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History of the ZGS 500 MeV Booster

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High Energy Physics Division

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History of the ZGS 500 MeV Booster

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by
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Abstract

The history of the design and construction of the Argonne 500 MeV booster proton synchrotron from 1969 to 1982 is described. This accelerator has since been in steady use for the past 25 years to power the Argonne Intense Pulsed Neutron Source (IPNS)

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Editor's Introduction

Tom Fields, ANL - retired

This document began when Jim Simpson decided (in retirement!) to write up an informal retrospective account of the 1970's project which he led to build a 500 MeV Booster injector for Argonne's 12 GeV Zero Gradient Synchrotron (ZGS) accelerator. Among his motivations were first, to document crucial contributions which were made by particular individuals and second, to describe the difficult technical decisions which were required to carry this project to successful completion in a situation of decreasing funding and ever-thinner staffing.

A broader motive of Jim's was to better document the origins of this Booster accelerator which later became the heart of Argonne's Intense Pulse Neutron Source (IPNS) and continues in that role (as the "RCS") today. Argonne's IPNS program has been and still is one of its most original, productive, and longest-running user research facilities. Consequently, this report should have some historical interest and value. (The American Institute of Physics book "History of the ZGS" [1] was written in 1979, before the ultimate outcome of the project to build the 500 MeV Booster could be known.)

After reading Jim's account of this crucial Booster project, I suggested that we make it into an Argonne report, and combine it with a few other accounts, if we could find willing writers. Doing so has added more details, some important additional perspectives, as well as a summary of the performance of this grand old accelerator during the past 25 years.

One of my qualifications for editing this report is that I was Argonne Associate Director for HEP during the years 1974-1977. In that role, I was deeply involved in a number of difficult high level decisions about Booster project issues. As described in this report, we all resolved to keep pushing ahead, despite the ever-increasing likelihood that the ZGS would be shut down before the Booster could used as the ZGS injector. The Argonne Director, Bob Sachs (who had himself previously been in charge of the ZGS program), gave us strong support in these challenging decisions.

A few key documents which describe some overall aspects of the Booster and RCS projects are listed in the References section. I believe that this present report gives a much fuller description of the Booster project than has been available previously. We have done our best to be accurate, but no doubt a few errors and inconsistencies remain.

On behalf of Argonne, I thank Jim Simpson, Ron Martin, and Bob Kustom for their dedicated work (and memory and notebook searches!) in creating this report.

Chapter 1

History of the ZGS Booster (1969 – 1978)

Jim Simpson, ANL-HEP (Retired)

The cornerstone of our 1970's program to raise the beam intensity of Argonne's Zero Gradient Synchrotron (ZGS) was to design and build a 500 MeV rapid cycling proton synchrotron. That 500 MeV accelerator was originally called the Booster, and later renamed the Rapid Cycling Synchrotron (RCS). It lives on to this day as the essential engine of Argonne's highly successful and productive Intense Pulsed Neutron Source (IPNS) program.

In this note, I describe, in almost chronological order, the history of the Booster from its inception to the time that the machine was turned over for final commissioning and for use in the IPNS. I will explain when and why certain designs and features were adopted or omitted, and will identify many of the key the people who participated in the design and construction of the machine. I have no doubt forgotten to mention a few names, and for that I apologize.

H^- Injection and Booster I

Many of the high energy physics experiments which were using the ZGS would benefit greatly if the intensity of the 12 GeV ZGS proton beam could be increased. A "sure thing" solution to this intensity challenge would have been to increase the injection energy, which was then limited to 50 MeV by the injector linac. That would reduce the space charge tune shift at injection. (See Chapter 2 by Ron Martin for more details about various ZGS intensity issues as they appeared at that time)

In the mid 1960s, Lee Teng, Ron Martin, and a few members of the ZGS Division's Accelerator Physics Group (notably Ed Crosbie, Larry Ratner, and Tat Khoe) studied the possibility of constructing a booster injector accelerator (synchrotron) to raise the injected beam energy to 500 MeV. They concluded that existing synchrotron designs were marginal for this purpose. However, as anyone who knows Ron Martin can imagine, the quest didn't stop there.

In 1968, Ron picked up some very interesting news out of the Budker Institute, Novosibirsk, USSR. A group there had demonstrated negative ion stripping injection into a tiny storage ring where they achieved a large increase in phase space density. In effect, they circumvented the Liouville Theorem by introducing a non-reversible interaction (charge stripping) into the injection process. Ron attended an all-Soviet accelerator meeting, learned more about the experiment, and returned with new enthusiasm for a booster injector at the ZGS.

About that time, in 1969, Bob Wilson's 2 GeV electron synchrotron at Cornell was decommissioned and became available. Ron surmised that the Cornell synchrotron, with some relatively minor modifications including the use of stripping injection, could be transformed into a 200 MeV proton accelerator. It could then be used to provide definitive studies and demonstrations of the new injection scheme, and perhaps it might even become a booster injector for the ZGS.

Ron assigned the task of moving the Cornell machine to ANL and re-birthing it, with the necessary modifications to a 200 MeV proton machine, to Dave Nordby. Dave had only a few dedicated helpers, perhaps three or four. Far too few, at any rate, to tackle such a task and expect to complete it in any reasonable time frame. Nevertheless, by 1970 Dave and his team had placed the ring magnets into approximate positions on the floor of the new "Booster Building", unboxed the vacuum chambers (a generous description of the chambers), installed the large filter choke (an old 300 MeV electron synchrotron magnet), and ordered a new PDP-14 control computer to control the vacuum and radiation safety systems. (If the reader believes it was easy, when the time came, to convince a radiation safety committee that it made sense to control things such as personnel access to the inside of the accelerator enclosure with a PDP-14, he or she has never dealt with a radiation safety committee.)

Nordby left Argonne in 1970 for a new job with a pharmaceutical company in the Chicago area. About that same time, I completed my commitment to the 12' Bubble Chamber project by managing startup and commissioning of the chamber. I had been offered a position in the Fermilab 15' Bubble Chamber project and was close to accepting it when Ron Martin proposed I lead a new group, the Booster Group, that he was going to form within the ZGS Division. I accepted the offer.

Our new group consisted of about eight people. Off hand, I can remember Ken Menefee, Ed Burrill, Mace Lieberg, Fred Featherstone, Vince Patrizi, Jerry Volk, Tom Hardek, Roger Sanders, plus maybe two more as the principals in the original group. This brave band then charged ahead to make Booster I a reality. Well, reality quickly set in!

After setting surplus ZGS area shield blocks in place and aligning the ring magnets as best we could, we installed the vacuum chambers and diffusion pumps. The vacuum chambers supplied with the machine had been constructed by soft-soldering ceramic plates, each about 4" wide by 10" long to rippled stainless steel sidewalls. The inside surfaces of the plates had plated-on copper strips to provide beam charge shielding, and the edges were metalized to permit soldering.

