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SNEAP 80

Proceedings of the Symposium of

NORTHEASTERN ACCELERATOR PERSONNEL

held at the

University of Wisconsin

Madison, Wisconsin

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October 13-15, 1980

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SNEAP 80

SYMPOSIUM

OF

NORTHEASTERN ACCELERATOR PERSONNEL

Held at the University of Wisconsin

Madison, Wisconsin

October 13-15, 1980

Compiled and edited by

James H. Billen

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Acknowledgements

This SNEAP meeting involved the efforts of a large number of people. Greg Norton played a major roll in organizing the meeting and developing the program. He has continued to provide invaluable assistance in preparing these proceedings. It has been a great pleasure working with Greg during the last several months.

Many SNEAP participants contributed to what we hope was a very fruitful and enjoyable conference. I would particularly like to thank Neil Burn for the survey of gas mixtures that he conducted (in addition to his several other contributions). Perhaps Neil will be more careful in the future when responding to the first announcement. I also thank Charlie Adams for his suggestions on organizing the meeting, Charles Jones for his helpful comments on the program format, and John McKay for his assistance with technical and financial details. Those participants who served as session chairmen deserve acknowledgement for their efforts to use the meeting time efficiently and productively. Finally, of course, we thank the many speakers who supplied manuscripts of their talks and those who provided copies of their laboratory reports.

Several people helped in the preparation of these proceedings. I would like to thank Sharon Warner of National Electrostatics Corporation for transcribing the tape recordings and Jeanette Day of the UW nuclear physics group for typing several of the manuscripts. I am also grateful to Steve Riedhauser for proofreading the proceedings.

Ms. Patricia Gaitan of the Wisconsin Center prepared the conference materials and provided registration services. Our conference coordinator, Mr. David Fjeldstad, oversaw all of the physical arrangements including those for the outing and dinner at Wisconsin Dells. I would like to express my gratitude to them and to Mr. Fjeldstad's secretary, Ms. Orpha Smith, for their courteous assistance both before and during the symposium.

We gratefully acknowledge the financial support that we received from Air Products and Chemicals, Inc., General Ionex Corporation, High Voltage Engineering Corporation, and National Electrostatics Corporation. The University of Wisconsin Department of Physics donated the use of its duplicating facilities for producing these proceedings. Additional funds to cover the costs of materials was provided from SNEAP membership dues.

Historical Notes

The Symposium of North-Eastern Accelerator Personnel was founded in 1967 by McMaster University, Université de Montréal, and Chalk River Nuclear Laboratories. The purpose was to provide a forum for the discussion of practical, day-to-day problems and their solutions that are never presented in formal papers at the larger meetings.

The name was chosen on the basis that some of the nearby U.S. labs might wish to join our meetings. The term "Symposium" was suggested by Neil Burn because of its classical Greek definition.[1]

The group has now outgrown its original geographic boundaries to include members from every continent except Asia. The formal organization consists of one filing cabinet at McMaster, a mailing list, and a bank account. The annual meeting is handled entirely by the sponsoring institution. The table below lists the sponsoring institutions and the number of participants for the first fourteen Symposia.

Through the years the style of the meeting has changed from time to time, but the emphasis has always been on informality and spontaneous discussion. The production of verbatim proceedings started with the second meeting. It is a difficult task, but the resulting volumes are extremely valuable to all members, especially those who were unable to attend the annual meeting. Hopefully, SNEAP will continue to be a small, but useful organization of people responsible for the operation of electrostatic accelerators. --- John W. McKay

[1] "Ancient Greek after-dinner party with music, dancers, or conversation..." Oxford Concise Dictionary (fourth edition, 1951).

SNEAP 1967-80

Date	Sponsoring Institution	Number of Participants
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1968	McMaster University	26
1969	Chalk River Nuclear Laboratories	27
1970	Université de Montréal	29
1971	McMaster University	34
1972	Florida State University	42
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*** Manuscripts for these papers still had not been received two months past the deadline date of 1 December 1980. If the paper arrived before the proceedings were prepared for binding it will appear at the end of the volume.

Biographical Note

Professor David Huber, Chairman of the Department of Physics, presented a short welcoming address and introduced the first speaker of the symposium. Raymond G. Herb is president of National Electrostatics Corporation and Charles Mendenhall Professor of Physics, Emeritus, at the University of Wisconsin. In 1933 as a second-year graduate student at Wisconsin, Herb began the development work that led to the first electrostatic accelerator insulated by high-pressure gas. In 1935, with D.B. Parkinson and D.W. Kerst, he developed the first column enclosed by closely spaced metal rings graded in potential. This second accelerator operated at 2.6 MV and after Herb and C.M. Turner remodeled the machine it achieved 4.5 MV with a beam of protons.

During 1940-1945 Herb worked on microwave radar at the Massachusetts Institute of Technology Radiation Laboratory. In 1946 he returned to Wisconsin with one of two Wisconsin accelerators that had been taken to Los Alamos and re-established the research program in nuclear physics. A broad development program was undertaken which utilized undergraduate and beginning graduate students. This work led to the first getter-ion vacuum pump and to the first beams of H^- and He^- making the tandem accelerator practical. Herb and his students developed metal-to-ceramic bonding techniques and the pellet-chain charging system.

In 1965 Herb with J.A. Ferry and T. Pauly founded National Electrostatics Corporation. He was named a member of the National Academy of Sciences (1955) and has received honorary degrees from the University of Basel and the University of São Paulo. In 1968 he received the Tom W. Bonner Award.

ELECTROSTATIC ACCELERATOR DEVELOPMENT IN THE 1930's

Raymond G. Herb

The Early 1930's

Developments in physics and especially in nuclear physics and accelerator development moved forward very rapidly in the USA during the 1930's. Most of the developments I will discuss came in this period.

In 1930, there was relatively little physics in this country and there was no nuclear physics. Students in physics were expected to go to Europe for at least part of their graduate work to be adequately trained. There was important nuclear physics in Canada and very active programs in France, Germany and England.

My interest in nuclear physics dates from 1929 when Fowler, a theorist at Cambridge, gave a talk in Madison on nuclear physics at the Cavendish Laboratory. I was then a physics major with junior standing. He reported extensive and, I thought, spectacular experimental results. I was sold. In 1930 physics in this country was changing rapidly and by 1933 many visitors from Europe came to learn rather than, as previously, only to teach. This complete turnabout was due partially to political difficulties in Europe. Many gifted physicists in Europe took positions in this country. Another factor of great importance was, I believe, the successful demonstration of a high voltage accelerator for nuclear transmutations. Americans liked machines and the discovery of Cockroft and Walton set in motion a great surge of accelerator development and building that carried this country forward to prominence in nuclear physics and served to stimulate other areas of physics.

The Cyclotron of E.O. Lawrence and the belt charged sphere of Robert Van de Graaff shared popularity and importance with the Cockroft Walton machines in our laboratories of physics. My account of further developments will be concerned only with the electrostatic accelerators that evolved from the Van de Graaff innovation. Van de Graaff first published in 1931. Figure 1 is a diagram he included in his patent application. In the fall of 1931 he took a position at MIT. Karl Compton, then President of MIT was very interested in Van de Graaff's machine.

The MIT Program

At MIT, Robert Van de Graaff with the enthusiastic support of Karl Compton built the machine of Fig. 2. The spheres were 4.57 meters in diameter (15 feet) and the machine was housed in a former dirigible hanger at Round Hill, Connecticut. Measurements of high voltage were then commonly achieved by observation of sparking between spheres utilizing the accepted gradiant value of 30 kV/cm for the dielectic strength of air. A smooth isolated sphere of 4.57 meters diameter was expected to have a sparking potential of 13.7 MV. With a positive ion source in one sphere at a positive potential and a target in the second, which was to be negative, ion energies up to 25 MeV were expected. Figure 3 shows easily

achieved sparking potentials with sparks from smooth surfaces of the sphere. Such sparking indicated achievement of expected voltage. An accelerating tube connecting the two spheres gave trouble at relatively low voltage. Accurate measurements of sparking potentials gave about 3 MV, less than one fourth of the expected value for the maximum voltage on each sphere. The Round Hill installation was abandoned. The sphere and their supports were moved to the MIT campus and were arranged to provide a single positive terminal which operated stably up to 2.7 MV and was used for many years for nuclear studies. This machine is now on display at the Museum of Science in Boston.

The DTM Program

The belt charged spheres that Van de Graaff had built at Princeton were taken on loan to the Department of Terrestrial Magnetism of the Carnegie Institution of Washington. Starting in 1926 a group at DTM under the direction of Gregory Breit had worked on a charged particle accelerator utilizing a Tesla coil and a subdivided accelerating tube in high pressure oil. Gregory Breit moved to New York University leaving Merle Tuve as director of the group. Starting with Van de Graaff's belt charged spheres Merle Tuve, Lawrence Hafstad and Odd Dahl launched a program that was to lead to the first successful nuclear transmutation experiments utilizing electrostatic accelerators.

Figure 4 shows their first successful machine. The multisection accelerating tube which cannot be seen in this photograph extended from an ion source in the terminal to a pumping system and target chamber toward the far wall of the picture. In November and December of 1932 Tuve, Hafstad and Dahl observed transmutations from a number of light elements bombarded by protons and by deuterons. Figure 5 shows their next machine with a terminal consisting of two hemispheres of 1-meter radius joined by a cylindrical section. It was put into operation in 1933 and was used over a period of three decades for a wide variety of experiments with ions of energies up to 1.2 MeV. This machine is now on exhibit at the Museum of Science & Industry of the Smithsonian Institution, Washington, D.C.

The Wisconsin Program

Soon after Van de Graaff's 1931 publication Glen Havens, a postdoctorate fellow at the University of Wisconsin was given support for a vacuum-insulated belt-charged machine and some months later, as a first year graduate student, I joined him in this work. Vacuum discharges between the spherical terminal and the tank wall of which there was no understanding limited potential to 300 kV. Glen Havens left and after much discussion with members of the physics staff I decided to convert to high pressure for insulation. Barton, Mueller and Van Atta had previously obtained high voltage utilizing high pressure air but had not attempted to build an accelerator. Two beginning graduate students, David Parkinson and Donald Kerst joined me in building the accelerator of Figure 6 with an outlay for capital equipment of about $150.00. The flat end plates of the tank were not designed for high pressure. I held a meter stick across one end plate when we first pressurized and, when it had bowed out 5 mm, I said "That's enough". We thereby adopted a working pressure of 3.3 atmospheres (50 psig).

A half million volts came quickly. One day working alone on a self-induced charging system which eliminated the belt charging power supply, I thought the belt needed cleaning. I selected a bottle of CCl_4 for the cleaning and was surprised by greatly improved performance which reverted to normal after a few minutes. Another cleaning again gave spectacular improvement for several minutes. Pouring a few cc's of CCl_4 in the tank gave the same spectacular performance. With prospects looking intriguing, I next poured acetone into the tank. With the first spark it caught on fire. Of easily available additions tried, CCl_4 proved to be best and with CCl_4 in air we reached one million volts in 1933.

Then came months of tests with a great number of accelerating tube configurations. We were forced to subdivide and for potential distribution we were forced to use corona gaps because of the compact geometry. Our best tube, about 42 cm long, using 7-cm lengths of pyrex tubing held 400 kV dependably. This was the first tube to operate in high pressure gas.

With a few more months of development we had a sufficiently compact ion source with low power requirements and its power supply. Donald Kerst wound the transformers for this supply. First observations of α particles from lithium bombarded by protons were made in 1934 utilizing scintillations from a zinc sulfide screen. Then, with point counters we developed and without the benefit of a scaler we obtained a yield curve up to 400 keV which I used for my Ph.D. thesis.

With papers written, thesis accepted and final examination passed I left for the East with Gregory Breit in the spring of 1935 and spent the summer with Tuve, Hafstad and Dahl at DTM where I built a 1-MV voltmeter using one thousand 1-kV resistors.

The 2.5-MV Accelerator

Back in Madison in the fall of 1935 on a postdoctoral appointment, I worked with Dave Parkinson and Donald Kerst on plans for a larger accelerator. This machine, shown in Figure 7, was the first to utilize a column for field control, with insulating supports, charging belt and accelerating tube in a uniform field. This machine went easily to 2.4 MV with very little tube conditioning and gave dependable data up to a maximum proton energy of 2.6 MeV. Data utilizing proton beams from this machine were used by Dave Parkinson and Donald Kerst for their Ph.D. theses. Groups from many laboratories came for visits and then built larger machines, with expectations of going to 10 MV or beyond. Figure 8 shows the pear shaped tank built by Westinghouse at their East Pittsburgh Laboratory. Results with the large accelerators were disappointing.

The 4.5-MV Accelerator

At Wisconsin we made plans for a very large accelerator but decided in 1938 to undertake instead a complete revision of our machine. Figure 9 shows the accelerator we completed in 1940 utilizing the tank of the machine of Figure 7. It went quickly to 4.5 MV in the fall of 1940. J.L. McKibben and C.M. Turner played major roles in the development work, and in the design and in construction of this machine. At that time the range of protons in air gave the most dependable energy measurements. With the beam emerging through an aluminum foil I held a meter stick up to the beam to determine the 4.5-MeV energy. In 1940 safety standards were not strict.

