chamber, and inadvertently at that. I also thought it would be interesting to look at one of the first pictures taken with the 7' chamber. This is shown in Figure 7. The chamber was sitting in what is now considered a canonical wide band neutrino beam which included an iron shield 16'x16'x100'. What is observed is an uncountable number of Compton electrons arising from γ rays from the $np \rightarrow d\gamma$ reaction. In other words, the chamber was sitting on a sea of neutrons. Careful plugging of all the holes in the shield as well as careful steering of the primary proton beam eliminated this source of background. Figure 8 shows the full picture of the first example of baryon charm. It occurs towards the rear of the chamber behind the four metal plates. For completeness Figure 9 shows a second view of the same event where the slow π^+ is seen to stop and decay via the μ,e chain. This provided a calibration of the magnetic field, which was important--and the two δ rays coupled with the well constrained kinematics in hydrogen provided the clear interpretation of baryon charm. Needless to say, it took six months to convince ourselves of the validity of this interpretation of the event and to publish the result.

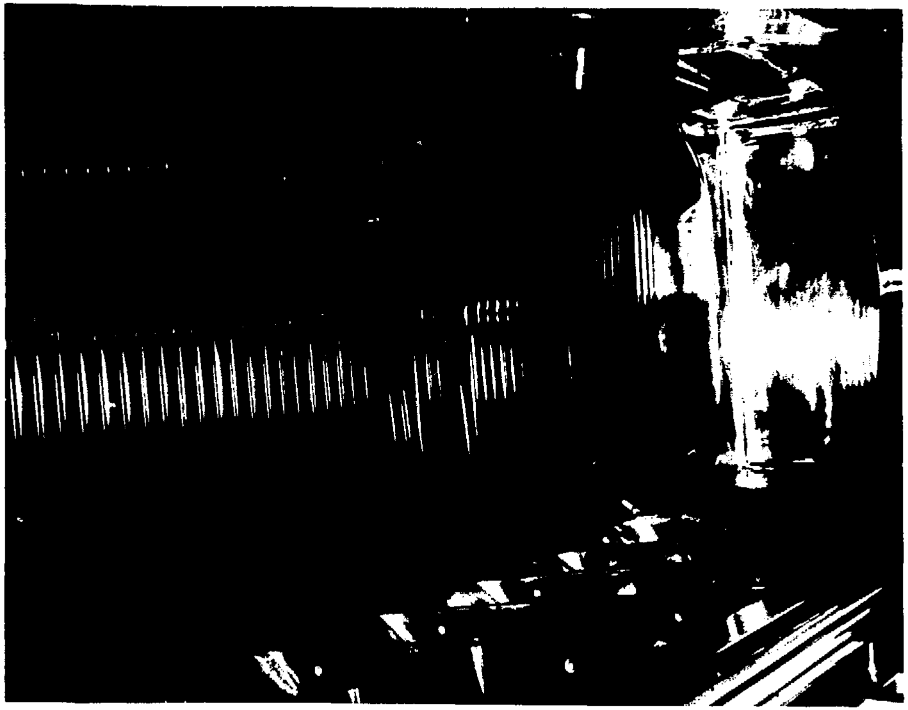
In these few minutes I have attempted to take you behind the scenes and show you that the physics associated with the Ω^- and Λ_c^+ was not as straightforward as one may have thought. In both cases there were formidable problems that had to be solved and some risks taken. It took a large number of skilled and dedicated individuals to accomplish the necessary tasks. All together there have been five bubble chambers operated at the AGS during this 20-year period, the 20", 30", 31", 80" and 7'. They have accumulated 40 million pictures, of which 10 million have been analyzed by Brookhaven physicists and 30 million by our university colleagues. The analysis of these pictures produced a variety of interesting and exciting physics. I've touched on just two which I considered the most important and are close to my heart. The topics here ranged from resonances, their discovery and their properties, the dynamics of a large number of processes to the search for quarks and tachyons, and so on. One of the strengths of the physics at the AGS has been not only the uncovering of new and unexpected phenomena but the lack of mistakes. We didn't find quarks or monopoles but lots of other goodies and we produced exciting physics. I'm now looking forward, after these 20 years with the AGS, to 10 more and to 20 years with ISABELLE.