The plates were also unbelievably fragile. Many leaks had developed during shipping and handling. It was impossible to obtain 10^{-5} Torr pressures even at the straight section diffusion pumps. Re-soldering and copious applications of glyptol provided short-term relief from vacuum problems; but even the slightest stress would open new leaks. Furthermore, we discovered by pulling a contact probe through chambers that many of the plated strips were electrically isolated, i.e. floating. Measurements also showed that the ring magnets were all twisted in some complicated way and could not be straightened. The warnings that the Cornell people had given us about the frailty of the vacuum system (and even the magnets) were indeed true.

Andy Gorka, assisted by Ken Kellog, produced a remarkable piece of machinery to manipulate the thin stripping foils that Chuck Krieger produced in the Plastics Shop. Resembling something from "The Time Machine", it consisted of a large (about 3 foot diameter) G-10 disk rotating synchronously with the 15 Hz ring magnet power. A number of stripper foils, each about 1 ½ " by 4" were attached by one end at the periphery of the disk The other end of each foil was then fastened radially inward after first being placed over a circumferential nichrome wire about 4" in from the outer radius.

At the hub of the disk, Andy had a rotating transformer core and winding which could be excited by a similar fixed core and winding. When the fixed core was excited, it pulled the rotating core, which was attached to a mechanical stepping mechanism, toward it about ¼". This had two results. The first was that electrical contact was established to a new section of nichrome wire (mentioned above). The second was that the transformer current heated the selected section of wire, cutting a single foil and thereby releasing a new stripping foil to be unfurled at the edge of the rotating disk. This mechanical approach to have the stripper in the injected beam path for only a short period was deemed necessary because the short straight sections of the machine would have made a fast orbit bump difficult. The stripper mechanism was remarkably reliable in spite of its complicated design.

An rf accelerating system was designed and built by Al Moretti and John Dawson. Unfortunately, the final amplifier had a nasty habit of self-oscillating; not a good thing for acceleration.

Stu Markowitz designed the beam line to transport the H^- beam from the linac to the booster injection area. We constructed the line as designed and had relatively little difficulty commissioning it. Jerry Volk designed and built the machine's control system, interfacing it to that of the linac's. It was straightforward thumbwheel-set time pick off from a digital clock, and served its purpose well.

John Fasolo built an H^- ion source by coupling a duoplasmatron proton source to a neutral gas charge exchange channel. It was a large contraption. Typical output was about one or two milliamps at 30 KeV.

By 1972 we were ready for beam tests. What a chore! Typically, a period of about two weeks would be allotted for booster test about two times a year. During that period the H^- source had to be installed in the HV terminal and made to work before any beam was available to the booster ring. From the very beginning, coasting beam lifetime was very short except for very weak currents. We performed orbit studies, tune measurements, and so forth. All indications were that the poor vacuum was the problem. But given the condition of the vacuum chambers, that was an impossible problem to solve. We tried, but were not successful.

In desperation and without consulting Ron Martin beforehand, I had stainless chambers fabricated in Argonne's shop. I placed a thin stainless strip along the bottom surface of each chamber, insulated by mica. This allowed the application of DC clearing fields inside the chambers if needed. I also had printed circuit sextupole windings fabricated and attached (glued) to the ring magnet pole faces. These were to compensate anticipated sextupole fields produced by eddy currents in the vacuum chamber. Solid-state amplifiers individually powered these. Ron was not happy when I told him about the chambers. On the other hand, the clearing fields turned out to be required and we were then able to coast and accelerate beam… sort of.

But the overall state of affairs was not good. Although we had demonstrated the effectiveness of H^- stripping injection, there were several serious difficulties which prevented further progress with the Booster I project:

- The ring magnet cores were twisted and had generally poor field quality.
- Magnet alignment could not be maintained for any length of time due to the expansive soil on which the booster building's garage quality slab floor had been poured.
- The rf system was unreliable.
- The Booster I project was often not well supported by the mainstream groups in the ZGS Division (Linac, Operations, and Controls). But we did receive valuable help from Vern Stipp and Marty Knott, in addition to the individuals mentioned above.

Nevertheless, to a large extent as a result of our Booster I results and developments, the ZGS adopted the use of stripping injection as its normal operating mode in 1973. It was the first high energy accelerator to do so. About that same time, Ron Martin decided to again pursue the design and construction of a 500 MeV injector synchrotron.

Booster II

A new injector synchrotron, called Booster II, was funded mainly as an Accelerator Improvement (AI) project for the ZGS. The projected schedule and cost estimate were certainly optimistic. However, since the ZGS program was facing likely termination by ERDA in just a few years, only an optimistic yet bare-bones approach was possible.

Martin Foss designed the machine's lattice using his own software written in a high-level code (SpeakEasy?). The racetrack ring incorporated FDFODO cells with two matching straight sections to accommodate injection and extraction hardware (bumps, strippers, kickers, etc). Improvements to the design for features such as sextupoles, orbit correctors, and tune shifters were later done using a code, "MINISYNCH", which I wrote in DEC Basic for a PDP-9 I salvaged from the 12' bubble chamber after it was decommissioned. I also acquired John Staple's (LBNL) code "LATTICE" which had been ported to Basic by a student at LANL. It was interesting in that Dick Cooper (LANL), whose student did the porting, liked my program and I liked his; we swapped.

Ken Thompson and Jim Bywater, with sage advice from Foss, did the mechanical and electrical designs for the two types of magnet structures, an FDF and a D. They used curved, laminated steel cores and several (four as I recall) parallel turns for windings. This winding configuration was necessary to keep the inductance to an acceptable value. Ken was very careful to assure that each paralleled turn enclosed the same amount of magnetic field flux; quite an achievement.

We awarded the magnet fabrication bid to Alpha Scientific in California after a site visit by Frank Brumwell and me convinced us that the company was capable of handling the job.

Unfortunately, we had to cancel the contract when it became evident that the company could not meet our contracted requirements. After a negotiated settlement, all materials and fixtures were moved to a high bay building at ANL where we proceeded to undertake the fabrication in house. Later, we concluded that the contract was probably too constrictive for it to have been met by any fabricator. For instance, we discovered that the amount of core compression specified could not meet the density requirement without using partial laminations. With the dedicated efforts of Jim Bywater, Jim Biggs, Ken Thompson, and the assistance of Frank Brumwell and a crew of technicians, magnet fabrication went forward at a good pace.

The inclusion of beam field shields, sometimes called "liners", has a particularly interesting history, which I shall detail below. These liners may have later become a misunderstood component of the Booster.