Later

In December of 1940 E.O. Lawrence visited me in Madison, and invited me to join the newly formed Radiation Laboratory at MIT for work on what was to become radar. War appeared imminent. I was number forty when I arrived at the Radiation Laboratory late in December. Almost all of the forty were nuclear physicists. Four years later the laboratory had expanded to over 2000.

My students remained at Wisconsin. Later they moved to the newly organized laboratory at Los Alamos with the accelerator of Figure 9 and with a second machine built by McKibben for development studies.

In the fall of 1945 as the Radiation Laboratory was disbanding there was much uncertainty about the future. There was widespread feeling that almost all of nuclear physics was understood. Julian Schwinger gave a talk in which he pointed out that this feeling was not entirely correct since the quadrupole moment of the deuteron was not fully understood.

In 1947 HVEC was founded for construction of machines for sterilization and for polymerization. Surprisingly, they received an order for a 5-MV research machine for Oak Ridge. Apparently some people thought there was some nuclear physics still to be done. The machine was built. It worked. In a discussion at HVEC I mentioned to Denis Robinson that I thought the country could use a second research machine. Denis quoted me. To the surprise of all physicists conditioned to the 1930's a second machine was built. It went to the Rice Institute.

In the summer of 1948 I was eating lunch on the Wisconsin Union terrace with Alvin Wienberg and Eugene Wigner. Alvin Weinberg commented on the surprising growth of the Physical Review which was more than 1-cm thick and came out every two weeks. Eugene replied that this was a strictly temporary expansion due to events of the war and like a delta function, it would soon revert to its previous lean form.

Viewed from the 80's activity in nuclear physics and in all of science may appear to be a bit slow. There is concern about funding lag and innovation lag. Viewed from the 1930's the present level of scientific activity is a dazzling spectacle. These levels of funding and research activity are being accepted as normal in many countries so that there is now the stimulus of competition. The pace of scientific and technical advance has been gathering momentum for twenty years. The next twenty years will be fabulous. I expect to be in the thick of it for the full twenty years.

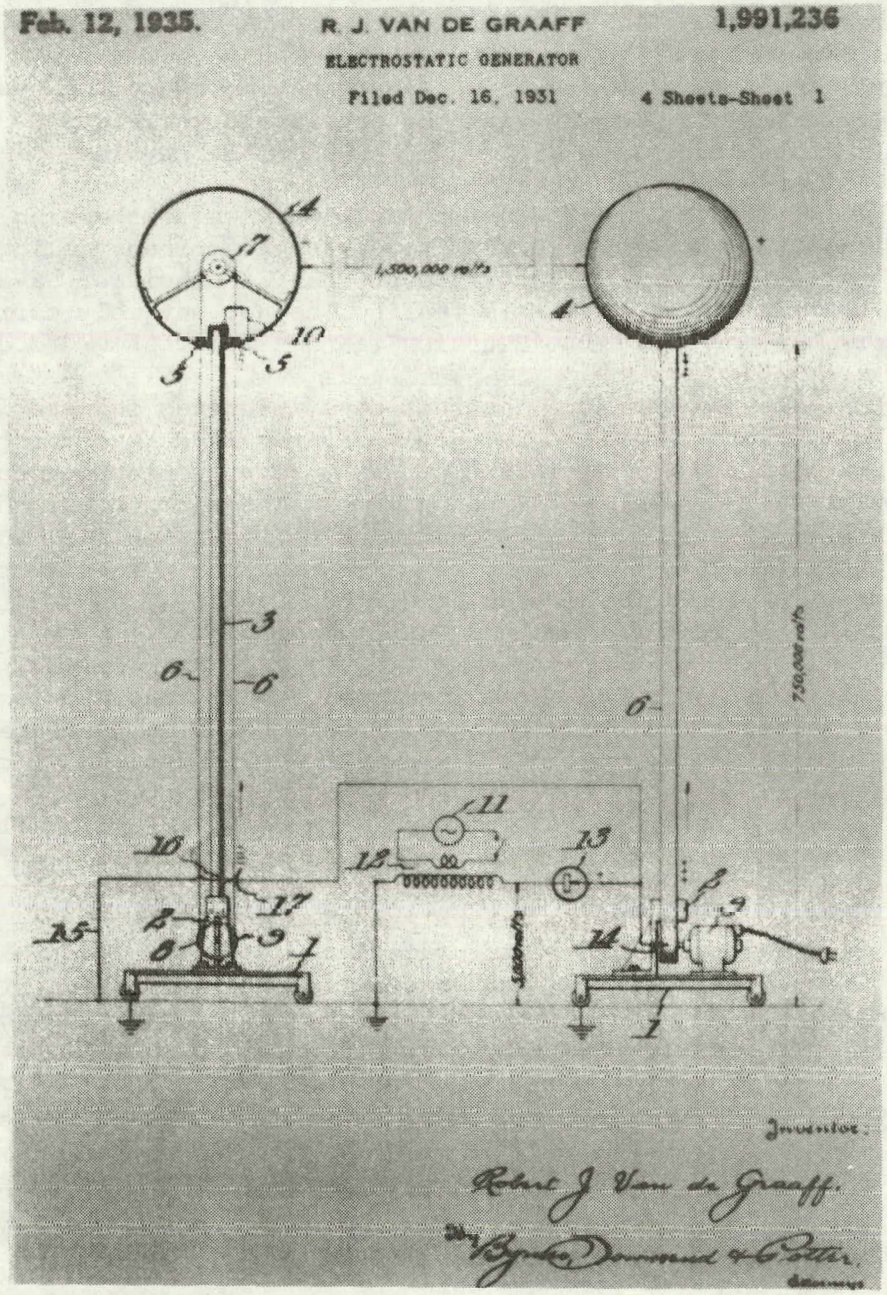


Fig. 1. Line drawing from Van de Graaff's original patent application.

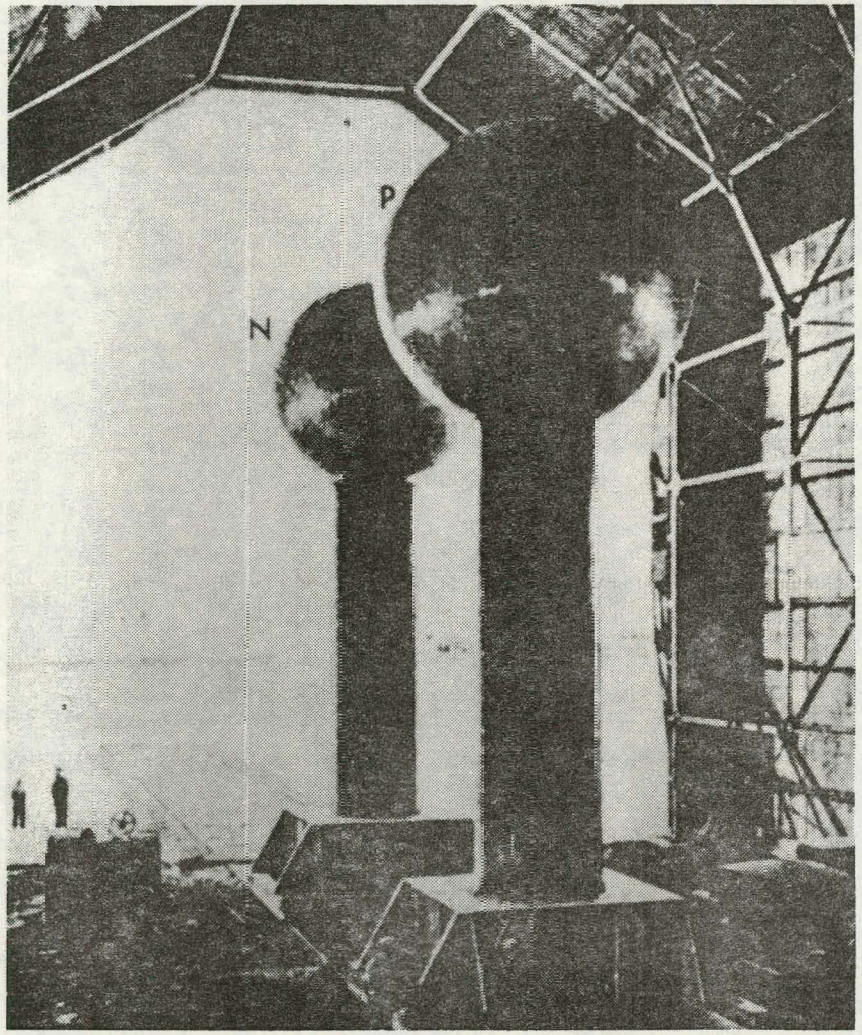


Fig. 2. Double Van de Graaff constructed at Round Hill, Connecticut.

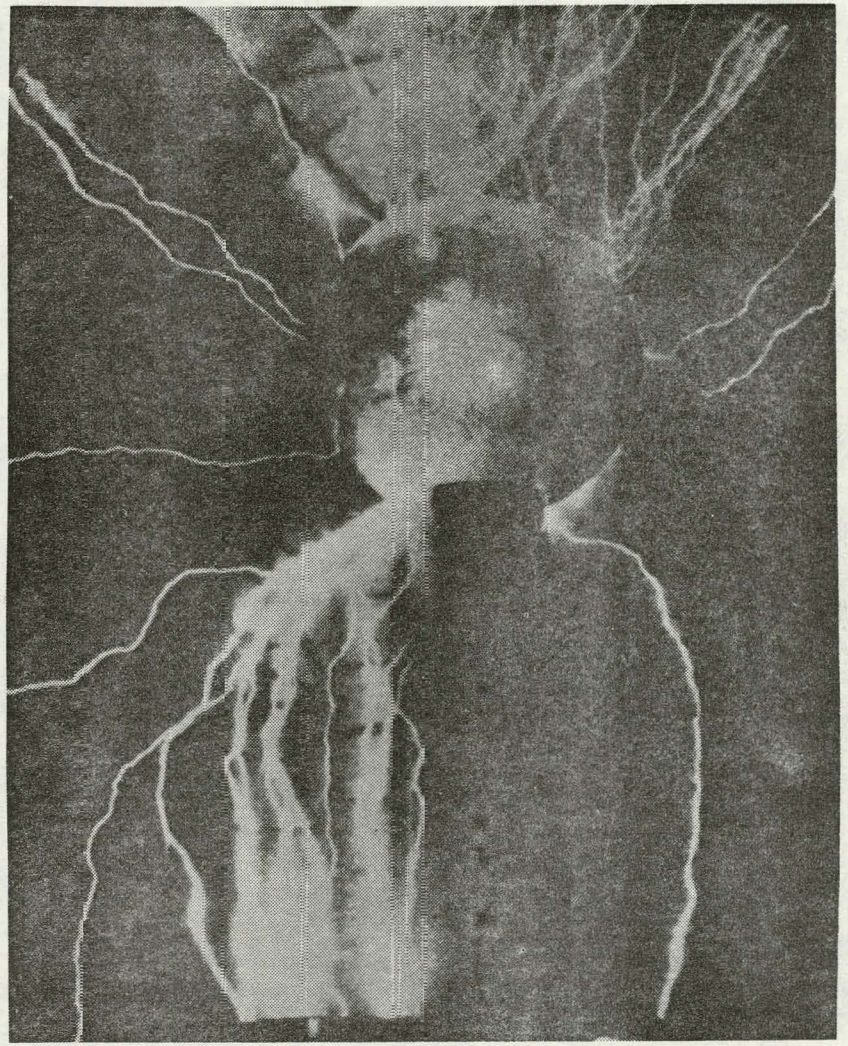


Fig. 3. Sparking at Round Hill installation.

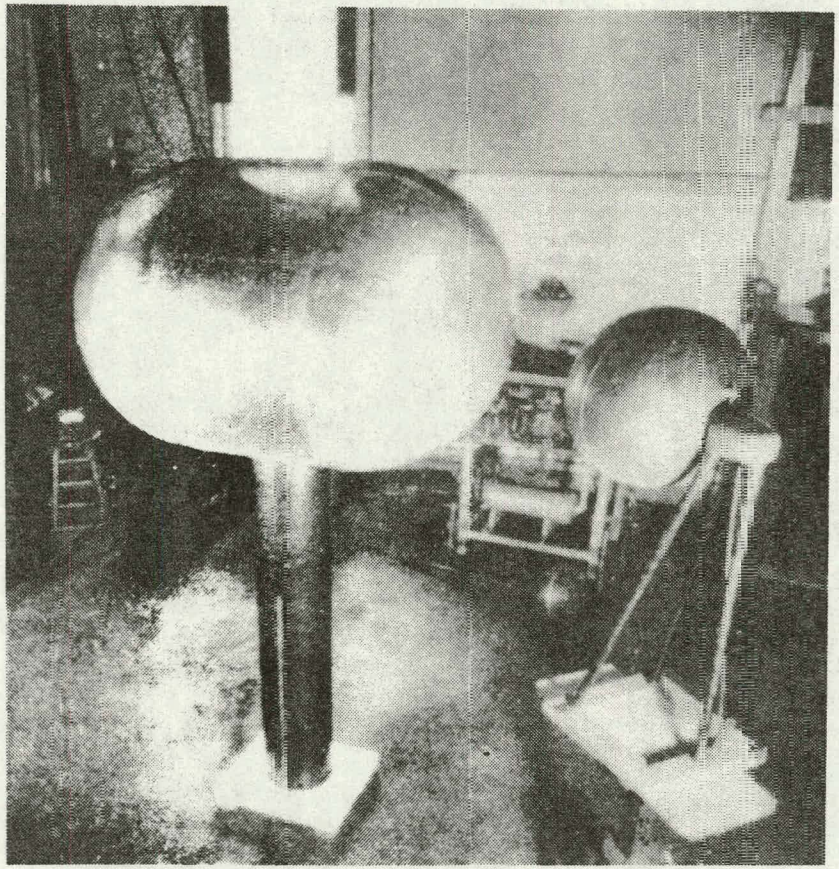


Fig. 4. (1932) First electrostatic accelerator used in nuclear physics research.