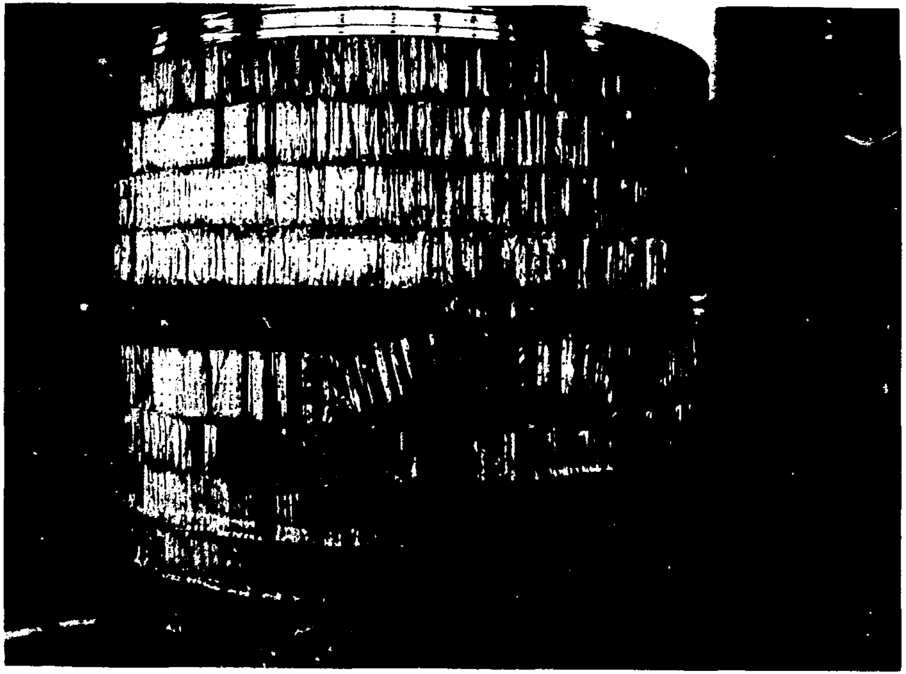
The first Ω^- event, observed in 80" bubble chamber in 1964.



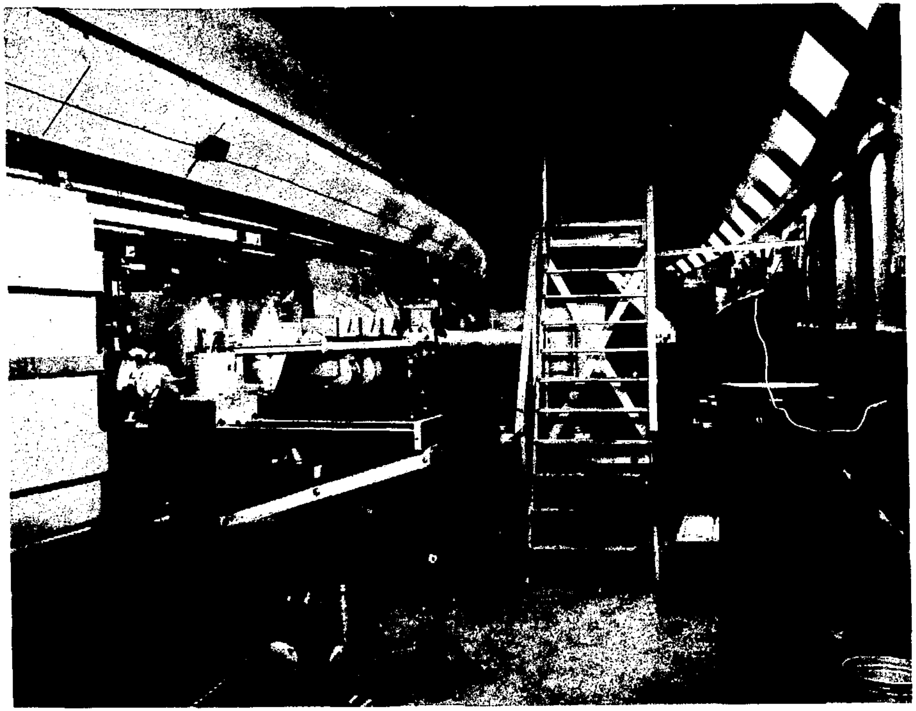
A complete view of the first Ω^{-}.



A view inside the 80" chamber, showing the "coat hangers."

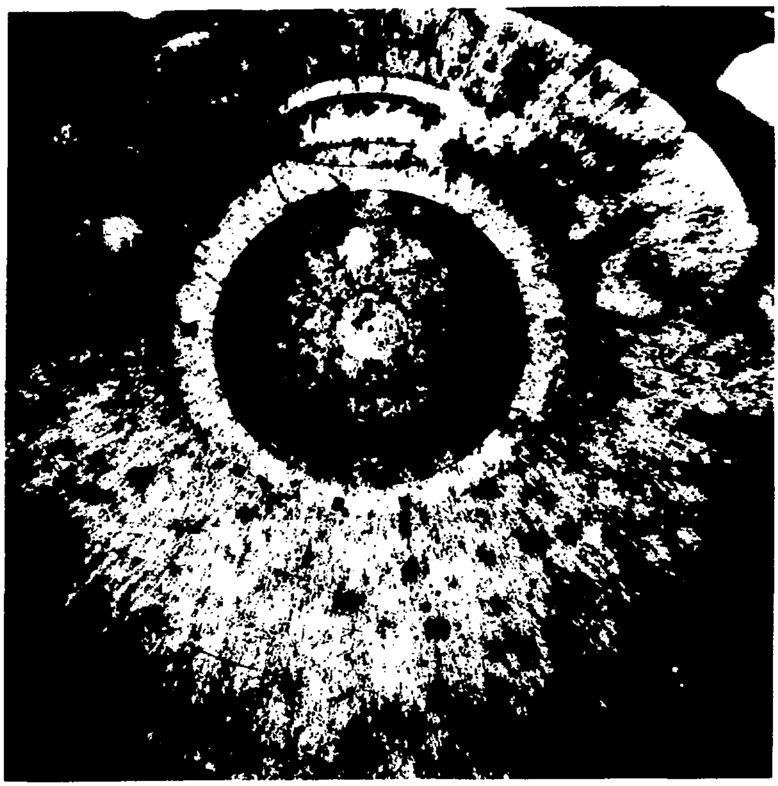


The bubble chamber piston.