We had opted on a ring magnet design whose vacuum shell was exterior; i.e. no traditional beam vacuum chamber. This was similar to the Fermilab booster's magnets, and it seemed like a good way to circumvent the eddy current problems that an internal beam tube would present.

However, late in our magnet production, Sandro Ruggiero (FNAL) investigated a problem in the Fermilab booster whose signature was an unexpected energy loss by bunched beam. He developed a model in which the laminations formed cavities whose widths were determined by the insulating coating on the laminations plus residual gaps due to incomplete compression. Image current of a bunched beam in the machine produced cavity excitation and hence the observed energy loss. Because the Fourier spectrum of the beam pulse was very low compared to the characteristic frequency of a typical cavity, the result was essentially the same as if one simply computed the i^2R skin losses of the image current running in-and-out of the cavities.

Bob Kustom calculated, and Al Morreti measured in the lab, the corresponding wall impedance of our magnets. I then used that information to calculate what the effects would be on the shape of booster rf buckets. The result was sobering. I showed that at B-dot equal to zero in both the Booster and the ZGS, the rf buckets could not be made to have more than about 50% overlap. I could not find any parameters (rf voltage, B-dot, etc.) to significantly improve the situation!

Martin Foss then proposed that we install a pulsed induction core coupled to the Booster beam to provide betatron acceleration for a short time near beam transfer. The core would have had about 1 m^2 area, and would have had complicated controls to compensate for various bunch currents, etc.

Meanwhile, Tat Khoe looked at the possibility of installing liners around the beam to shield the exposed laminations from beam fields. Several configurations were examined from the standpoint of eddy current effects (most notably sextupole fields). He also attempted to at least qualitatively evaluate microwave characteristics of various liner configurations. We eventually decided to use a design in which a thin-walled stainless tube is squeezed to a roughly elliptical cross-section and then sliced from one side almost to the other side at roughly one-inch intervals. Tubes such as these were then inserted into the finished magnets, held in place by the springiness of the tubes.

Because the Booster was at that time still being designed to be an injector to the ZGS, it was essential that the wall-loss problem be solved. This was of special importance since Bob Woods, the DOE overseer for the project, was well aware of the Fermilab booster problem and of the similarities between our magnet design and that of the Fermilab booster.

Shortly into this construction phase of Booster II, Ron Martin imposed a then-popular management style upon the project that he believed would alleviate the cooperation problem mentioned earlier, and thus lead to better support of the booster project by the major ZGS groups. Everett Parker was to be responsible for coordinating manpower allotments to the project while Bob Kustom was to provide technical and engineering guidance. I was to oversee the installation, be responsible for machine physics issues, and for machine research. At this point allow me to quote from a well-known text: “No man can serve two masters…” I’ll call that the “2M” effect and make later reference to it.

Although magnet construction was proceeding well, design and construction of the new rf system was stagnant, and the 2M effect came quickly to a head. Ultimately, I insisted that responsibility for the new rf system be assigned to a bright young engineering assistant, Tom Hardek. Tom was an amateur radio enthusiast who knew rf theory and high-power rf fabrication practices very well. Tom addressed a problem common to most ferrite loaded accelerating structures; how to keep the gap shunt impedance low. His solution was a cathode follower final amplifier that used transformers with coaxial windings. The amplifier worked beautifully. John Forrestal and Roger Hogreffe helped with the fabrication. Tom would later give well-received talks about his design at the Particle Accelerator Conferences and at seminars at other laboratories.

I designed beam optics to match the existing transfer line from the linac to the machine’s injection point and had it assembled from existing hardware. I also simulated the use of injecting beam which was “chopped” to better match the booster rf buckets in order to hopefully have more efficient injection. Those simulations showed that buckets actually formed by accretion of beam occupying the full circumference of the machine and, therefore, that chopped injection would have little advantage over continuous injection.

Walter Praeg designed and had fabricated a new ring magnet power supply. Like all of Walter’s designs, it worked well.

Ron Timm was responsible for a new control system. Unfortunately, and due to the 2M effect, I was unable to dissuade him from using TTL logic level (5 volts) links between the control room and equipment in the booster building in spite of the presence of strong electrical noise in the vicinity.

The problem of booster building floor motion was circumvented by mounting the ring magnets on girders supported by poured concrete columns extending through the building floor down to undisturbed, compacted soil. Care was taken to insure that the columns were mechanically isolated from the floor slab. Special shield blocks were ordered to allow the ring’s shielding to better conform to the ring curvature than would have been possible using only rectangular ZGS style blocks. Because the booster was (at least on paper) being built as an injector for the ZGS, we constructed a beam line to transport beam toward the ZGS. This line was also used for pulsed neutron production studies, which I will not discuss here.

Despite Bill Bryan's bookkeeping skills and Ron Martin's best efforts, money and time for the machine's construction became so tight that we were force to omit something(s) in the project. The most notable something was a sextupole correcting system.

The ZGS made major changes in its operation during this period of booster construction as well. In 1973, the ZGS became the first "real" accelerator to adopt H^- stripping injection as its standard mode. The booster's experience with stripping injection was certainly a major justification for the decision. Of no less importance was the development of a new type H^- source by Dimov and Dudnikov (Novosibirsk). Fermilab had made further improvements to the Novosibirsk source design. Consequently, our H^- source had become a reliable and well-performing device.

In addition, the ZGS was devoting ever increasing running time to its polarized proton mode of operation. A new additional HV preinjector was installed allowing relatively painless switching between polarized proton and H^- acceleration in the linac. Interleaving occasional booster testing with ZGS operation nevertheless continued to be difficult. A major factor in that difficulty was that the booster remained a relatively low priority activity within the Division as a whole (2M struck again).

Commissioning the Booster

Initial commissioning of Booster II was very encouraging. Coasting beam lifetimes were reasonable, and acceleration was achieved. There were, however, beam loss problems during the acceleration cycle. My suspicion was, and still is, that the beam shield liners contribute to microwave instabilities in the beam. As I previously implied, it was difficult to estimate with any precision the microwave properties of the liners we chose, and so we just used our best judgment to make a choice.

My frustrations with problems produced by the management organization described above became obvious to everyone. Thankfully, Ron Martin relieved me of responsibility for the booster project, allowing me to assume an active role in the anti-proton (pbar) production and cooling work which was getting underway at Fermilab. I became head of the pbar study group at Fermilab, conducted stochastic cooling experiments in the Fermilab cooling ring, developed Argonne's beam pickup test facility, and initiated the Argonne Advanced Accelerator Test Facility and the Argonne Wakefield Accelerator projects.

After operation of the ZGS was terminated, the linac and booster facilities were transferred to the IPNS Division. Yanglai Cho and Jim Norem each participated separately in final commissioning work on the booster. I was told that there did, in fact, appear to be microwave beam instability problems. This was not surprising, given the liner history. With additional money made available, a previously deferred sextupole correction system was installed.