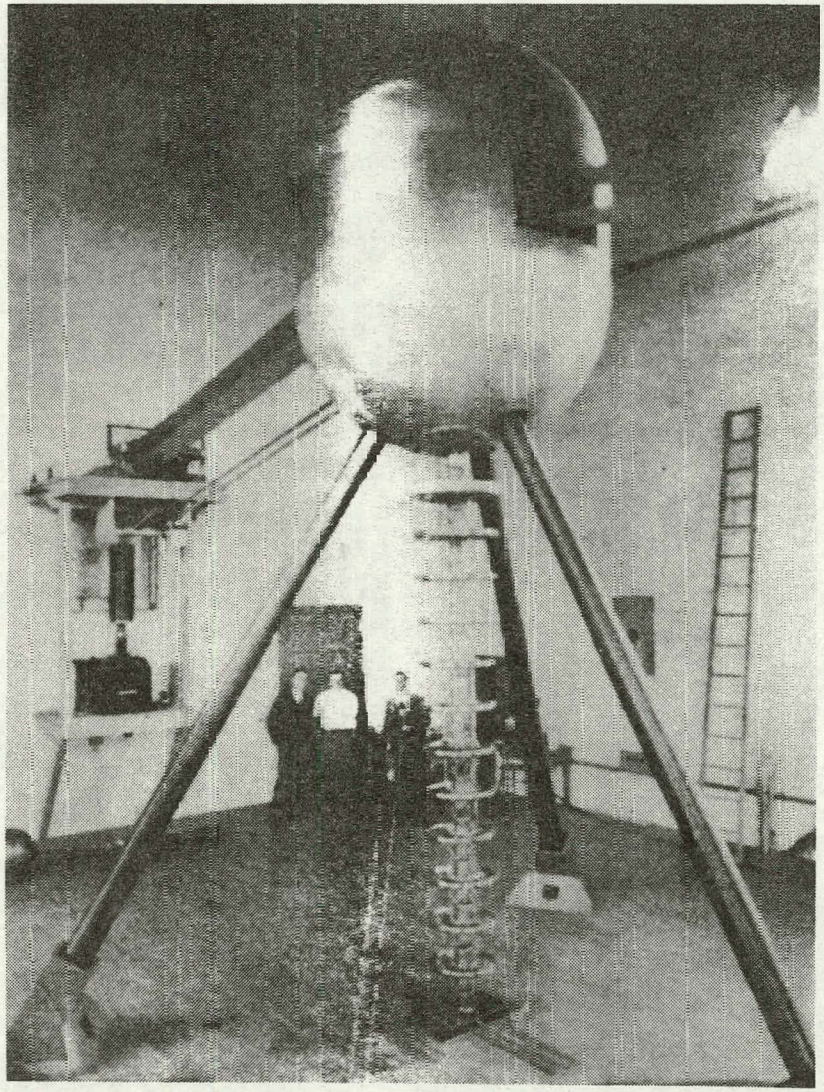


Fig. 5. (1933) Two-meter DTM accelerator accelerated ions to 1.2 MeV. In background from left are M.A. Tuve, L.R. Hoafstad, and O. Dahl.

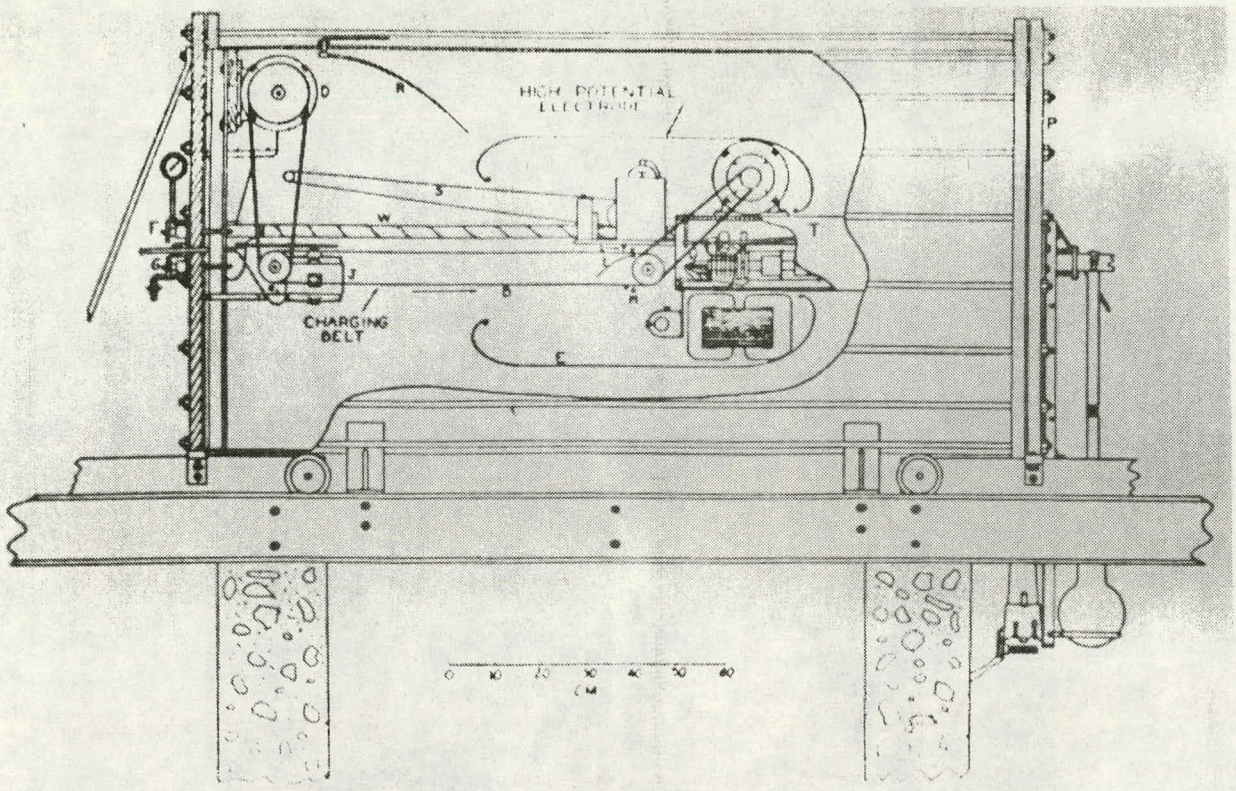


Fig. 6. (1933, Wisconsin) The first pressurized electrostatic accelerator which reached a maximum of 1 MV.

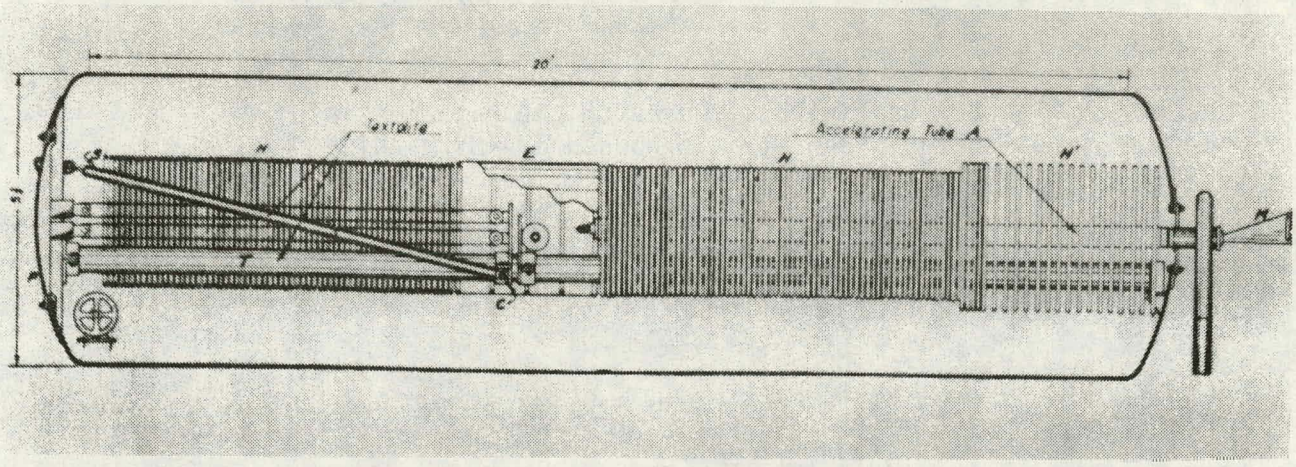


Fig. 7. (1935, Wisconsin) Drawing of the machine that reached a dependable proton energy of 2.6 MeV.

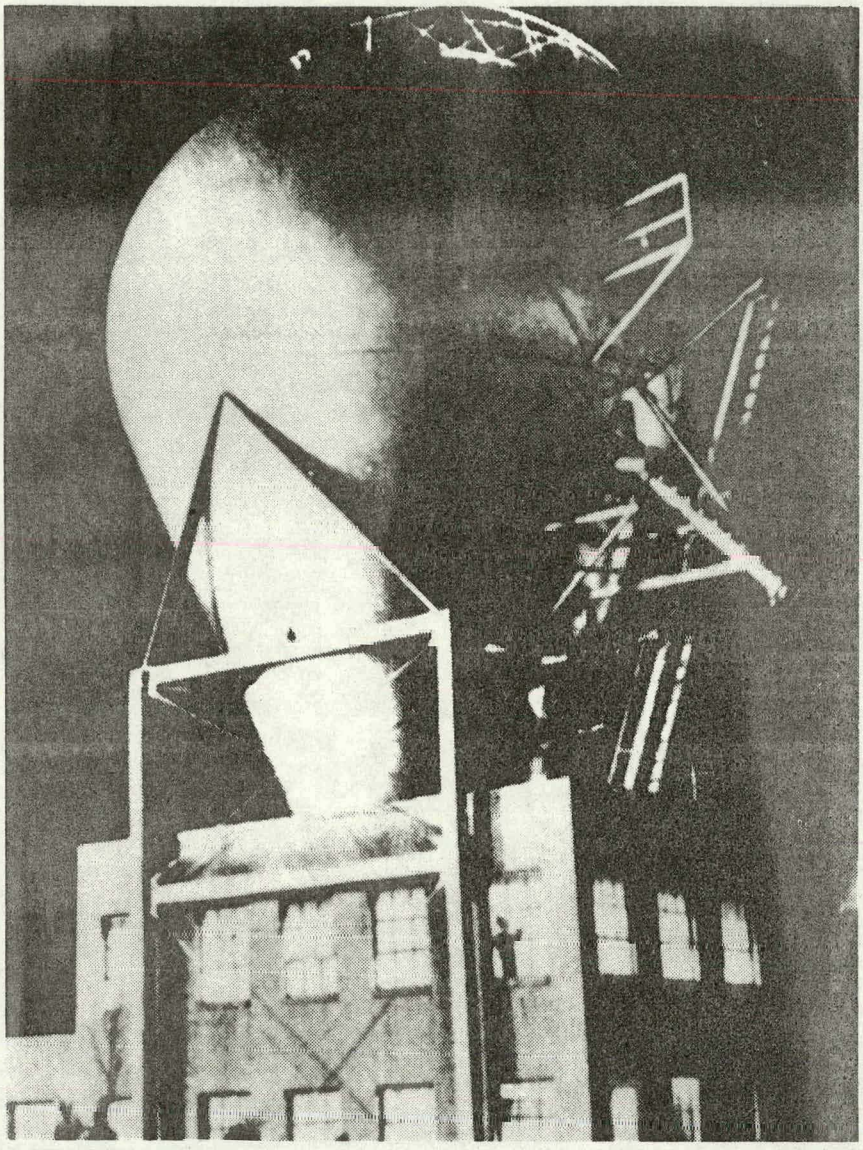


Fig. 8. Westinghouse Research Laboratories in Pittsburgh, Pennsylvania housed a 4-MV machine.