The K$^-$ beam extraction point inside the AGS.

Clean-up of 7' bubble chamber building after unplanned discharge of foam fire extinguishing system.



One of the first pictures taken with the 7' chamber.

The first charmed baryon event discovered in the 7' bubble chamber in 1975.

The first charmed baryon event showing a π-μ-e decay chain.

The Discovery of the J Particle
at Brookhaven National Laboratory

Samuel C. C. Ting

There have been many brilliant experiments carried out during the first twenty years of the AGS. Among the most outstanding are experiments by S. Lindenbaum, M. Schwartz, L. Lederman, J. Cronin, V. Fitch, N. Samios and others. I will present here my own experience at the AGS which culminated in the discovery of the J particle.

Ever since 1965 I have been working on experiments associated with electron positron pairs produced from hadron interactions at high energies. In the spring of 1970 after five years of continuous work at the 7.5 GeV Deutsches Elektronen-Snychrotron, I became exhausted and following the advice of my doctor took a year off to rest. It was during this year that I had the opportunity to have many discussions with friends and read the work of others in the field. I also took the time to think carefully over the implications of our past work and to consider how we should next proceed in the new generation of high energy accelerators which were about to become ready for use at that time.

By the spring of 1971 I had come to the conclusion that the most interesting physics was in the field where my group had the most experience. I decided that we could contribute most significantly by doing a systematic study of e^+e^- and $\mu^+\mu^-$ mass spectrum from 1 GeV up to the mass of 50 GeV using a high resolution dector designed to search for new particles and to study the quantum numbers of these particles.

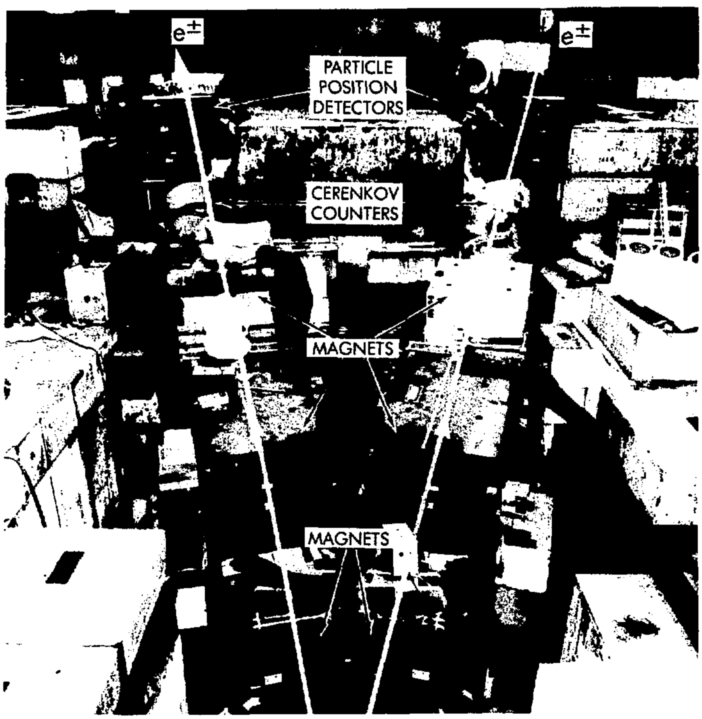
From 1971 to 1972 Professors Becker, Chen and I carried out many discussions on how to proceed. It soon became obvious to us that in order to cover the photon mass region up to 50 GeV we would have to perform three large scale experiments: From 5 GeV to 50 GeV at the Intersection Storage Rink, ISR, from 1.5 - 5.5 GeV at Brookhaven National Laboratory, and from 0.5 - 2.0 GeV at the 7.5 GeV Electron-Synchrotron at DESY. This division came about because the main background of $\pi^{+} \to \mu$, $\pi^{o} \to e$ decays can best be handled at low mass region with a lower incident energy. Thus we did not want to extend the ISR measurement down to 2 GeV region, and also we felt we could not safely perform the Brookhaven experiment down to 500 MeV/c region.

The detection of lepton pairs in the 30 GeV region was first done by L.M. Lederman and collaborators. This experiment gave the size of the cross section. It was an important experiment and generated much interest among the theoretical physicists.

During the year 1971 - 72 we performed a series of Monte Carlo calculations on detailed designs of the spectrometers needed and went over the logistic problems of performing 3 large experiments in 3 different countries. We came to the conclusion that in order to perform these experiments carefully we might try to set up these experiments simultaneously but could only run one at a time. In this way we could concentrate all our efforts at one experiment, finish it quickly and go on to the next one.