I recommended to Charlie Potts and later to Frank Brumwell that the liners be removed. Not only are they the most likely source of microwave frequency wall impedance, but they also restrict the physical aperture available to the beam and introduce vacuum pumping impedance to the beam region.

Final Comments

The IPNS program has been very productive in its many years of operation. Booster II, later named the RCS, remains a major component of the IPNS facility. I am proud to have played a key role in the realization of the machine.

The ZGS program and, in particular, Ron Martin, have never received appropriate recognition for such outstanding contributions to accelerator research and technology. Had it not been for the pioneering development and application of H^- stripping injection, pushed by Ron, in the booster and in the ZGS, it is unlikely that the technique would have been so quickly adopted at other accelerators. Other notable accelerator achievements such as simultaneous resonant extraction and polarized proton acceleration were also carried out at the ZGS.

Chapter 2

Early History of the Argonne 500 MeV Booster

Ronald L. Martin ANL-retired

ZGS Intensity Challenges

It is important to understand the competitive feeling among the high energy physics accelerator laboratories in the early nineteen sixties, particularly as regards the circulating intensity of their accelerators. The CERN PS came on line in late 1959. The Brookhaven AGS followed a few months later in early 1960. Within three years both were operating at nearly 1e12 protons/pulse (both machines having 50 MeV linac injectors and alternating gradient rings).

The ZGS was later, but was expected by some to yield higher intensity because of its large aperture since it was a weak focusing machine. The ZGS also used 50 MeV injection. Lee Teng had estimated the intensity limit of the ZGS to be about 5e12 protons/pulse (ppp). So it was something of a surprise that alternating gradient rings with such small apertures could do so well. I think it was at this same time that some mention was made of 1e13 ppp as an ambitious but reasonable goal for the ZGS.

When the ZGS did begin operation in August 1963 it ran into a serious instability (the vertical resistive wall instability) at an intensity of 3e11 ppp. A collaboration was formed between accelerator people at Argonne and the MURA group at Madison (and particularly Fred Mills and Don Young) to develop an active damper for the ZGS to overcome this roadblock. It took more than two years and five equipment modifications to be successful. When this work was completed, the ZGS intensity reached 2.5e12 ppp. For a short time (about a year), the ZGS was the most intense on a per pulse basis of the three synchrotrons.

Injector Upgrade Possibilities

Programs were initiated at each of the laboratories to improve their accelerator performance. It was known that the limiting intensity was proportional to the linac energy and a national study group (including physicists from Brookhaven, MURA, Argonne, and perhaps Los Alamos) was formed to design a 200 MeV proton linac.

The Argonne-MURA group also studied other options for improving the ZGS intensity. One of the concepts studied was a 500 MeV rapid cycling synchrotron injector. The latter, however, was not considered a promising way, with existing technology, to increase the intensity of the ZGS. A 200 MeV linac injector at a cost of $20M was proposed as the most practical way to improve the ZGS intensity.

Argonne submitted this linac proposal to the AEC in 1966 along with proposals for two other ZGS projects: a superconducting magnet 12 ft hydrogen bubble chamber ($13M), and a second proton experimental area ($4M). Argonne was given the choice of either the linac or the other two projects. Robert Sachs, Associate Laboratory Director for High Energy Physics, chose the bubble chamber and second proton area. That decision pointed the ZGS toward becoming a highly productive user facility with many simultaneous beam lines and innovative detector and magnet development. I think that the ZGS was quite successful along these lines.

In the same year Brookhaven received $50M for a 200MeV linac injector and a new ring magnet power supply to double the repetition rate of the AGS. I think that commissioning problems lasted about a year after construction was completed, and then the AGS intensity exceeded 1e13 ppp. From my perspective, it appeared that the ZGS would no longer be involved in a serious intensity competition.

As an aside - CERN chose to upgrade the PS with four 1 GeV synchrotrons as injectors (with magnets stacked one on top of another vertically) and the PS also exceeded 1e13 ppp. I don't recall the dates of these CERN developments.

H^- Charge Exchange Injection

In view of the above developments, one can understand my elation on my first visit to the Soviet Union in Nov. 1968. There I saw at Novosibirsk a 16mA H^- ion source and a demonstration of H^- charge exchange injection by Gennady Dimov. He used a two stage gas stripping technique to store 1e10 protons in a small ring at 1.5 MeV. While this is not an earthshaking intensity, it was most impressive at such a low energy. It clearly demonstrated the validity of the charge exchange concept to avoid the limitations of the Liouville Theorem at injection.

What it meant to me was that if one could develop practical foil stripping at 50 MeV , then a 500 MeV rapid cycling synchrotron could be a useful injector for the ZGS. Furthermore, it could be built at a small fraction of the cost of a 200 MeV linac -- just what was needed at Argonne to improve the intensity of the ZGS beam.

Dimov was the leader of the ion source group at Novosibirsk, which included another physicist by the name of Dudnikov. Dudnikov has become well known worldwide by extending the H^- ion source development to exceed 100mA. I brought back with me in late 1968 the design details of the Dimov-Dudnikov ion source and took them to our source expert, John Fasola. John was adamant that he could do as well with H^- ions by modifying his source technology, the duoplasmatron. (I think that John was the inventor of this type of source, which works well with protons).

As a result we had only 1/4mA of H^- when we did the first test of charge exchange at 50 MeV into the ZGS in 1970. The intensities were too low to be useful and there was only one foil in the ZGS so a shutdown would be required to change foils. The results, however, were very encouraging and we could extrapolate to 10mA of H^- and a foil changing mechanism inside the ZGS.

Andy Gorka designed and built a foil changing mechanism for the ZGS with much different restrictions than those for the booster. I've always thought of his ZGS device as a stroke of genius, typical of Andy Gorka.

This device, together with our new source of H^- of about 10mA, made possible charge exchange injection into the ZGS at 50 MeV in 1973. It became the normal operating mode of the ZGS. The intensity quickly rose to 6e12 ppp, justifying Lee Teng's calculations of many years before. In addition to increased intensity, the reproducibility on a pulse-to-pulse basis, even after downtime due to equipment failure, was quite remarkable and not anticipated. This effect, apparently due to relaxation of injection conditions, was also observed at other laboratories later.

The success of charge exchange into the ZGS, however, did not eliminate the need for booster injection, whose goal was to exceed 1e13 ppp. Back in early 1969, we designed a 500 MeV rapid cycling synchrotron with H^- charge exchange injection for use as a new injector to the ZGS. Consultants in writing the proposal were Lee Teng and Stan Snowden (Fermilab), Fred Mills (U. of Wisconsin), and Martyn Foss (Carnegie-Mellon U.). [Martyn Foss, who was the principal designer of the ZGS magnet, later rejoined the Argonne Accelerator Division and designed the Booster II magnet.]