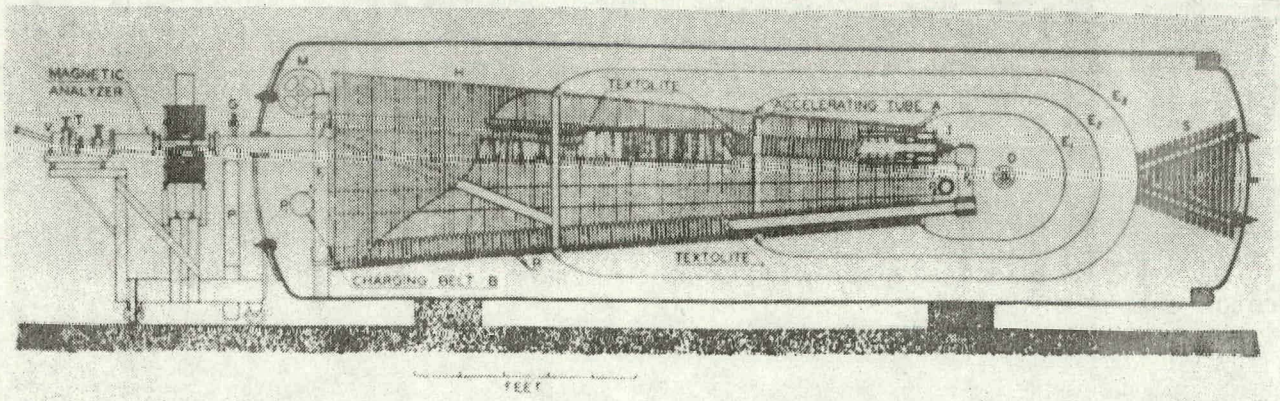


Fig. 9. (1940, Wisconsin) The 4.5-MV electrostatic accelerator.

ARGONNE NATIONAL LABORATORY

1979-80 TANDEM ACCELERATOR REPORT

J. L. YNTEMA, P. K. DENHARTOG, P. BILLQUIST, F. MUNSON

The Argonne FN tandem operated during the past year for 20 weeks as an injector to the Superconducting linac booster and as a stand alone tandem the remainder of the time. It is expected that during the coming year the tandem will function as an injector for the booster for about 26 weeks. A complete report on the status of the tandem-linac system will be given in a later session.

The accelerator operated well during the year with no major problems. An increasing instability evident in the most demanding of experiments accompanied by decreasing transmission led to the removal and rebuilding of the dual enclosed corona tube systems. Both the column and accelerator tube corona tube systems had logged in excess of 15,000 hours of operation at the time of their removal. The relative needle wear on the two systems indicates that the tube corona should probably be replaced at a more frequent interval, possibly 10,000 hours, while the column system can probably function well in excess of 15,000 hours. During the past year the accelerator tubes have not been vented and have not required baking.

A microcomputer has been installed in the terminal of the FN tandem and has functioned well. The computer is presently used to index foil positions and more applications are planned for the future. Communication between the terminal computer and ground is through a fiber optic link. In addition, a separate link is used to continuously monitor a 25 ℓ/s ion pump.

We continue to utilize the Florida State inverted sputter source of Dr. Chapman almost exclusively. The negative ion extraction efficiency has been doubled by a redesign of the extraction geometry. The potential is now split between the original Pierce lens and an additional gap lens. The sputter target-ionizer distance was also shortened. CaH_3^- output from this source has been increased by hydrogenating the Ca cone and work is continuing on the development of beams using very small pellets of separated isotopes.

Two other ion sources are being prepared for use with the FN. These are an NEC Aarhus type cesium sputter source and a University of Wisconsin SNICS source. Also, a new He injector is being designed which will use the General Ionex Model 711 charge exchange canal. A new test stand is being built to aid us in these ion source development activities.

Noé: What is the impetus for operating the charge exchange source?

Den Hartog: The He beam is wanted for some experiments on the linac.

Saylor: What is the transmission of α particles?

Den Hartog: At Argonne its about 30%. What do other people get with their machines?

Berners: At Notre Dame we usually get 50% or 60%.

Rowton: We also get 50-60% at LASL.

McKay: What is the charging current on the Pelletron?

Den Hartog: About 45 μA per chain. We are limited by feedthroughs to inductor voltages of 30 kV.

Brookhaven National Laboratory

Robert Lindgren

Brookhaven tandem facility is still operating 3 shifts per day but in the past year we have had to shut down operations for one weekend out of every three for economic considerations. As the first table shows, we were on for 78.6% of the year. This is the first year we've dropped below 80%. The previous year we were at 84.6% as shown in table 2. The graph of terminal voltage of MP7 vs. operating time shows we were running about 19% of the time between 13.25 and 13.75 MV with about 1% at 14 MV. I find that after a tank opening, on an average, we are not able to operate at 14 MV for a period of about 3 weeks.

We have been having chain breaking problems - 3 in 14 months - that I will discuss later in the session on charging systems.

The MP6-MP7 upgrading is continuing, these following subjects will be discussed at the last session on Wednesday.

MP6 Modifications

Pelletron installation
Power drive shaft
HVEC 14" SS Accel tubes
Dead section ion pump
Improved resistor protection
5-6 dead section foil stripper
Cryopump replacement for diffusion pumps

Ongoing Improvements

Solid terminal shields for both machines
Power drive shafts and dead section pumps for MP7
Final resistor installation MP7
Turbo and cryo pumps for Neg ion injectors
Improved extraction supplies Neg ion injectors

Developments and Future Plans

Insulating gas optimization
Redesign of Pelletron drive sheaves
Accelerator tube extension
Acceleration tube conditioning system - as per Rochester
4 stage acceleration and deceleration system

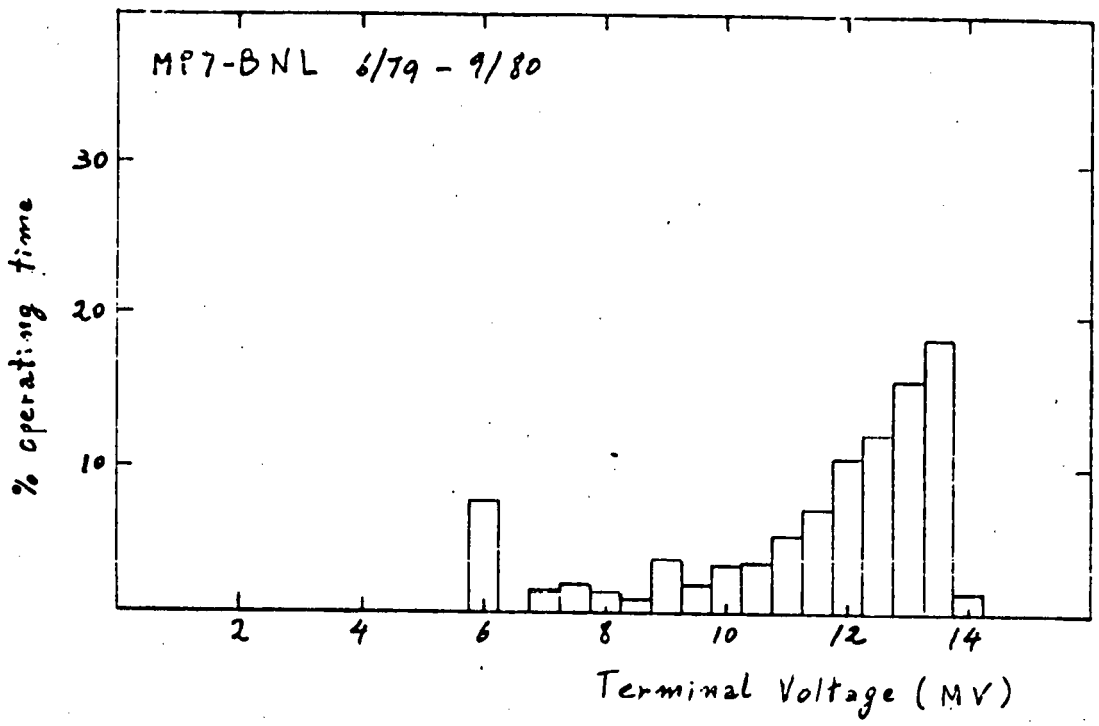
MP 7 Utilization

June 1, 1979 --- May 31, 1980

3 stage operation	20.1%	78.6%
2 stage operation	58.5%	
Scheduled maintenance	3.2%	
Unscheduled maintenance	3.3%	
Upgrading	2.6%	
Shutdown	12.3%	

May 1978 - May 1979

	Shifts	%
2 stage operation	661	60.4
3 stage operation	265	24.2
Scheduled Maintenance	67	6.1
Development + Conditioning	23	2.1
Stand-by + Holidays	38	3.5
Unscheduled maintenance + power failures + snow days	41	3.7



Weitkamp: For your negative operation at what voltage is the negative terminal?

Lindgren: This would be for three stage operation. In that case the negative terminal is always operated between 6.5 and 7 MV.

McKay: In your tables of utilization time how do you define available time?

Lindgren: These are percentages of the total number of hours in a year. Is there some other way to define it?

McKay: In fact there seems to be quite a range of definitions used these days. Some only include usual daytime working hours, for example. That is why I asked.

Lund: When you say that you cannot operate at maximum voltage for three of four weeks after a tank opening does that include openings in which the beam tube was not opened?

Lindgren: No, we can go up faster in that case.

THE CHALK RIVER MP TANDEM ACCELERATOR FACILITY

by

N. Burn

Apart from two incidents described below, the Chalk River MP Tandem Accelerator operated reliably and uneventfully during the past year. By April 1, 1980, the high gradient tubes installed in 1972 had accumulated 52,000 h of operation and the Pelletron chains installed in 1974 had accumulated 38,000 h. During the year, the accelerator was conditioned as high as 13.8 MV with the highest operation for experiments at 13.2 MV.

One of the major advances during the past year has been the reliable and routine production of cracked ethylene slackened stripper foils[1)2)] which have useful lifetimes 30 times longer than those previously in use. Initial tests were made in the terminal of the MP accelerator at 4.9 MV with a one microampere injected beam of ^{127}I. Comparative lifetimes are shown in Figure 1.

Pelletron Chain Break

On February 7, 1980, one of the three chains in the Low Energy end of the accelerator broke during routine operation;[3)] the terminal voltage was 4 MV at the time. The only previous chain break occurred during the initial installation of the Pelletron in 1974; it was caused by an idler coming loose and jamming in the chain, resulting in a break at one point only. This time, unlike the earlier break, the chain shattered into about 30 pieces varying in length from one or two pellets up to about 30. No damage was caused to any other components in the accelerator other than one or two minor dents on the outside of the accelerator tube electrodes in section No. 1. Subsequent tensile tests showed that the nylon links in the failed chain had all become brittle. However, these test failures occurred at loads in excess of 700 kg, whereas normal operating chain tension is less than 30 kg.

One disturbing aspect of the latest failure is that no undue load was being placed on the chain and the break occurred in a nylon link; all of the metal connecting pins were still in place. It has not been determined whether the embrittlement and subsequent failure of the nylon was caused by fatigue due to vibration, insulating gas decomposition products, poor pellet articulation due to mechanical misalignment or some other cause. In any case, the decision has been made to replace all six chains with new ones.

Column Member Break

When the accelerator tank was opened for routine maintenance on June 29, 1980, a small quantity of broken glass was found under the Low Energy column. Further investigation revealed that one of the upper horizontal column members in section No. 1 had broken, the heavier piece having dropped about 5 mm in relation to the other piece. The broken ends of the glass were still in

contact at the top but had separated by about 3 mm at the bottom. The two upper column members at the Low and High Energy ends of the accelerator are not in compression; they are simply supported at both ends and serve only to hold gradient rods, hoops, etc.

The two pieces of the broken member, weighing a total of 300 kg, were removed from the accelerator and shipped back to High Voltage Engineering Corporation (HVEC) in Burlington, Massachusetts, USA. The member was repaired, baked and subjected to the standard tensile pull test of 9000 kg for 30 minutes. It was returned to Chalk River, re-installed and the accelerator was operating again on July 16, 1980. Thanks to the skilled tradesmen involved, the cooperaration of HVEC and the immediate assistance from top levels of administration at Chalk River, the whole operation was expedited smoothly and efficiently in only 18 days. HVEC have been unable to suggest any cause for the break.

Further study shows that had the broken column member been able to move longitudinally, it could have swung down and would most likely have broken the diagonal member immediately below it, causing a complete and catastrophic collapse of the whole column structure. The ends of these upper column members are, in fact, free to move longitudinally towards the base of the accelerator. An elongated hole permits a two to three cm movement of the free end to compensate for changes in accelerator tank length during pressurizing; however, these changes are probably less than one mm. The broken member was only restrained from moving by a cross-piece tying it to the base end of the corresponding upper member on the other side of the column. The sudden stress caused by the break might have caused a similar fracture in that member. Accordingly, it has been decided to install shims behind the free ends of the upper column members at both ends of the accelerator. These shims will take up most of the existing space but leave sufficient for travel during pressurizing. The extent of this travel will be determined by actual measurement. These shims will then prevent excessive longitudinal movement in the event of any future failures.

Superconducting Cyclotron Status

A heavy ion superconducting cyclotron[4] is planned as a post accelerator for the MP Tandem (Figure 2). The main magnet has been built and operated in the superconducting mode; field mapping is complete. The RF system will be installed in the magnet late in 1980. Fifty percent of the concrete for the new building has been poured and orders have been placed for the Phase 1 beam transport system. It is expected that the cyclotron will be moved into the building early in 1982.