In the spring of 1972 we submitted a proposal to DESY and a proposal to Brookhaven and they were approved right away. The ISR proposal which involved occupying a whole intersection region with a 4π magnetic detector, was submitted jointly with Pisa, Genoa and Harvard Universities and was approved in the fall of '73.

From the early experience at DESY we felt that the best way to build a detector that could handle 2×10^{12} protons per pulse and at the same time have a large mass acceptance of 2 BeV and mass resolution of 5 MeV was to detect electron pairs with a large double arm spectrometer locating most of the detectors behind the magnet, so they did not view the target directly. To simplify analysis and to obtain better mass resolution we only used dipole magnets with vertical bending to decouple angle and momentum.

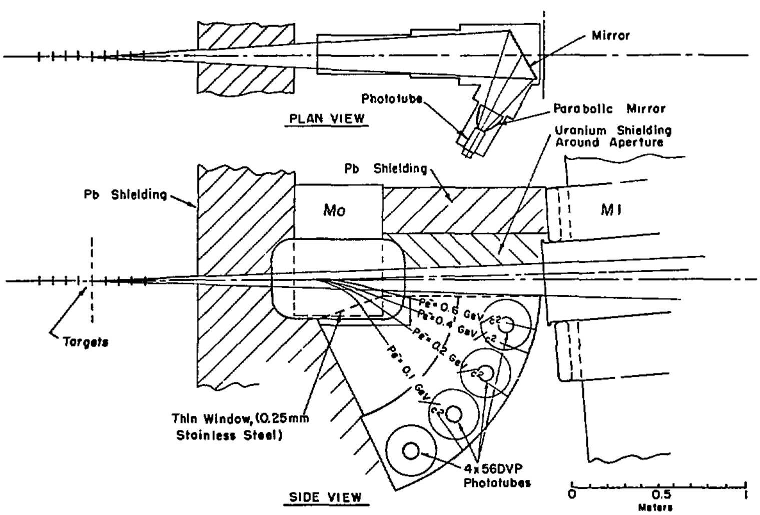


The MIT/BNL two-particle spectrometer used at the AGS to discover the new meson of mass 3.1 GeV.

To insure that the spectrometer could indeed handle the high rates and had the desired mass resolution, we put in 11 planes of proportional chambers in each arm, a total of 10,000 wires which provided a space resolution of mm and a time resolution of 50 ns. Time resolution was improved to 2 ns by installing thin (1.6 mm) hodoscope banks behind the chambers. The chambers were rotated 22^{o} with respect to each other so as to be able to sort out 7-8 tracks simultaneously. To insure that the chamber would be able to stand a high radiation level, we made tests with various gas mixtures, and finally one mixture was found to stand high radiation.

The hydrogen Cerenkov counter in the magnet had a large spherical light collector with a radius of 1 m. This was followed by another gas hydrogen Cerenkov counter behind the second magnet with an elliptical mirror of size 1.5 x 1.0 m. These Cerenkov counters are the most crucial part of the experiment: they give a π/e rejection of $10^{4} - 10^{5}$ in each arm. The 2 Cerenkov counters were filled with hydrogen, so that the knock-on electrons from pions, which would give a false signal, was reduced to the minimum. The separation of the 2 counters by strong magnetic fields insured that the small amount of knock-on electrons from the first counter was swept away and did not enter into the second counter. To reduce multiple scattering in these counters, the mirrors were ~3mm thick and to avoid large angle Cerenkov light reflection the mirrors were made out of black lucite. Through the help of my good friend Marcel Vivargent, we were able to have these mirrors made at the precision optical shop at CERN.

To properly control the $\pi^{o} \to \gamma\ e^{-}e^{+}$ decays, which were the main background, we designed two pair spectrometers (one in each arm), using a small magnet together with a pie-shaped Cerenkov counter detecting in coincidence with the main spectrometer the $e^{+}e^{-}$ from π^{o} decay. This counter was very directional, with a black wall to absorb non-directional light, and was filled with isobutane at one atmosphere so that it would count electrons down to 10 MeV/c but pions at about 3 GeV/c. The Čerenkov light in this counter was collected by four elliptical mirrors and focused onto four 2" tubes. The location of this counter was only ~2m away from the target where 10^{10} - 10^{11} particles were produced. The proper functioning of this counter was our main worry during the construction of this experiment, as there was a good chance that the counter would be buried by background and would not work at all. This counter was not only essential for π^o rejection but conversely, by triggering on this counter, it provided a clean electron beam into the main arm of the spectrometer for calibration purposes.



Counter arrangement to measure π^o decay. The counter was located 2m away from the intense proton beam of 10^{12} per pulse.

To further reduce the knock-on problem and to serve as a redundance check on our rejection against pions, we installed 70 lead glass counters at the end of the detector.

From the summer of '72 to the summer of '73 we constructed all the detectors, wrote a Monte Carlo analysis program and also began to estimate the soft neutron background which might trigger our proportional chambers and counters. We found no reliable estimated on this problem.