Chuck Krieger, the leader of our plastics shop, said he thought he could make foils of 25 micrograms/cm2, adequate for stripping H^- ions to H^+ at 50 MeV. The synchrotron proposal was submitted to the AEC in June 1969, only six months after origination of the concept. The estimated cost was $5.3 M but the proposal was turned down.

In the meantime we needed to begin our development of H^- ion sources, foils for charge exchange, and prove that this was practical at 50 MeV. I called Boyce McDaniel at Cornell Univ. to ask about the 1 GeV electron synchrotron I had worked on from 1953-56. (This was the first of the alternating gradient synchrotrons, although Brookhaven had operated an electrostatic model before building the AGS). Boyce said that magnet had been thrown away, but that the successor, a 2.2 GeV electron synchrotron, would be decommissioned in 6 months. Thus began the saga of the Cornell machine, modified for 200 MeV protons at Argonne.

Booster I

The Cornell machine, which we often called Booster I, was not intended to be a new injector for the ZGS. Our purpose was to demonstrate the practicality of the charge exchange injection concept at 50 MeV, that is: foil lifetime and replacement, reliability of H^- sources, and quantitative emittance enhancement. In spite of all the difficulties it did succeed in these goals, thanks to the heroic efforts of Jim Simpson and many other people. In addition, the existence of the 200 MeV proton beam led to Jack Carpenter's initial experiments on spallation neutrons (from protons) and to my own work in demonstrating the advantages of protons for medical diagnostics.

Other results of the Booster I project were also extremely important. We needed a building to house the ring. That was provided by the Laboratory as a General Plant Project. We needed a transport line from the linac to the synchrotron. That was paid out of Accelerator Division Equipment funds. We needed minimal diagnostics, controls, etc. These came out of Accelerator Improvement funds. When the 500 MeV Booster II project was built, all of these systems were utilized. The Booster II project could not have been carried out without them.

Booster II

Only when the practicality of stripping injection at 50 MeV was demonstrated on the Cornell machine did we proceed with the construction of the 500 MeV Booster II. The multiple minimal funding sources meant that Bill Bryan had to keep very close tabs on all costs. A crisis came, however, when a contract was let with Alpha Scientific to build the Booster II magnets. The costs were to be comfortably below the $500K limit for Accelerator Improvement Projects. When we decided to cancel the contract with Alpha and instead build the magnets at Argonne, the costs escalated.

It was touch-and-go to keep costs below the legal limits. The RF system was funded from Accelerator Improvement funds the following year, at a cost of less that $400K, so that problem did not exist for the RF system. As Jim Simpson has noted, Tom Hardek did an outstanding job with design and construction of a low impedance cathode follower RF driver. (I had built a cathode follower RF system for the Cornell 1 Gev synchrotron in 1954, but acceleration of electrons is considerably easier than accelerating protons).

There were very few funds for other important elements of the machine, most notably for the vacuum system, diagnostics, and correction magnets. This bare-bones situation exacerbated the difficulties of bringing the machine into good operation. The dedication and innovation of Jim Simpson and the small group of people involved were essential in making both the Cornell project and the Booster II project successful.

Later, both Yanglai Cho and Jim Norem worked on the further tuneup of Booster II. Within a few years, Booster II became the proton source for the extremely successful IPNS, and is still in operation as a workhorse user facility today, 27 years later.

I maintain that the original concept of a 500 MeV rapid cycling synchrotron with H^- charge exchange injection to increase the circulating intensity of the ZGS was valid. Had the ZGS not been turned off in 1979, the project could have been completed and, I believe, with 8 Booster pulse injection at 3e12 could have raised the ZGS intensity to 2.4e13 ppp. *[Editor's note - these intensity estimates of Ron Martin's are strikingly similar to the actual performance which has recently (2005) been achieved by the Fermilab Booster injecting into the Main Injector for the NuMI/MINOS neutrino experiment. One should also note that the normal IPNS/RCS intensity has been about 3e12 ppp for the past 2 decades.]*

The above story of the early development of charge exchange injection at Argonne would not be complete without mention of two other accelerator projects which I have been advocating and designing for many years. Both are conceptually related to charge exchange injection and were stimulated by our Booster ideas. Neither has yet reached the proof-of-principle stage, but, in my opinion, both are still very promising:

> One of these is the appropriate accelerator (the Proton Diagnostic Accelerator -PDA) for medical applications in hospitals or clinics. Such accelerators (i.e. for proton computed tomography, and even for radiation treatment of cancer) require much smaller circulating intensities than those for physics applications. Therefore, injection intensity is not a problem. Rather the important criteria are simplicity, reliability and relatively low cost. These criteria rule against resonant extraction, the normal extraction mechanism for physics synchrotrons, because it is too sophisticated. Utilization of acceleration of H^- ions and charge exchange extraction is much simpler, lower cost and results in a much higher quality extracted beam.
>
> The other spinoff is the extremely important concept of energy production from Heavy Ion Inertial Fusion.- HIF. This application requires the highest available intensities but relatively low energy ion beams. Since H^- charge exchange injection can produce space charge limited intensities in small machines I was motivated to suggest that 100 such small machines with protons might be interesting for inertial fusion. I was quickly informed that my concept (including the upgrade to deuterons) was totally inadequate for the purpose. About the same time Al Maschke (Brookhaven) speculated about the large stored energy in a ring that could be achieved with singly charged heavy ions, and the large dE/dx when such a beam penetrated material (because all electrons are quickly stripped off). Production of such beams was far from clear at the time, however. In 1975, I began looking, along with Rick Arnold of the ANL HEP division, for an injection technique for singly charged heavy ions into a ring comparable to charge exchange injection of H^-. Molecular dissociation of the HI molecule to neutral hydrogen and singly charged iodine was suggested by Joe Berkowitz of Argonne's Physics Division. An accelerator of Iodine ions was proposed for this purpose, attracted considerable interest, and led to international workshops on this subject.

Chapter 3

Improvements of IPNS Accelerator Systems (1978-1982)

Robert L. Kustom ANL, retired

I was Manager of IPNS Accelerator Systems from 1978 to 1981. Then I became Division Director of the Accelerator Research and Facilities (ARF) division. Responsibility for the Rapid Cycling Synchrotron (RCS) resided in the ARF division, and therefore remained my responsibility. As Division Director, I was asked to do what was necessary to bring the accelerator systems on line for IPNS user programs.

The RCS had been commissioned by the time I took responsibility for it in 1978. I took over from Jim Simpson, who had been responsible for early R&D, design, construction, and commissioning.