References

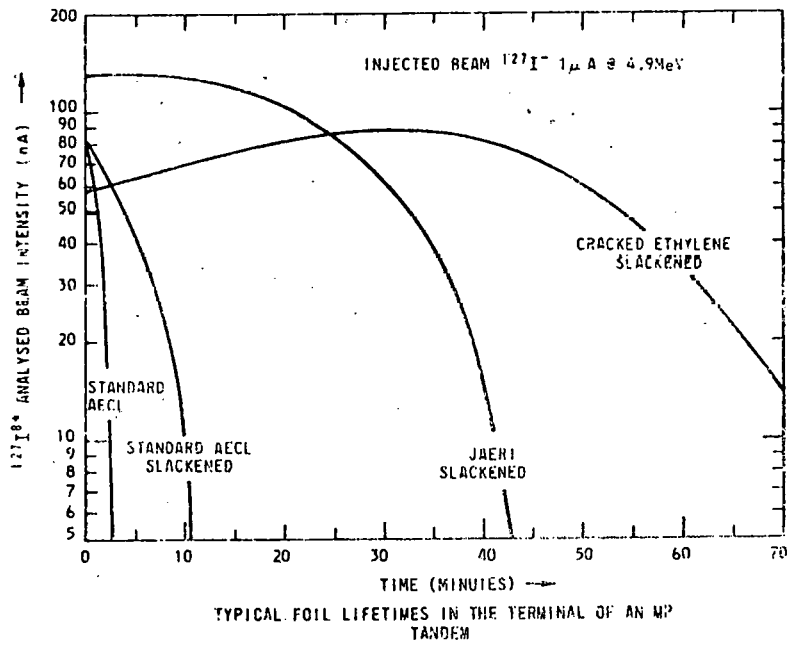
1) *Experience with Foil Strippers in the Chalk River MP Tandem Accelerator.* J.L. Gallant, D. Yaraskavitch, N. Burn, A.B. McDonald, H.R. Andrews, World Conference of the International Nuclear Target Development Society sponsored by New England Nuclear, Boston, 1979 October 1-3.

2) *A Simple Ethylene Cracking Apparatus for the Preparation of Carbon Films.* J.L. Gallant and P. Dmytrenko,

3) *Analysis of the Pelletron Charging Chain Break in the Chalk River MP Tandem Accelerator.* N. Burn, B.F. Greiner and C.E. Coleman, AECL-7099, 1980 September.

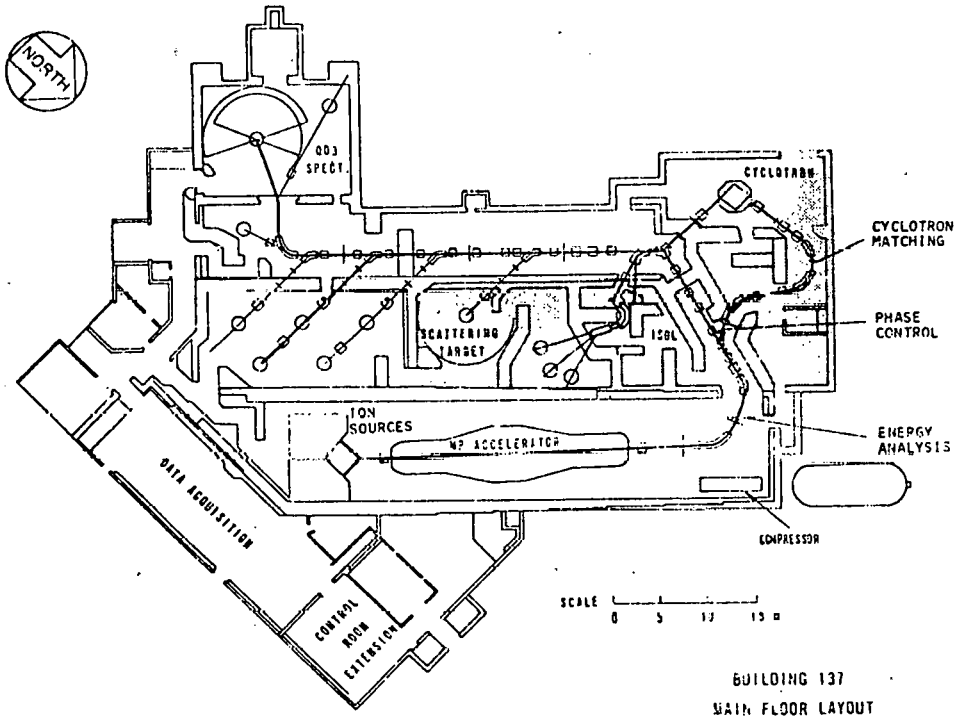
4) *A Superconducting Heavy-Ion Cyclotron as a Post Accelerator for the MP Tandem.* J.S. Fraser and P.R. Tunnicliffe, CRNL-1045, 1974 February.

Figure 1



TYPICAL FOIL LIFETIMES IN THE TERMINAL OF AN MP TANDEM

Figure 2



BUILDING 137
MAIN FLOOR LAYOUT

Berners: On the column break did the glass actually break or did the glass separate from the metal at the glue joint?

Burn: In the first case the glass broke. The second time the glass also broke but the break was closer to the metal.

The Chalk River High Voltage Mass Separator

D.A.S. Walker

This facility is still averaging approximately 100 hours/month for an accumulated running time of 6000 hours. Singly and doubly charged beams of heavy ions accounted for 90% of our operating time during the past 12 months.

We are unable to run reliably above ~ 1.7 MV because of suspected tube loading. The frequent shutdowns for cleanup of the injection system, reported last year, are no longer necessary following a 4-week shutdown during which the entire terminal lens and X-Y steering systems were carefully realigned and suspect electronic components replaced. It is not at all unlikely that some fraction of the source material has found its way into, and eventually formed a deposit on, the accelerator tube proper, causing increased leakage from the terminal. A full complement of tube sections and coupling flanges, together with a complete set of NEC "second generation" corona points are ready for installation. We are in the prolonged "HOLD" state with which most of you are surely familiar, wherein the accelerator is in need of maintenance to return it to rated specifications, but such time cannot be fit into the schedule because you can still provide a bit of beam to the target area.

Our 2UH still uses the original pulley design, based on spring contact with the chain. During "loss of upcharge" investigations we installed a pellet monitoring system similar to the BNL design described at SNEAP '78. This led to the installation of a new set of springs in our base pulley and improved charging uniformity. Details of this investigation will be given in a later session.

No SNEAP meeting would be complete without reference to a portable SF_6 detector. We recently purchased an Alltemp Products Co. model HH300 "Halide Hound". This instrument has impressive sensitivity and, with SF_6 presently at $435.00 per cylinder, it paid for itself in short order.

User requests for more complete beam diagnostics have led to the development of an "in line" beam current monitor and a massmeter. A lengthy effort into the use of fast integrators and sample and hold circuits, based on the time related information provided by our five NEC Beam Profile Monitors, proved of little practical use as an "absolute" device because of the uncertainties associated with secondary electron production within the monitor, and differences in scanning motor speeds. A much simpler system, based on slower integration of the BPM preamp output provides an adequate monitor. Once the beam shape and intensity have been optimized for a given experiment, the monitor readout can be normalized with a Faraday cup and used as a measure of beam intensity during that run. This method of measurement obviates direct connection to the target and is, therefore, independent of target material (i.e. no variation in secondary electron emission).

Analog signals, derived from the accelerator generating voltmeter (GVM), and from a Hall probe located in the analyzing magnet, are fed into a circuit whose output generates a signal proportional to $M = k B^2/V$, where M is the mass (amu), B is the magnetic field, V is the accelerator voltage and k is a calibration constant. Bench tests indicate a system accuracy of approximately 0.3%. Once normalized "on-line", no change in k was required for the following conditions: a) generating voltmeter variations of 0.6-1.5 MV at mass 28 and b) mass variations of 12-44 at 1.5 MV. Case (b) illustrates our most practical application. Subsequent runs have extended the range to mass 132. Inherent

nonlinearities in the GVM necessitate minor recalibration, primarily at lower (< 500 kV) terminal potentials. As might be expected, recalibration becomes more critical for large masses at low terminal potentials. To date the maximum recalibration correction is less than 1% which is well within the original design goal.

For part of the year we had an electrostatic charge state and energy analyzer in operation in the target area. We have made use of the charge state distributions and energies of ions transmitted through a foil placed in the beam to permit identification of the beam constituents (both atomic and molecular, the latter dissociating into equal velocity fragments in the foil). With the help of this diagnostic method we have identified $^{14}N^{++}$ (2E) and $^{14}N_2^{++}$ (E) beams at a magnet setting appropriate to a mass 7, charge state 1, beam of energy E, the molecular beam charge changing to 2+ in the flight path between the acceleration tube and analyzing magnet. The intensity ratio N_2^{++}/N_2^{+} was $\sim 10^{-3}$. Similar analyses have identified an $(AlCl)^{++}$ beam at "mass 31" and a $(BF_2)^{+}$ beam at "mass 49". A full report will be presented at next month's conference on the Application of Accelerators in Research and Industry to be held in Denton, Texas.

TRIANGLE UNIVERSITIES NUCLEAR LABORATORY

Duke Station
Durham, North Carolina 27706

John F. Wilkerson and Chris R. Westerfeldt

In the last year several projects in the laboratory have continued or begun:

1) Polarized Ion Source Upgrading - The Lamb - Shift polarized source is being modified as quickly as the experimental program will allow. New interlock and control systems based on a (M-6800) microprocessor have been built and are being installed. New spin filter coils, r.f. cavity, argon cryopumping, and fast spin - flip systems are in various stages of design, assembly, and testing. The bunched polarized beam continues to run reliably and often for an active program using pulsed polarized neutrons from the $D(\vec{d},\vec{n})^3He$ reaction.

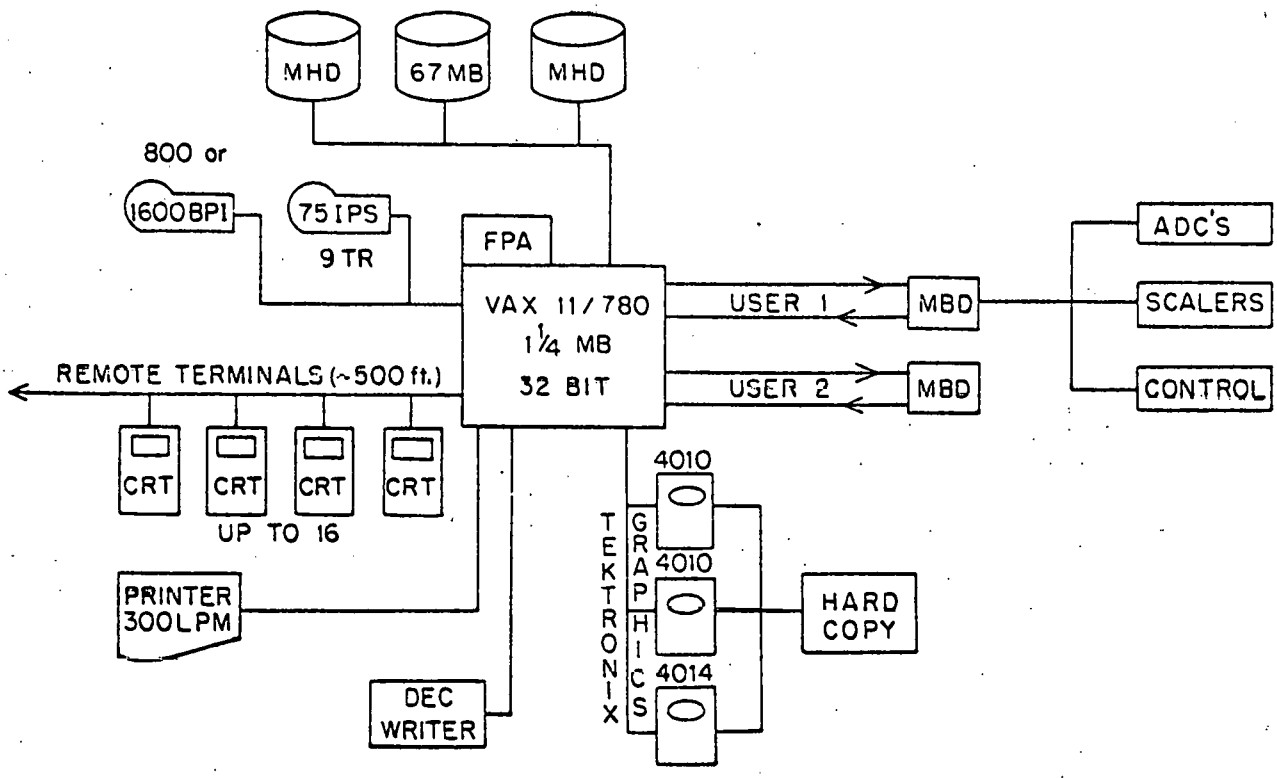
2) Cryogenic Polarized Target Facility - Within the last six months the laboratory has recieved funds to purchase a ^3He - ^4He dilution refrigerator, a cryostat, and a superconducting magnet system as a basis for a new system for producing spin polarized targets by brute - force cooling to low temperature. The basic physics interest is in spin - spin effects in neutron - nucleus scattering as observed in experiments using polarized neutron beams incident on these polarized targets. The refrigerator and magnet have been ordered, and the cryostat is being designed. Component delivery and initial system tests are expected in late summer or fall 1981.