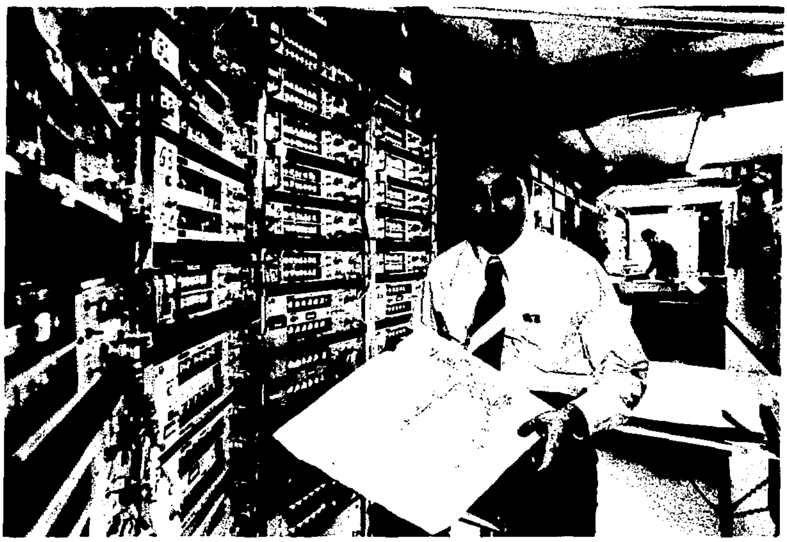
In the fall of '73, Dr. Y.Y. Lee of Brookhaven joined us and designed an excellent intense proton beam for the experiment. We began setting up the experiment on the floor and soon realized we needed 10,000 tons of concrete for shielding. This was solved by borrowing all the shielding from the Cambridge Electron Accelerator which had just closed down. To reduce the background of soft neutrons, we bought 10,000 lbs. of borax soap and placed them around the magnets and Cerenkov counters. Everything went very smoothly until:

BANG!

In late December of 1973 I went to DESY to discuss with Dr. Rohde the progress of his spectrometer. On the night before my return to Brookhaven, we had a traditional Christmas party in my office. Just as we were about to sit down to eat, I received an overseas phone call from J.J. Aubert, a very gifted French physicist from Orsay, who was spending a year with us. He said that there had been an implosion of the mylar window on one of the large Cerenkov counters during the process of testing. The force of the implosion was so strong that all the mirrors were broken to pieces of a few cm^2 in size and the implosion could be heard over the whole AGS floor. It was just fortunate that no one was near the counter at the time and no serious personnel injuries occurred.

Following the implosion we made an investigation but could find no reason why it happened. We could not repeat the implosion under identical conditions. Nevertheless, it was decided to remachine all the contact surfaces between the window and mylar foil and install shutters around these Cerenkov counters during the pumping down process.

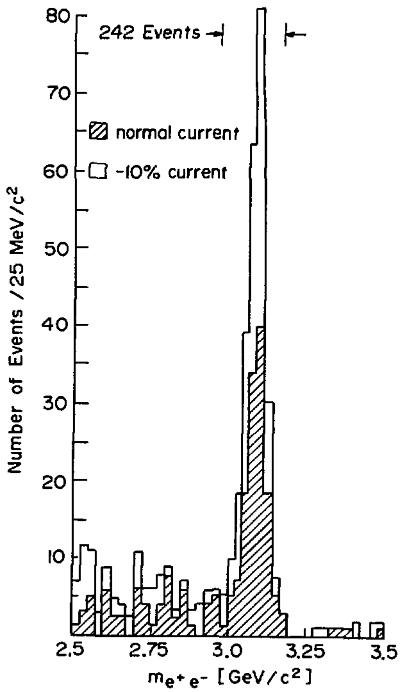
In April of '74, we finished the setup of the experiment and started bringing an intense proton beam into the area. We found that the radiation level in our counting room was 200 mr/hr. We looked very hard for a period of 2-3 weeks, could not find out why and became extremely worried as to whether we could proceed with this experiment at all.



Counting room for this experiment.

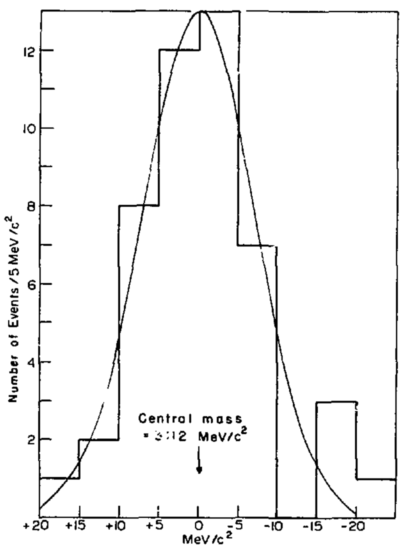
One day Becker was walking around with a Geiger counter and suddenly noticed most of the radiation came from one particular place in the mountains of shielding. Upon close investigation we found out that even though we had 10,000 tons of shielding and everywhere were blocks of concrete, the most important region, the top of the beam stopper, was not shielded at all. After this correction, the radiation level went down to a minimum and we were able to proceed with the experiment.