It's fair to say that Jim and his team did very well to be able to get the accelerator built and into the commissioning stage, since it was never afforded line item status and was chronically understaffed and under funded. Innovative approaches to funding the project and to constructing the hardware were essential. It's not surprising that some of the IPNS hardware systems and supporting machine studies had not reached the stage necessary for a reliable, well-understood user facility. In fact, even a well funded and well-staffed accelerator construction project usually has its infancy problems and requires time and additional effort to achieve reliable user operations.

When I inherited the responsibility for the IPNS accelerator systems in 1978, I went through an assessment period, and came to the following overall conclusions:

(1) Relations between the accelerator staff and instrument scientists and users:

There was a major culture shock and clash between the accelerator staff and the instrument, materials science and condensed matter scientific staff. It was going to be a difficult task to build trust in the user community and to successfully integrate activities in the two communities. The accelerator staff, for the most part, had a high-energy-physics (HEP) accelerator background. In HEP, accelerator development was often integrated into the HEP experiments. The HEP experiments tended to acquire and integrate data over long periods of time, sometimes years, and often were tolerant of frequent short shutdowns. At least that was often true for the first experiments coming on line. Accelerators were usually pushed to the limit of technology and it was understood that it took time to identify the problem areas and correct them before reliable, stable operation was achieved. Also, the peak performance that was expected of the new accelerator was often quoted in terms of the Laslett tune shift. A high level of performance that was usually not reached until after a few years of stable operation and supporting machine studies, and then often reaching only to within 50-75% of the Laslett tune shift level. High-energy physicists were familiar with these challenges; indeed, many of the early accelerator builders were high-energy physicists.

The material science users previously got their neutrons from a nuclear reactor and were used to the nuclear reactor environment. Because of the obvious safety and environmental issues related to operating a nuclear reactor, the engineering design margins were considerably more conservative and extensively tested for performance prior to being used in the reactor. Reactors tend to operate without shutdowns for long periods of time, and then have long refurbishment periods. Many material science and condensed matter users perform experiments with highly unstable crystals or samples. As one biologist related it to me, his bacteria eats itself and within 36 to 48 hours he was left with nothing but excrement. Likewise, some condensed matter scientists performed experiments on deuterated samples that would reabsorb hydrogen and drive the deuterium out of the sample over a period of days. Short, unexpected shutdowns of a day or more often interrupted their data collection stream and forced the data collection to be aborted.

(2) Hardware reliability and machine availability:

The machine availability was poor at the time I took responsibility for it. My recollection is that availability was on the order of 65% or a bit worse. Some of the reasons were consequences of the marginal funding arrangements, so that some hardware was forced to be hand-me-downs or compromised by limited funding. Additional hardware unreliability was due to the kinds of infancy problems that occur when first of a kind equipment that is not commercially available is brought on line. The most difficult hardware problems that had to be overcome were related to the pulsed extraction septum magnet, the kicker magnet system, and the stripping foils.

(3) Accelerator physics issues related to the RCS:

The basic machine parameters and the proof of design were successfully demonstrated by the achievement of acceleration to full energy of the beam, albeit at low beam intensity and low repetition rate. There were, however, some clear issues that had to be addressed before a healthy user program could start. A heavily beam-loaded RCS is beam-loss dominated. Pounding away at 30 hertz, it doesn't take long for radioactivity resulting from beam losses to make machine maintenance extremely difficult, if not impossible. Unacceptably heavy losses were occurring at injection and extraction. In addition, attempts to accelerate more than 6 to 8 e11 particles per cycle resulted in a large increase in beam size and heavy losses in the extraction septum magnet.

RCS Accelerator Physics Issues

Yang Cho and Ed Crosbie were asked to direct their attentions to the basic physics issues of the machine. Yang was given the lead in characterizing the state of machine, identifying problem areas that had to be addressed, and developing an overall physics upgrade plan. Charley Potts, Frank Brumwell, Tony Rauchas, and a significant number of the Operations Group staff and technicians became more involved with the Booster group. They worked on upgrading key hardware elements of the RCS in order to achieve a production facility level of performance.

Yang Cho, with help from Rauchas, made a comprehensive tune map of the RCS as a function of time in the acceleration cycle and radial position in the chamber. He discovered that the chromaticity (change in betatron tune as a function of fractional change in momentum) flipped from negative to positive at around 14 milliseconds. He also identified the beam growth problem as a head-tail instability wherein coherent vertical oscillations occur around the median plane along the length of the bunch. The head-tail instability was previously studied by Sacherer at CERN. His formulation predicts that positive chromaticity will drive the instability in synchrotrons that are operated below the transition energy, the energy at which the time it takes a particle to rotate around the circumference switches between slower to faster with increasing momentum. The RCS operates below transition.

Attempts to make the chromaticity negative throughout the acceleration cycle met with difficulties because the existing sextupole power supplies were driven in a 30-hertz resonant-mode. The necessary corrections at high energy invariably caused serious beam disruption at low energy. It was clear in hindsight that the cause of the problem was the large phase shift in the sextupole content of the field due to the high permeability of the ring magnets. The fields could not be corrected satisfactorily throughout the acceleration cycle by a simple resonantly driven power supply. A design and construction program for a transistor-driven, programmable power-supply was started. The new power supplies were able to correct the chromaticity problem and led to significantly higher stable thresholds of operation.

Yang Cho also discovered that the beam losses on the extraction magnet were unnecessarily high because the extraction magnets were not properly aligned at installation, occluding part of the beam in passage through the extraction channel. This was ultimately corrected and beam losses at extraction were significantly reduced. Trim magnets were also added to help steer the beam into the extraction channel.

Another significant reduction in beam loss was achieved by improvements in the RF capture process after a careful study by Yang. Painting the injected beam across the aperture to reduce space charge detuning of the lattice was planned as part of original design. However, the capture process in the RF bucket doesn't have time for abiabatic bunching because of the rapid field change and the rapid start of acceleration in the RCS. Yang worked on developing early injection, prior to B-minimum, along with tailoring the RF voltage program to achieve as much bunching and subsequent capture as possible. He optimized the process of injecting on the falling magnet field 50 to 150 microseconds before B-minimum with a low RF voltage on the RF cavities. This scheme actually captured around 90% of the injected beam. Ed Crosbie further improved injection efficiency by working on the alignment and tune of the injection line.

The original extraction kicker system was a two stage process with one kicker magnet in the S-3 short straight section of the RCS and the second kicker in the S-4 straight section. The extraction system was simplified by relocating the kicker magnets into a single straight section, S-3. This made more efficient use of the kicker strength because the magnets in the two straight-section kicker configuration were significantly less than 180^{0} apart in betatron phase space.

Implementation of these improvements took about 2 years in total, 1978 to 1980, and in due course allowed stable operation at 30-Hz with an average beam current of about 6 μamperes. The RCS was shut down on August 4, 1980, in order to switch the external proton beam line from the Zing-P' target to the new IPNS target.