3) A Pulsed Beam Injector for the Cyclotron - To facilitate experiments at cyclograff energies (Ep = 17 to 32 MeV, Ed = 17 to 25 MeV) which utilize neutron time of flight techniques to detect the outgoing products of elastic scattering or reactions, a project has just begun to develop a new external ion source for the TUNL cyclotron pulsed at 2.5 or 5 MHz. The goal is to pulse and bunch the external beam from this source efficiently at 1/5 or 1/10 the cyclotron frequency, inject this beam into the cyclotron, accelereate it, and extract it cleanly in bursts at 200 ns or 400 ns intervals. Design and construction of the new ion source has just begun. Tests of prototype r.f. systems for bunching the beam are proceeding separately.

4) New Detector for the Radiative Capture Program - The gamma-ray detector presently used in the TUNL radiative capture program

is a Bicron 25.4 x 25.4 cm NaI detector viewed by six RCA 8575 photomultiplier tubes. The NaI detector is surrounded by an NE-110 plastic annulus viewed by eight XP-1031 photomultiplier tubes. The shield is operated in anticoincidence with the NaI detector primarily to eliminate cosmic ray background in the data. The detector assembly is enclosed in a massive shield of lead and paraffin (doped with Li_2CO_3) and mounted on a steel carriage which rolls on steel plates to enable angle and target-detector distance selection. We have ordered a duplicate of the NaI detector and anticoincidence annulus from Bicron, and it is to be delivered during 1980. Work on the supporting carriage is nearly completed in the TUNL shop. The availability of a second detector will reduce the time required for data taking and substantially improve the accuracy of analyzing power data since it will permit simultaneous measurements on both sides of the beam.

5) A New Computing Facility at TUNL - Early this year, TUNL took delivery of a DEC VAX 11/780 computer system, which will soon replace the two DDP-224 computers, in use since 1965, for data acquisition and analysis. The present configuration of this new system is diagrammed below.



This system was up for interactive and batch job usage soon after acceptance. At this time, enough of the data acquisition interface has been implemented to allow acquisition of single parameter data. The data acquisition programming that has been installed is an event analysis language (EVAL). EVAL provides a language which is broad enough to allow data sorting algorithms to be easily specified, but which is simple enough that the compiler is able to generate code which executes quite quickly. With EVAL, allocation of data arrays, parameters, and devices is done dynamically. This means that there is no need to compile separate data taking programs specific to each experiment. In a sense, there is only one such program, general enough to let each user configure it to his own needs at run time. At this time we expect to be able to use the VAX for data acquisition early next year.

6) The 3 MeV Accelerator Laboratory - We are preparing to upgrade our 3 MV KN Van de Graaff accelerator in several areas. We have ordered from HVEC, a stainless steel electrode accelerator tube, with which we hope to obtain stable operation at 4 MV. At present we are able to operate as high as 3.5 MV with no instability or breakdown.

We are also upgrading our electrostatic analyzer and associated high resolution system. This system utilizes the HH^+ beam from the ion source to measure the beam energy fluctuations and put a correction signal on the target rod. The present system limits high-resolution work to 3.3 MeV.

The third upgrading project involves the design and installation of a terminal stabilizer for this machine. Our design is an improved version of the system installed in the 5.5 MV CN Van de Graaff at Zurich in 1967. We plan to obtain our error signal from the electrostatic analyzer as before, or in the case of other beams, from a system of slits located in the experimental beam leg. The terminal stabilizer has been designed and is in the process of being debugged. We hope to be able to install it for testing later on this fall.

McKay: What beam currents do you get from the polarized source?

Wilkerson: I'll answer for Chris. From the source we get about 350 nA continuous beam. In pulsed mode we get 200 nA of polarized deuterons. Analyzed beam is about 75 to 100 nA.

FLORIDA STATE UNIVERSITY

TANDEM LAB REPORT

K. Chapman

The S.F.N. tandem at Florida State has operated reliably during the last year. Most of our effort has been directed towards planning and the initial stages of construction and installation for the cryogenic booster.

A source data link was built and installed and has worked well. This uses a fiber optic isolation bundle and digital read out. The stabilizer on the pre-accelerator supply was redesigned to provide a higher degree of stabilization and has proved quite helpful.

A new belt was installed in January. The old one had not failed, but was in very poor condition. We followed our usual procedure of conditioning for this new belt and after approximately two weeks, a tank entry was made to retrack the belt and clean the screens. It has, to date, run approximately 4,000 hours.

One troublesome problem that developed in July was the development of a pressure sensitive leak in the terminal region. This was finally traced down to a faulty lead through insulator on the stripper box for the terminal steerers.

Water cooling has also caused considerable difficulty during the year though little down time. In March our water chiller, which was installed in 1959, failed due to corrosion in a heat exchanger allowing water into the compressor and freon circuit. We continued to run by tapping into the campus wide chilled water system but, unfortunately, this supply is contaminated with soil. This required us to change our main water filters every 5 days; and still, sufficient very fine material passed through the filters that the cooling coils on diffusion pumps started to block. This has now been cured by installation of a new heat exchanger cooled by the campus system and we recirculate clean water in a closed system through the pumps.

A terminal data link was installed during the year. In January the fast light link to the terminal controlling the potential on the modulated stripper was installed and worked well. In May the data link from the terminal to the control console was put into operation. Some problems with cross-talk between the up and down light links and vulnerability of the photo diodes to spark damage

still needs more work, but the system works well and no problems have been encountered with damage to the terminal electronics.

A comprehensive system of tandem parameter monitoring is nearing completion and monitor boxes were installed on all supplies during the year. These give an output signal compatable with the micro-computer which will scan these outputs. Data will be available as hard copy by interfacing with a printer and the computer will retain data obtained prior to a fault for later recovery.

As described in our last report, the pre-tandem pulser and new low energy beam line were installed last year. This year the resonator cryostat was completed and the resonator installed and tested off line. In August this cryostat was installed between the 90° and beam switching magnets. Considerable changes were made to the high energy vacuum systems to protect the resonator. A vacuum protection system was built and installed with pneumatic valves on either side of the cryostat and sensing units in all experimental beam lines. The pump under the switching magnet was replaced by a turbo-molecular one. The two pumps before and after the 90° magnet were replaced with a single turbo pump, pumping into both beam lines by means of a 6 inch pumping link.

The sweeping plates and phase sensor were also installed, the former before and the latter after the 90° magnet.

The high energy pumping station was replaced with a cryopump also in August. This was of the same type installed in the low energy line last year. We have been very satisfied with these pumps. The low energy one has now been operating for over a year with no serious problems and the efficiency of the system is indicated by a recent foil change. The tube was let up to dry nitrogen and the foils changed. The system was evacuated to approximately 100μ with a mechanical pump and then opened to the cryopumps. By the time the operator had returned to the control room, both high and low energy vacuums were $< 10^{-5}$mm. The vacuum continued to improve rapidly to approximately 10^{-7}mm.

Testing of the resonator with beam is just starting. It has been cooled to liquid helium temperature in its new position and R. F. tests completed. Beam tests will be conducted during the next weeks.

The building to house the linac booster is well under way. The architect has completed the plans and the contract is about to be bid.

University of Guelph
Guelph, Ontario

Laboratory Report Presented to SNEAP, October 1980

Richard Gingerich, Physics Department.

Accelerator

Model KN3000, HVEC, 3Mv.
Ion Source - protons, alpha particles.
Beam current is typically less than 10 μa because of the accelerator proximity to public areas and the type of experiments done.

Experimental Facilities

Target chamber for Rutherford Backscattering and channeling experiments.
Target chamber for PIXE (Proton induced X-ray Emmision).
Proton μ Probe for PIXE. (Under development).

Problems

Instability in the cross sectional homogeneity of the beam, characterized by blowing up, flickering and swirling. (Kaleidoscope effect).

Chapman: What is the humidity of your tank gas?

Gingerich: According to this modern device we have for measuring dew point the dew point is -65°F.

Janzen: Can this be one of those Arrow Industries things for monitoring relative humidity that have carbon dioxide cooled condensation tubes?

Gingerich: I'm not sure.

Walker: Is it the standard HVEC device in the black box?

Gingerich: Yes.

Janzen: It's very unreliable. You probably need to dry the gas.

Gingerich: It was a very low-cost device. We are recirculating our gas now. That seems to have helped.

Fauska: Did I understand you to ask earlier if there was a low-cost device for monitoring the humidity?

Gingerich: Yes.

Fauska: We use a sensor made by Panametric. It is porous aluminum with a layer of gold on each side that is transparent to air or gases. You measure the capacity of that. That was the device I was speaking of earlier that we had to move outside the tank after we lost four of them. It has worked there for several years now.

Gingerich: Did you buy the electronics or did you make your own?

Fauska: We eventually designed our own.

Noé: You might be interested in an English unit made by Shaw that we use to monitor the dewpoint of the helium gas in our refrigerator. It also works for SF_6.

Lindgren: I would like to note on the Panametric's dewpoint that when that unit fails it indicates a very good dewpoint.

VICKSI Laboratory Report 1979-80

Hahn-Meitner-Institute
Glienicker Str. 100
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by K. Ziegler

The main event on the electrostatic part of our accelerator in the past year, was the necessary change of the charging belt. We use a rebuilt HVEC CN machine with an NEC-tube as injector for our separated sector isochronous cyclotron. The quality and stability of the beam out of the cyclotron is strictly correlated to the phase of the beam injected into the cyclotron, which in turn depends directly from the terminal ripple of the Van-de-Graaff injector. In the best times we have a terminal ripple between 200 - 250 Volts at a terminal voltage of 6 MV which gives a quite satisfactory phase stability. However the terminal ripple kept slowly increasing to as much as four times the best value with a dramatically negative effect on the beam quality and stability. Furthermore it turned out that we needed increasingly higher belt charging voltages hitting the voltage limit of the charging power supply. This made it necessary to reduce the terminal voltages to values below the specified 6 MV. As the charging belt had over 18000 hours of operation we finally decided to change it and install a new belt. This meant a complete dismantling of the column. The dismantling, cleaning, checking of resistors etc. and installation of the new belt took about 5 weeks. With the new belt the ripple was back to normal values, however we started to observe from time to time larger excursions of the terminal voltage. A reworking of the slit control-system amplifiers improved the situation somewhat, however we finally believe, that this problem is caused by a slipping belt. We are using pure SF_6 as insulating gas and are running routinely at a pressure of 6 bar. We plan to make measurements to confirm this suspicion of belt slipage, and we will then install grooved pulleys to improve the situation.

Ashbaugh: We did a number of years ago some calculations on belt slippage in pure SF_6 in our FN, and we believe that at a pressure of 6 bars the belt will float on the pulley. You can get around this by grooving the drive pulley on the drive motor. The grooves need only be a few mils deep and a few mils wide and spaced perhaps a centimeter apart.

K. Ziegler: And this will cause the belt to go straight through the pulley?

Ashbaugh: Yes, straight around the pulleys. I would be happy to send you these calculations.

K. Ziegler: Thank you.

Rowton: High Voltage has been grooving the pulleys that they have designed for driving belts in pure SF_6 for a number of years now. If I remember correctly, the dimensions are for the groove width 1.5 mm and for the depth of the slot 0.75 mm on a 1-inch pitch.

K. Ziegler: We did get this drawing but we were not quite sure how our pulley was built. They did not have drawings of that anymore. So we didn't want to go ahead and put in this groove. Perhaps the wall was not thick enough.

Rowton: I think on all of your pulleys and alternator motors the wall is thick enough to take a 0.030-inch speed groove safely.

K. Ziegler: OK. Thank you.

Saylor: Can you say a little bit more about what you observe when the belt slips?

K. Ziegler: Well, sometimes the terminal power supply voltages will drop. We have two generators there, a 3-phase and a 1-phase. We don't observe the fluctuations in the terminal ripple, but we observe it on the beam. This could mean that some of the power supplies in the terminal don't have the proper input voltage.

Saylor: What kind of time fluctuations do you see?

K. Ziegler: It's in the second range.

Janzen: You said you got 200 volts of ripple on the 6-MV terminal?

K. Ziegler: That's what we measure on the pickup.

Janzen: What do you use as a pickup?

K. Ziegler: A capacitive pickup. I would guess it is accurate to about 20%.

Saylor: Do you control the charge on the belt in any way?

K. Ziegler: No.

Saylor: I was at Utrecht recently and they are actually modulating their belt charge current to reduce terminal ripple.

K. Ziegler: We are observing an excessive fluctuation of the belt charging voltage. Because it is current stabalized the current is rather constant but the voltage is really going up and down. I thought that might be an indication of the belt slipping.

Noé: Can you say a little bit more about your tandem injector?