From April till the end of July of '74 we did the routine tune-ups and found that the detectors performed as designed. We were able indeed to use 10^{12} protons and we took some data in the high mass region of $4<m<5$ GeV. The small pair spectrometer also functioned beautifully and enabled us to calibrate the detector with a pure electron beam. However, analysis of the data showed very few real electron positron pairs. In August we started again and this time we went to masses between 2.5 and 4.0 GeV. Immediately we saw genuine electron pairs and furthermore they all peaked at a mass of 3.1 GeV. Before we had the time to investigate the nature this peak, we ran out of our scheduled time.



On-line data from August and from October, showing the existence of the J particle.

At the beginning of October, in order to make sure that we would receive priority on the machine scheduling, I informed a few people at Brookhaven about the existence of a sharp peak at 3.1 GeV. By the middle of October we had finished all the experimental checks and were convinced that the spectrum was indeed dominated by a sharp narrow peak and there were very few continuum events. Around the 17th. of October there was a symposium at M.I.T. in honor of V.F. Weisskopf and we had the opportunity to discuss our results with a few physicists. On October 22nd., Becker gave a seminar to the physicists at M.I.T. on the existence of the peak. We called this new particle the J particle, as J is the symbol used to denote electromagnetic currents and internal rotations, or spins, in nuclear physics. We also analyzed part of our runs in more detail and found that the width of the particle was less than 5 MeV.



Measurement of the width of J particle, showing that it has a width less than 5 MeV. Measurements at DESY, SPEAR and Frascati have indicated that it has a width of $\sim$100 KeV.

In the last week of October I began to hear many rumors about the particle we had found and received a few phone calls from Martin Deutsch, who has been our strongest supporter at M.I.T., saying that we should write this up and publish it. After I wrote the manuscript, I had a conversation with Dr. George Trigg on the new rules with regard to publications in Physical Review Letters, as we had previously had difficulties with regard to the length and style of our papers. There was still one question which very much puzzled us and that was the e/π ratio which was found to be of order of 10^{-4} at ISR and at FNAL. We had hoped that this new particle could give the explanation for such a large e/π ratio and decided to perform a measurement of the π -e rate and try to understand the production mechanism of the new particle. The measurement on single electron yield, however, turned out to be a much more difficult problem and took us a longer time than anticipated to complete.

On the 10th. of November I went to California to attend the Stanford Linear Accelerator Program Committee Meeting. As soon as I checked into the hotel at Palo Alto, Martin Deutsch informed me that he had heard rumors that the SPEAR Storage Ring had some new exciting results over the weekend, but he did not know the details. I placed a call to Stanley Brodsky of SLAC who had collaborated with me 10 years ago at Columbia and informed him about our results. Stan was very excited, but he did not tell me anything about the SLAC results. He told me that he would arrange for me to give a presentation at SLAC the next day. The next morning I went into Pief Panofsky's office and mentioned to him our results. Pief was very happy and informed me that similar results had been obtained at SPEAR by the Richter group.

In retrospect, the principal reason we were able to carry out such a difficult experiment was twofold. First, we had many years of experience in doing e^+e^- experiments at DESY and, secondly, we received the fullest support and cooperation from the B.N.L. management (from R. R. Rau to the staff engineers and technicians). We are grateful for the fine efforts and supportive spirit of everyone at the Laboratory which enabled us to pursue this difficult experiment to a successful conclusion.

The existence of such a long-lived particle with such a heavy mass was totally unpredicted and has since established a new field in particle physics. It has also clearly shown the importance of e^+e^- colliding beam accelerators in the study of this exciting new field.

After Dinner Speech

AGS 20th Anniversary Celebration

May 22, 1980

Maurice Goldhaber

Nowadays most of the talk concerning accelerators is usually about future accelerators. But we all can learn from history and it was therefore a splendid idea to take time out today to celebrate the 20th anniversary of this remarkably successful machine, the AGS. During about a dozen years or so it was one of the foremost machines in the world and made some of the most important discoveries in elementary particle physics, as we heard today. Quite unknown to the planners of this machine Nature has been very kind to us in putting so many interesting phenomena in an energy range where the AGS proved so suitable; and the physicists from Brookhaven and from the universities have been clever enough to get interesting answers to the questions they asked of Nature. We learned today about the intertwining of ideas between experiment and theory, between machines and detectors, and we must never forget the importance of all of these approaches.

Einstein once said that a scientist is an unscrupulous opportunist. This was not meant as a derogatory remark but rather to illustrate that scientists will make use of any opportunity which will allow them to understand Nature better. AUI and BNL considered it their duty to give scientists such opportunities to develop the potential for discoveries. This included not only the provision of machines but also often the provision of detectors, and especially in Brookhaven bubble chambers were built for everybody's use. We all owe great gratitude to Ralph Shutt who developed most of our important bubble chambers. It is up to the scientists how well they use the opportunities which are given to them. And as we have heard today the physicists made excellent use of the possibilities here during the last twenty years.