Achieving Reliable Operation

Improvements in facility reliability were also a high priority during the buildup to operation of the RCS with the IPNS target. By 1982, the reliability was improved to 85%. See Fig 3. This improvement was critical to establishing a sound user program.

Bob Macek of Los Alamos summarized the view from the experimental floor at an ICANS meeting several years ago: His contention was that a viable user program exists with 85% percent and higher reliability; a poor program exists with 75% to 80% reliability; and no program exists at less than 75%. I totally agree with that view. Both of us being in the accelerator community and not the condensed matter/materials science community, we were on the receiving end of acrimonious suggestions from the user community, with most of the comments not related to technical aspects of facility improvements.

We established a goal to achieve 90%, or better, reliability early in the RCS program. We pursued this goal vigorously and effectively at Argonne, with results as shown in Fig 3. At Los Alamos's LANSCE, progress on reliability has been slower. Hopefully, the latest improvement programs on LAMPF and LANSCE have finally gotten them over the accelerator reliability challenges.

The improvement in RCS accelerator system reliability is the result of the dedication of many individuals. Stripper foil failures and the resultant system downtime was one of the earliest problems to manifest itself. Frank Brumwell forged major breakthroughs in this area by depositing 3000-4000 Angstrom foils with a 400-Angstrom layer coating of aluminum and supporting them in a suspension system with a lightly-weighted bottom. It was eventually possible to operate with millions of injection pulses without foil failure.

Extraction septum magnet failures represented the most severe problem that was encountered in the evolution of the facility. The original design was an 8-turn thin septum magnet that was based on what had been developed for the ZGS. These magnets would have lasted for decades on the ZGS, but because of the 30-Hz pulse rate and very heavy beam losses, they were only lasting 2 to 4 months. And to complicate matters, the residual radiation levels were still a few R/hr even after 2 to 3 weeks of cooldown. Repairs were difficult. The staff was accumulating their allowable dose limits in working on the magnets. Spare production couldn't keep up with failures. Operation was very close to coming to a dead stop. Frank Brumwell and his crew deserve considerable credit for the effort that they expended in keeping the facility struggling along.

Martyn Foss came to the rescue with an ingenious solution (not an uncommon event for him, of course), the transformer septum. He suggested splitting the extraction septum in half. He suggested that the first half magnet be built as a shorted transformer where the septum is a single bar of copper wrapped around a magnet core that is excited by a multiturn primary winding. The multiturn winding provides the impedance match to the pulsed power supply and can be located physically away from the beam. This allows the multiturn windings to be rugged and well insulated without occluding the extracted beam. He suggested that the second half-magnet be a DC-driven septum magnet to complete the extraction. Brumwell sweated and slaved to keep enough multiturn septum magnets in an operating state while we struggled to build and test Martyn's new design. The transformer/DC septum was a fantastic success. None failed during my remaining tenure on IPNS. I believe someone told me that one failed after several billion pulses.

The next major reliability problems in order of impact were failures in the extraction kicker magnet power supplies and the vulnerability of low level electronics to kicker generated noise. The high voltage cables between the kicker magnets and switching tended to fail frequently. Dale Suddeth and Gerry Volk eventually improved the design of the kicker system to increase its availability to 98.5% and also improve the shielding of pulsed fields to an acceptable level.

The amplifiers in use on the RCS were modified versions of the original ones built by Dawson and Moretti. Tom Hardek did a good job of rebuilding parts of the original amplifiers to get them to work reliably.

After operation was switched to the new IPNS target in 1982, reliability was up from 67% to 85%, accelerated beam was increased to 6 μamperes with occasional 24-hour averages reaching 7.6 μamperes, and losses were contained to a level that hands-on maintenance remained possible.

User relations improved considerably, at least in part because they started getting more data than the user teams that were then available could analyze. I'm quite sure that real trust of the RCS accelerator community by the IPNS user community was not achieved in 1982. Later, with the continued success of accelerator operations and steady increases in beam intensity on target, a higher level of trust was achieved.

External Reviews and Events

I haven't yet mentioned the relatively hostile attitude of some members of DOE and OMB. This may have been based upon their desire to reduce the number of accelerator laboratories. In any case, it seemed clear that IPNS was considered to be expendable.

I don't remember the dates, but there were two committees commissioned by DOE, a few years apart, to review neutron science facilities and make recommendations to DOE on scientific direction. These were popularly known as the Brinkman committees, Bill Brinkman being the chairman. The first Brinkman committee had an accelerator sub-panel. I don't remember the exact dates or all the committee members, but I remember that Herman Grunder and Dave Sutter were on the sub-panel. The general conclusion of the sub-panel was that of the two spallation sources, IPNS and PSR (Proton Storage Ring at Los Alamos), PSR was going to easily achieve several times higher current than the RCS and be more reliable. The panel's view was that the RCS would be more difficult to bring on line. IPNS somehow managed to stay alive despite these rather damning opinions.

I remember that when the second Brinkman committee was announced, IPNS was functioning fairly well and, for whatever reasons, PSR and LANSCE weren't. There was still a serious measure of concern on the part of the IPNS staff, since the first Brinkman committee did not support the IPNS facility. Apparently, the findings of the second committee were much more favorable.

In any event, these year-after-year machinations on the part of and behalf of the funding agency didn't make our tasks any easier. The recommendations of the first Brinkman committee certainly made funding more difficult to get and created a difficult environment for the IPNS staff. It's interesting to note that many years elapsed before PSR and LANSCE established a strong user program. In any case, with the powerful new Spallation Neutron Source (SNS) soon coming on line at Oak Ridge in Tennessee, it's about as certain as it can be that older U.S. spallation sources will be totally outdone.

It's also interesting to note that a version of the 800 MeV upgrade of IPNS that was proposed long ago by Argonne, but not approved, was built in the UK. For many years, it (ISIS) has been the premier spallation neutron source in the world. ISIS will also soon feel the pressure of SNS.

[Editor's Note: Speaking of SNS, it should also be mentioned here that two senior RCS accelerator physicists [Cho and Kustom] played key roles at Oak Ridge National Laboratory in the initial design and management of the SNS accelerator system. Their work at Oak Ridge was preceded by their leadership roles in building the Advanced Photon Source accelerator systems at Argonne.]

Chronology of Booster Project

Year	Events	See Page
1969	Booster I (Cornell Ring) project began	3
1970	First test of H^- injection into ZGS	11
1972	H^- injection into Booster I	4
1973	H^- became normal ZGS injection mode	5, 12
1974	500 MeV Booster II construction began	5
1978	Booster II accelerates beam, renamed "RCS"	15
1979	ZGS operations terminated	9, 13
1981	Intense Pulsed Neutron Source began operation	18

References

1. History of the ZGS, American Institute of Physics (1980).

2. Symposium on the 30th Anniversary of the ZGS Startup, ANL Report ANL-HEP-CP-96-12.

3. Argonne IPNS website: www.pns.anl.gov

[Editor's note: The following four figures were kindly supplied by Frank Brumwell of the ANL IPNS division. His contributions to the success of this accelerator project during both construction and the past 25 years of operation have been crucial.]