K. Ziegler: We are preparing to add a straight-through 8-MV NEC 8 UD which will be put in a separate place. We'll keep the CN so that we will be able to switch between the two. We will be feeding into the injection beam line about 8 meters in front of the cyclotron so the matching from the EN beam will be identical to the matching of the CN beam into the cyclotron. There will be a 200-kV platform as an injector. And we plan to put in a sputter source. With the 8 UD we will be able to get 32 MeV per nucleon up to about mass 40.

Noé: Does that require a new building and tower?

K. Ziegler: It requires a new building and tower. It will be a vertical machine straight through. This is because of building restraints. There are buildings all around so the only way we can do it is with a tower.

University of Ife Nuclear Sciences Laboratory

[Editor's note: G.A. Oso and J.A. Idowu of the University of Ife had planned to attend SNEAP and had prepared the following laboratory report. Unfortunately, problems with overseas air transportation connections delayed their arrival in Madison until after the Symposium had ended.]

The Nuclear Research Program at the University of Ife is currently in the development and implementation stage.

A 9MV upgraded FN model Van De Graaf accelerator has been purchased from High Voltage Engineering Corporation and we expect delivery and commencement of installation by end of 1981. The building for the accelerator has been designed and construction work is expected to start by March 1981. The building layout is typical of many FN laboratories in the U.S. The accelerator and target halls assume an L-shape and a large control and data room is sandwitched between them. Three functional beam lines are planned initially and three interchangable injectors will be used. Provisions are included in the building design for possible future post acceleration work and the addition of future new injectors. The gas storage and transfer system is expected to handle simultaneously the use of 100% SF_6 gas and the mixture of $CO_2 + N_2 + 30\%$ SF_6. This approach is to forestall anticipated difficulties in the procurement and handling of SF_6 gas. We intend to test the accelerator system for acceptance with 100% SF_6 gas but make initial runs with gas mixture while conserving the expensive SF_6 until adequate experience is acquired in its handling.

The initial research activities planned with the accelerator include nuclear structure and material studies, gamma-ray spectroscopy, electron-capture, ion chanelling and radiation damage studies, radio-nuclide production and some activation analysis.

Other nuclear research facilities being planned for the laboratories besides the accelerator include a 1-2MW research reactor, Co^{60} gamma source, a neutron generator, and materials studies equipment such as scanning electron microscope, transmission electron microscope, and x-ray units. No commitment has been made on a reactor yet but we have on ground already, a model 1254 neutron generator from Kaman Sciences which we expect to instal early in 1981. It will deliver 14MeV neutrons with a flux of 10^{11} neutron/cm^2/sec. It will be used principally for activation analysis. The Geology Department of the University is interested in analysing geological samples of uranium deposits and tar sand

of which Nigeria has one of the largest deposits. The Agriculture Departments will use the facility to study the effects of germicide sprays on cocoa plantations. Nuclear physics experiments using the neutron generator will include neutron cross-section measurements and neutron radiography. Ultimately, the neutron generator will be used for the determination of the oxygen and silicon contents in steel produced by the nation's steel companies.

We also have on ground a Co^{60} gamma source with an activity of about 10 kilo Curie. It will be used for sterelisation, food preservation and genetic studies.

Our data acquisition system is the PDP11/40 based (Scopio 3000) computer. It is already installed and tested. It is presently being used with NaI detectors and some radioactive sourses for caliberation, and measurement and computation analysis. The system is also used to train some of our data technicians.

The above stated programs form the basis of the first phase of our planned activities for the period 1980-1982. We anticipate a number of problem, the larggest of which is perhaps the lack of trained technical personels. We are making efforts in this direction by attaching our technical staff to other establish laboratories. We maintain a working agreement with McMaster Universityin Canada and we intend to establish similar agreements with other interested laboratories that are willing to assist us. Areas we are paying urgent attention to are Data Acquisition, Electronic Technology, Health Physics and Radiation Protection, Vacuum Technology, and operators training on the major nuclear equipment.

We are working under very adverse conditions. Lack of essential service facilities like constant electrical power supply, difficulties in the procurement of essential equipment and accessories, communication difficulties between us and our overseas vendors are some of the major set-backs that are responsible for delays in the execution of the program.

Lehigh University
Sherman Fairchild Laboratory

Presented by Richard O. White

Sherman Fairchild Laboratories has a KN-2 Horizontal 3 MeV accelerator in operation. The primary usage is for e^- radiation of samples used in EPR, DLTS and ODMR studies performed by Dr. G. D. Watkins' research group. Major additions this year have been a target chamber diffusion pump and four beam lines. One line is dedicated to Rutherford backscattering, one to PIXE (proton induced X-ray emission), both projects directed by Dr. Smith; one for channeling studies by Dr. Kanofsky, and the fourth for high energy gamma radiation.

Early in our attempt to achieve positive beam operation, we modified the original positive RF source using transformer coupling instead of capacitive coupling. This technique is used at Bell Labs at Murray Hill, NJ, and was suggested to us by Mr. W. Augustyniak to overcome a tuning problem (Fig. 1).

Our system has operated with positive beams of H_2 and H_e with currents up to 10 UA at 1 MeV. Because the source fails to ignite when the tank pressure exceeds 200 PSI, we have had to lower the pressure to 195 PSI, which allows a maximum stable voltage of 1.5 MeV. We are concluding external pressure tests on the source to find out why the source fails to ignite when the tank pressure exceeds 200 PSI. No specific results have been reached thus far.

Because of the need for an electron beam, an attempt to generate electrons using secondary emission from a H_e^+ beam was tried but had doubtful success. A diffuse 3 UA 1.5 MeV e^- beam was created but terminal voltage

became unstable and internal focusing had little effect on the electron beam size. With a TV monitor we viewed an angled lucite target in air placed at the end of the beam line. It showed no concise beam pattern. However, a dim circular fluorescent area approximately 18 cm^2 in the lucite showed that we had very diffuse unusable beam. Further experimentation with this method has been suspended until the RF source has completed its tests. We will try again when the system is converted back to positive ions in November of this year.

With such a low yield of e^- by secondary emission we had to adapt the positive tube to a standard electron filament source. Our requirements were for e^- beams of 20 to 80 UA currents. The KN-2 system is now in operation with a positive accelerator tube and an electron filament source, and can be reconfigured to positive RF ion operation within a down time of three to four days. Our electron operation has usable beam currents of 5 to 80 UAmps and energies of 1.5 to 2.5 MeV.

Figure 2 shows thermofax targets of the electron beam in air 1/2" away from the 5 mill Al exit window. The beam diameter varies from 1.5 to 3.0 cm. The only focusing of the beam is done by a high voltage supply in the terminal. It is connected to the extraction tube internal to the accelerator tube.

Other items brought on line this year are the original corona stabilizer unit, and the voltage stabilizer for electron operation.

(1) 1.8 μh choke
(2) Teflon block
(3) 1/8" soft copper tubing
(4) Ceramic piston tuning capacitor

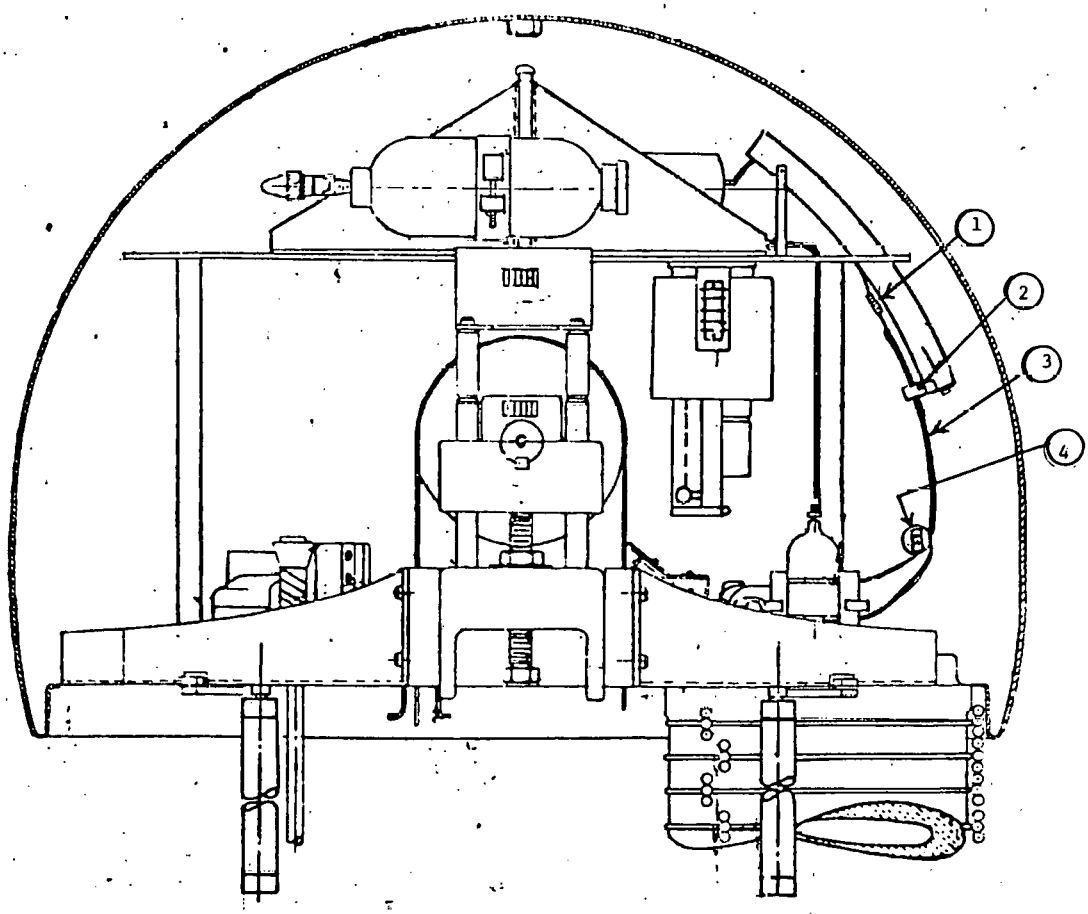
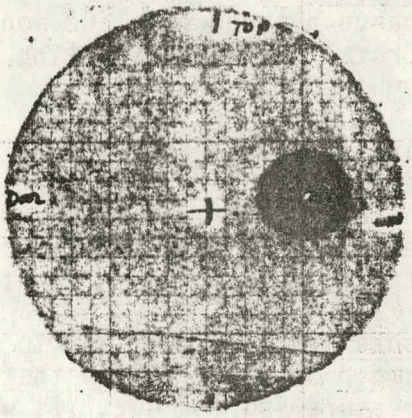


Figure 1

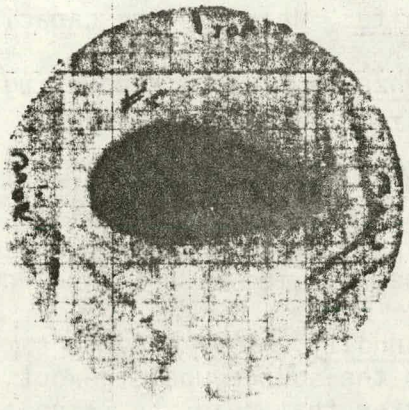
ELECTRON BEAM THERMOFAX

2 MeV 20 μAmps at 60 sec exposure



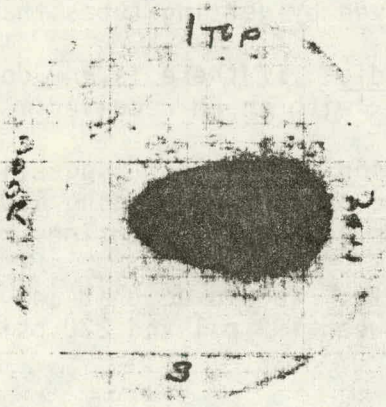
Beam line repositioned

2 MeV 20 μAmps at 75 sec exposure



1.5 MeV 40 μAmps 20 sec exposure

Changed material to heat thermofax



2.5 MeV 60μAmps 10 sec exposure

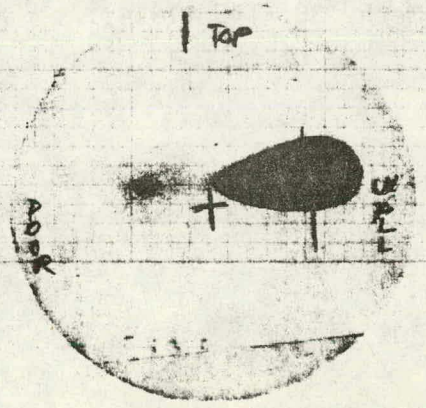


Figure 2

Berners: Some 15 years ago we also had to open the tank and beam tube to change between positive ions and electrons. It used to take 2 days, but after a rotating source mounting arrangement was installed in the terminal the time required was only 2 hours. I can give you a reference to a paper on the device.

Janzen: When you say that the source doesn't ignite, are you sure the RF is on?

White: As far as we know, yes. But we have no telemetry equipment inside the terminal. The source did come on when we lowered the tank pressure. We started at 275-280 psi. As we went down in steps of 25 psi the source came on at 195 psi.