In deciding which experiments to accept at the AGS, we are confronted with the same dilemma which confronts all modern societies: to find a reasonable compromise between elitism and democracy; that balance can make the difference between destructive or creative tension. We here balanced elitism and democracy by inventing new administrative structures which, with some variations, have been copied all over the world. Some of my elitist friends have often accused me of being too democratic and some of my democratic friends of being too elitist. At least we cannot be accused of having disapproved an experiment here which then gave exciting results elsewhere. I am not sure whether Mel Schwartz will agree with this but he has mellowed enough, as I noticed today, to perhaps agree with this. I've often been asked what was the secret of the kind of experiments which I accepted. I was a bit shocked this afternoon walking back from the talks with one of our promising young physicists when he said, "Gee, I was really frightened. You were quite a dictator". Well, it wasn't quite so bad. Anyhow, the secret was that I accepted those experiments which I would rather have liked to do myself. But remember Brookhaven is not just a high energy laboratory, and finally when the high energy headaches became too big a fraction of the Director's headaches, I had to appoint people in charge of what you might call "associated headaches for high energies." first Rod Cool and then Ronnie Rau. For short, they are called Associate Director for High Energy.

When I first became Director in July of 1961, I was immediately confronted with a number of crises, not all of them had to do with the AGS. In fact, the first crisis which hit me within an hour after becoming Director had to do with the new Chemistry Building and I am only telling you about this because it is a typical crisis of elitism. Jake Bigeleisen who was then a member of our Chemistry Department in charge of their Building Committee insisted that the new building, though it was cheap, should not look cheap. He went directly over the head of the Director to the Chairman of the AEC who was his friend, Glenn Seaborg, which was very shocking to some people. Well, he won his point and the building is still with us and it still does not look cheap. A few years later when the building was being dedicated I used this occasion to bestow on him the equivalent of the old Maria Theresa Order, which was named after the Austrian Empress. Some of you may not have heard of this Order before. It was given to those who had succeeded against orders. It probably helped the old Austrian monarchy to survive for a century longer than it otherwise would have.

The next crisis which hit me was a much more serious one and it involved the AGS. Leland Haworth had promised the AEC to build the AGS for $30,000,000 as you heard today from Nick Samios. In this famous six-page letter which contained his promise and which was signed by Leland--in the copy which Nick had his name only typed in--and which was typed by my secretary, Anna Kissel (where are you--she should get up if she is here; I don't know whether you noticed there were the little initials "ak" there), Leland promised this but when the machine was finished I learned that a thousand dollars were left over. Nobody knew what to do with it. These thousand dollars plagued us for quite awhile until our financial geniuses found a solution which they never explained to me and the left-over money at last disappeared from the books. I wish George and Jim a similar crisis at the end of ISABELLE!

The next crisis which seemed to have an effect on morale arose from the fact that our sister laboratory as you have heard today, CERN, finished their machine about a year ahead of us. Immediately dire predictions were made that we will never find any cream. But milking a cow is an old and honorable profession. We had enough country boys here to know that when you milk a cow you also get cream. But more seriously, that fine balance between competition and cooperation which developed between Brookhaven and CERN again created that creative tension which was so useful to both our laboratories. In these days when the motto seems to be, at least among machine people, "whatever you can do I can do faster," don't be too afraid of competition. It is useful to remember this, and we learned this today. Thank you.

After Dinner Speech
AGS 20th Anniversary Celebration
May 22, 1980
C. N. Yang

Like George Vineyard I also will not make a long speech. Long speeches after dinner have the effect of putting people to sleep. There was a story about Mr. Li Hung-chang who was one of the last Prime Ministers of the Ch'ing dynasty in China before Sun Yat-Sen's revolution. Toward the end of the 19th century Li Hung-chang came to the United States on an around-the-world trip. He was about to buy a lot of naval equipment for China. So all the businessmen and local politicians descended on him in order to get some piece of the business. One day he was in Philadelphia and was given a big welcome party, during which the Mayor made a very long speech. Li Hung-chang was at that time a rather old man and he promptly fell asleep. When the Mayor realized this he stopped, and the silence woke up Li Hung-chang. The Mayor turned to him and said, "Your Excellency doesn't like long speeches?" When that was translated to Li, he said, "On the contrary, I like long speeches, during which I take long naps."

The AGS is undoubtedly one of the great accelerators that physicists and accelerator builders have produced. In fact, it is not an exaggeration to say that the history of our field in the last twenty years is, to a large extent, very much the history of the AGS, as the talks this afternoon vividly showed. Now the involvement with the accelerator is necessarily closer on the part of experimental physicists than theorists. A theorist is not as much married to the whims of the machine and does not suffer as much when it misbehaves. As a consequence, he also does not get the same amount of elation when it does work very well. But nevertheless, a theorist lives in the environment provided by the general development of the field, and that is of course very much influenced by what is coming out of the large machines.