Fig 1. View inside the Rapid Cycling Synchrotron enclosure. On the right is a triplet main ring magnet. On the left, behind the survey post, is a singlet main ring magnet.

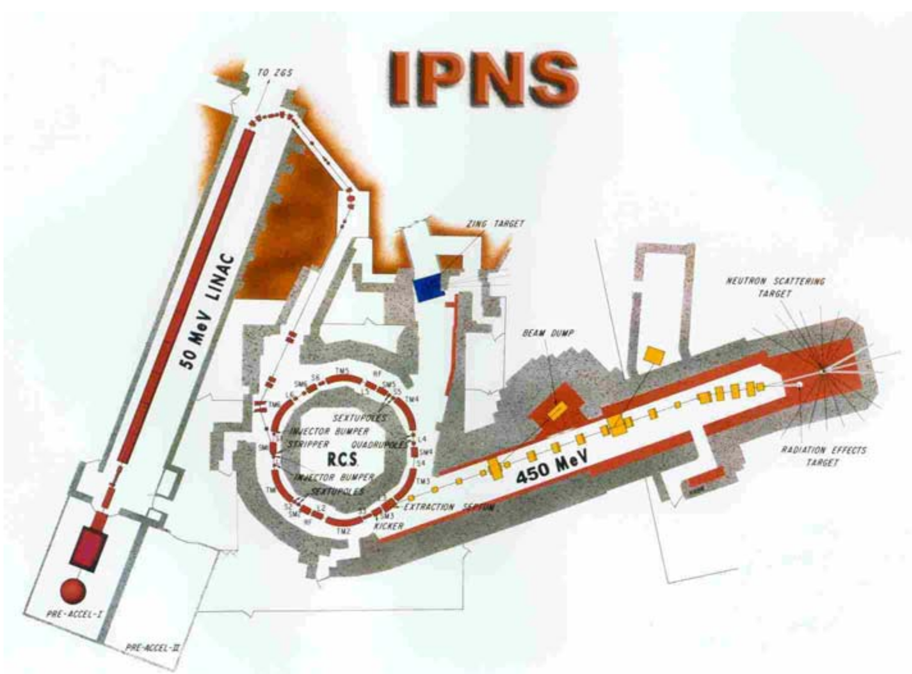

Fig 2. Plan view of the IPNS facility. The RCS accelerates protons from 50 MeV to 450 MeV. The 50 MeV injector linac and the beam line which transports the 450 MeV proton beam to the spallation target can also be seen.

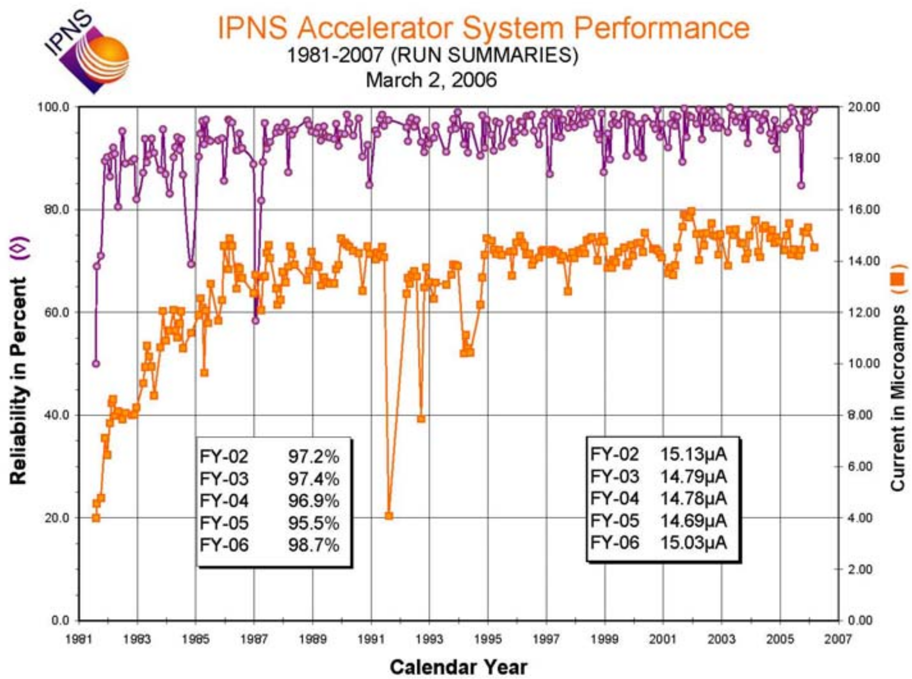

Fig 3. RCS beam current and reliability, 1981-2006.

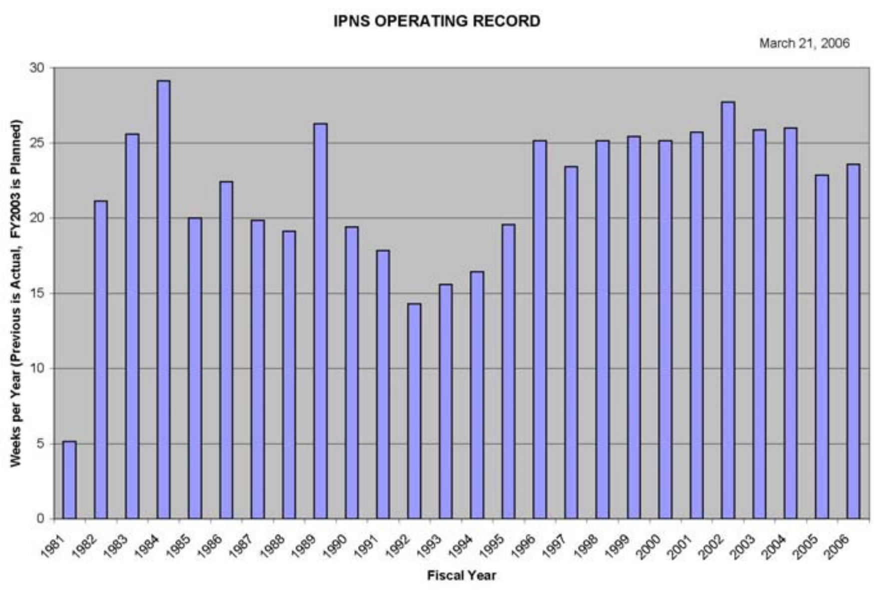

Fig 4. RCS weeks of operation per year, 1981-2006.

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