Listening to the talks this afternoon there raced through my mind memories of the various periods in my own career when in various ways I was involved with or reacted to the developments that we heard. Allow me to share some of these memories with you. In the fall of 1952 I was in Princeton and heard about a very interesting paper about a strong focusing principle. I was in the midst of doing statistical mechanical calculations but since the strong focusing paper was quite easy to understand, I learned something about it and got deeply interested. For a period of several weeks I considered the problem of the resonances and how one could get across them. I had some ideas and thought about exploring them. The Institute for Advanced Study at that time had just finished constructing the world's first large computer, the predecessor of the JOHNIAC. I went to the computer project and talked to my friend Herman Goldstine to learn about how one could use the computer. That was before FORTRAN and the computer was a very complicated thing to use. One had to use machine language and I spent some time learning that. Then I made a little computation of how many machine orders I would have to write in order to make progress. After that little computation I decided I couldn't possibly write that many orders without mistakes. So that was the end of my accelerator design career. I would not say I regretted that I missed out on the later developments in accelerator design, but I did kick myself later for not having thought of the possibility of a program language like FORTRAN.

In 1959 at a time when there had been a lot of work about weak interactions, T.D. Lee and I were trying to see whether one could have additional leverage on the weak interactions. We had intensive discussions for a long time, but did not hit upon the right idea. The right idea was due to Mel, who pointed out that neutrino induced experiments were in fact feasible, although not easy. We all know that led to the important experiment on the AGS that Mel told us about this afternoon. If we reflect on the many neutrino experiments which were later done all over the world and the physics that came out of them, we would appreciate even more the importance of that essential idea.

In 1964, in the summer, I was visiting Brookhaven and heard the rumor that there was about to be a paper declaring that time reversal invariance and CP conservation were violated. That was something that very few people were inclined to believe for reasons which were quite natural: everybody preferred more symmetry than less. When parity conservation had been found to be not valid everybody seized on CP conservation as something which one could hang onto. So to be told that CP is not conserved was a great shock. We didn't ask Val this afternoon whether it was true that in order to make their bump disappear they had labored for so long precisely because they also believed that CP conservation should be not violated. But I suspect the answer would be yes. That summer after hearing about the CP experiment I put through some calls, if I remember correctly, both to Val and to Jim Cronin. Neither Val nor Jim is famous for being very talkative. But I did get some information over the phone and I knew that what they lacked in loquaciousness they more than made up for by credibility and reliability. So I began to work on the CP problem with T. T. Wu. In fact, for a few weeks it seemed that every theorist was working on this problem. Theorists are in general in a higher excited state than the experimentalists when some breakthrough takes place in our field. There is a reason for this. The theorist can speculate into the seventh heaven while the experimentalist is more tied to the ground. When an unexpected discovery is made, the theorists all go to work. Although most efforts turn out to be futile, they do provide great excitement. In the late sixties, I assigned a job to one of my graduate students, to look into the rate of papers published in theoretical high energy physics in the PHYSICAL REVIEW LETTERS and to ploc that rate against time. He reported to me that in the summer of 1964 the rate of theoretical physics papers published in the PHYSICAL REVIEW LETTERS jumped by a factor of 50%.

Nick Samios' and Sam Ting's report on their work on the AGS both recalled further exciting periods. For example, in 1974 at Stony Brook when we learned about the discovery of the J and then the Ψ's we all raised ourselves into a highly excited state. I remember vividly continuous bull sessions in our small group which made a lasting impression not only on the faculty members but also on the graduate students. In several of the courses the lecturer switched to discussing the newest data rather than following the regular course work.

I said earlier that the history of the AGS in the last twenty years is, to a large extent, very much the history of our field. In celebrating the twentieth anniversary of this great accelerator the question naturally arises as to the status of high energy physics in the next twenty years. I for one believe that undoubtedly there will be great mysteries revealed and great discoveries made. Physics is a continuing development of new wonders. But in this respect I would like to share with you two stories. What you make of them is up to yourself.

The first story was told by Eddington. Once there was a fisherman who was a very keen observer of nature. As he fished day in and day out he also observed and observed. After twenty years he formed a law of nature that all fish are longer than four inches.

His net was a four-inch net.

The next story was due to Galileo. It is a remarkable story which began, in translation, with the following passage. "Once upon a time in a very lonely place there lived a man endowed by nature with extraordinary curiosity and a very penetrating mind. For a pastime he raised birds whose songs he enjoyed." Then Galileo went on to describe how through such observations this man gradually learned how different birds make songs. Then he learned how the mosquitoes make sounds, how crickets make sounds and he became a great expert in this field.

Now I read again: "Well, after this man had come to believe that no more ways of forming tones could possibly exist...when, I say, this man believed he had seen everything, he suddenly found himself once more plunged deeper into ignorance and bafflement than ever. For having captured in his hands a cicada, he failed to diminish its strident noise either by closing its mouth or stopping its wings, yet he could not see it move the scales that covered its body, or any other thing. At last he lifted up the armour of its chest and there he saw some thin hard ligaments beneath; thinking the sound might come from their vibration, he decided to break them in order to silence it. But nothing happened until his needle drove too deep, and transfixing the creature he took away its life with its voice, so that he was still unable to letermine whether the song had originated in those ligaments. And by this experience his knowledge was reduced to diffidence, so that when asked how sounds were created he used to answer tolerantly that although he knew a few ways, he was sure that many more existed which were not only unknown but unimaginable."