Janzen: It's important to make sure the RF does come on. We had exactly the same problem. I found that it was due to a small mica capacitor which under pressure shorted out. It was all right at atmospheric pressure.

White: Where is the capacitor located?

Janzen: It is a decoupling capacitor from the RF tank circuit to ground. One way to find out if your RF is coming on is to mount a little antenna in the tank.

White: We have an external stainless steel tank that we will be able to pressurize to about 300 psi for checking the entire system. We'll look into that capacitor effect. The capacitor in the circuit now is probably the original one supplied with the source.

Connolly: I've had the same problem in a 2-MV machine. As the pressure went up the source would go out. This machine had a window that allowed us to see a meter that read the B+ voltage. At a certain pressure the B+ would cut out. We traced it to a 5T4 tube that was breaking down under pressure. The problem was solved by getting tubes that had been pressure tested.

Goldie: If there is any doubt about that you can hear the RF from outside the tank with an FM receiver.

Larson: That last suggestion is very good. I was going to suggest that you have the source running and then increase the pressure and see if it cuts out. This could be a starting problem as opposed to an oscillator problem.

White: We did do that and it did go out at about 220 psi. The source flickered between 195 psi and 220 psi pressure.

* C.P. Browne, A.L. Schaller, W.C. Miller, and S.E. Darden, Nucl. Instrum. & Meth. 30, 145 (1964).

LOS ALAMOS 3-STAGE VAN DE GRAAFF FACILITY

presented by Robert Hardekopf

The major development at the LASL Van de Graaff facility this year was the installation of a heavy-ion sputter source in the terminal of our single-ended "Vertical" accelerator. Since initial operation in July 1980, we have run over 800 hours of 3-stage heavy ions in which negative beam from the Vertical accelerator is injected into the FN tandem accelerator. The most popular accelerated particle has been ^{14}C, and most of the 3-stage time has been at about 6 MV on the Vertical and 8.5 MV on the tandem for a 65 MeV 6^+ ^{14}C beam.

Vertical Accelerator

The total amount of running time for the Vertical accelerator during this year, about 1900 hours, is a record for the last 5 years. This is especially significant since during more than half of this period the accelerator was operated only one week out of four to allow time for development of the heavy-ion terminal. Table 1 is a breakdown of the hours for this period.

Several hundred hours were spent testing the light-ion terminal pulsed-beam operation, and the initial problems observed with this terminal seem to now be under control.

Terminal voltage capabilities are still less than anticipated. Experiments have been run at 7.6 MV negative, but only after the tube pressure near the terminal was increased. The magnetically suppressed accelerating tube is showing considerable radiation-caused glass darkening near the magnets, but no interior spark tracks on the glass have been observed and no tube resistors have failed. The main problem is excessive tube activity at the higher terminal voltages.

Tandem Accelerator

Total operation for the FN tandem accelerator during this year was about 5200 hours, of which about 970 was 3-stage. The total is about average for the last 5 years, but there is a definite shift toward heavier ion beams and higher energies, as the statistics in Tables 2 and 3 indicate. Eighty-seven percent of the time was at terminal voltages of 7.5 MV and above.

The most popular beams were tritium and ^{14}C, reflecting the uniqueness of these radioactive particles in nuclear structure studies. Polarized particles were less in demand this year than in the past, partly because of some difficult maintenance problems with the polarized sources.

The major tandem maintenance items this year were an optical realignment of the beam handling equipment and replacement of the charging belt. Washing of the column structure and internal tank

components with detergent and water remarkably improved subsequent voltage capability.

Because of poor foil lifetimes with the heavier beams, particularly ^{28}Si, we have been flashing alternate foils in the holder with a xenon strobe light. The immediate effect of the flashing is that the foil becomes very slack in its holder. Although statistics are somewhat poor at present, the flashed foils appear to last at least twice as long as the unflashed foils under heavy-ion bombardment.

Table 1 - Operating Hours For LASL Vertical Accelerator October 1979-September 1980

Operation	Hours
Single Stage Positive	120
Single Stage Negative	485
3-Stage Light Ions	159
3-Stage Heavy Ions	810
Testing and Conditioning	323
TOTAL	1897

Table 2 - Operating Hours for LASL Tandem Accelerator October 1979-September 1980

Beam	Hours	% of Beam Time
p	448	9
d	733	14
t	1066	21
Polarized p,d	329	6
Polarized t	334	6
^{3}He	312	6
^{4}He	64	1
^{7}Li	144	3
^{10}B	74	1
^{16}O	211	4
^{18}O	155	2
^{14}C	877	17
^{28}Si	518	10
	5225	

Table 3 - Terminal Potential For LASL Tandem Accelerator, October 1979-September 1980

Potential (MV)	% of Beam Time
1.0-4.9	6
5.0-5.9	3
6.0-6.9	4
7.0-7.9	21
8.0-8.9	65
9.0-9.3	1

K. Ziegler: What is the intensity of the ^{14}C beam?

Hardekopf: About 300 nA for the $^{14}C^{6+}$ beam.

K. Ziegler: Is that the electrical or particle current?

Hardekopf: That is the electrical beam current for the 6+ charge state. This is beam on target in a fairly restricted beam line associated with the spectrometer.

University of Lowell

Charles E. Connolly

During this past year we were able to use our 2-MeV Van de Graaff Accelerator in the junior and senior laboratory schedule. This facility has all been built from surplus material and includes a homemade chassis and terminal. An old deflection magnet was modified and installed as an analyzing magnet with three ports at 0 and ±25 degrees. We restored the old NMR manufactured by Agallala. This system came with the original CN installation. Students helped build a corona stabilizing circuit and components similar to the system on our CN machine. There has been good student interest in this lab. This type of involvement is a great help to our undergraduate physics program.

The CN accelerator developed trouble with the belt charging system. We were unable to transfer enough charge to the belt. New points were installed on the spray bar and we replaced all the resistors in series with the points. The old resistors had changed value dramatically. We like the spray charge system; it has given us good service. Our belt and tube now have over 27,000 hours of service. The belt still has a hard and shiny surface and the tube holds voltage very well.

Our 80-kV belt charge power supply, an old tube rectifier type, started breaking down until, finally, we lost the filament transformer. We then replaced the tubes with solid-state rectifiers. The power supply is back on the job. We have also rearranged components and chassis in the console for more efficiency and ease of operation.

A safety program was reinstated after an accident involving a work-study student in the reactor laboratory. The accident was mechanical and did not involve radiation. The student was not seriously injured, but an accident such as this shows how important it is for all of us to continuously search our labs for hazards, especially for those hazards not involving radiation.

McMaster Tandem Accelerator Laboratory

FN Operation

Machine operation has been routine during the last twelve months. A few successful runs were accomplished at 9 MV, but in general, high voltage operation has been restricted due to the condition of the tubes. Some low value resistors have been installed on damaged sections in number 3 tube. We now have over 70,000 hours on our accelerator tubes.

Calculations were made to check the beam transmission through the machine. Regrading of resistors in the injection area of the #1 tube has resulted in significant improvement in beam transmission. We are now transmitting 60% or better of the low energy current to the image cup (protons and deuterons).

During the week of September 27, 1980, a five day run using low intensity triton beams was successfully performed. Many of the components used in the previous triton run were reused with minimal contamination of the ion source box or "down-stream" components. At the conclusion of the run, the entire source was moved to the tritium source rack for use in future experiments.

Ion Sources

The reflected Cs sputter source for obtaining negative ion beams from milligram-sized samples is now in operation. Beams of 25 μamps of $^{12}C^-$ and .5 μamps of BeO^- have been produced. Development work is continuing, to improve reliability and output, and to reduce sample changing time.

This source is now currently used for all heavy ion work.

Pulsed Beam System

The fast beam chopper at the high energy extension has been rebuilt and the driving electronics modified to enable operation at 10 MHz. Measured burst

widths of 1 to 2 ns have been obtained easily for proton and α beams. The 10 MHz injection buncher and its driving electronics are complete. Measured bunch widths have been limited to 3 ns for protons and 3.5 ns for α beams by velocity spread in the ion source. These widths will improve after the planned ion source upgrading to allow 100 kV injection energies is completed. Using the fast chopper to take a 2 ns "slice" of the present beam bunches will allow approximately 20% beam utilization.

The plates for the μs pulser, already in place after the analyzing magnet, will be used to reject unwanted bursts, providing beam burst separations of 50, 100, 150 ns etc. Design work on the driving electronics is underway.

Electronics

Three research instruments have been developed during the past year; a Time to Digital Converter, a Multiplicity Filter unit and a Five Input Coincidence unit.

There is no commercial instrument available for measuring time intervals in the range of a few microseconds to about 0.1 seconds. A Time to Amplitude Converter was developed to fill this need. It consists of a counter and a 20 Mhz crystal oscillator with the necessary logic to start counting and to read the contents of the counter when an event occurs. It contains a first-in-first-out memory that can store the time of occurrence of as many as 64 events in a burst. The instrument is constructed in a NIM module and replaces an ADC in the pulse height analyzer system.

The Multiplicity Filter consists of circuits to detect the time of occurrence of events from 8 scintillation counters, and logic circuits that produce an output pulse if any two or more signals occur in coincidence within 100 nanoseconds. It is used to study decay schemes and filters out the high rate of

singles events. All of the circuits are contained in a single 10" x 12" chassis.

The Five Input Coincidence unit is contained in a NIM module. It is designed to receive the timing signals from up to 5 Ge detectors. Logic signals are produced when double or triple coincidences are detected. It is possible to gate this unit by the output of the Multiplicity Filter unit.

Accelerator Utilization

(based on belt charge timer, Oct. 31 to Oct. 31)

	1977	1978	1979	1980 est.
FN hours	6055	5125	5526	5715
% utilization	69%	59%	63%	65%
Max FN voltage	9.3	8.7	9.0	9.0
No. of days at or above 9 MV	50	0	1	4
No. of ion species	12	15	16	17
% p & d (unpolarized)	33%	36%	23%	24%
% $\vec{p}$ & $\vec{d}$ (polarized)	30%	9%	11%	17%
% tritium	---	7%	15%	2%
% ^{3}He and ^{4}He	15%	19%	28%	23%
% other ions	22%	29%	23%	34%
KN hours	3382	3330	2465	3340
% utilization	39%	38%	28%	38%

Rowton: John, you mentioned that you were going to split your extraction electrode so you could steer the Cs beam. Are you using the concentric reflection geometry or the off-center reflection geometry?

McKay: Actually, Phil had better talk about this. It's not the extraction electrode, though. It is the einzel lens.

Ashbaugh: We are using the off-axis geometry. We moved the whole source off axis as well as the cone. That is, the Cs gun and the split einzel lens will move as a unit off axis in any direction. The cone will only move up and down. We now offset the cones by about 0.080 inch and the Cs lens by about the same amount. The material of the cone is then on axis. We have not had much experience with the split lens yet, but we built it and it looks nice.

Rowton: We have two sputter sources at Los Alamos. One uses the off-axis geometry and the other uses the concentric geometry. Our feeling is that the concentric geometry is much nicer and less troublesome. It is risky to say there are differences in output.

McKay: The off-axis one has the advantage when you are using very small sample sizes.

Rowton: Well, not really. Our concentric cones can take extremely small samples.

Saylor: We have two sources at Pittsburgh. One is in the terminal of the first machine and the other is external to the machine. On both of them we have a totally concentric geometry with a large aperture cone. Across the center of the cone is a bar of the material we want to sputter.

Rowton: I've used that method also. We now use a machined cone that has a support bar through the middle and a little pellet in the middle of the bar. The sample goes in a recess in the pellet. One can use a standard Middleton-type source and positive reflection voltage on the negative extractor. With that method steering the Cs might not help at all, and it is a much simpler system.

1980 SNEAP LABORATORY REPORT

Laboratoire de Physique Nucléaire, Université de Montréal

Claude Brassard.

1- Design of new particle accelerators

In September of 1980, the University has appointed three research associates to work full time with the laboratory staff on the design of a new 1 GeV CW electron accelerator which will be constructed in the Montréal area. Our Dynamitron will be fitted with an electron source and serve as a 3.5 MeV, 2mA injector in the initial stage of the project. The design of the injection buncher and of the first stage 150 MeV CW racetrack microtron will be completed in October of 1981. The construction of some of the critical components of this new machine will begin in January of 1981.

2- Operation of the tandem and of the Dynamitron

1980 has been a very good year for our tandem (EN-1), which provided 6566 hours of beam time. Our 4 MV Dynamitron has reached an excellent reliability this year. The relatively low (449 hour) beam time was limited mainly by the ackwardness of working with a single beam line (no switching magnet) and by the fact that the physicists still refrain from using the machine during the evening and the night shifts, when no technician is available. The installation of a second beam line, which is planned for 1981, should help considerably.

3- Development related to SF_6

The work related to SF_6 purification, fast transfer and storage systems are covered in a separate article.

4- SF_6 Alarm system

In 1980, we have designed a remote SF_6 pressure monitoring device based on variable reluctance transducers. This monitor is fitted with an alarm which will warn us in case of an unexpected loss of pressure in the tandem, in the Dynamitron or in the underground reservoir. We hope to avoid the recurrence of last year's incident, when 3000 SCFM of SF_6 were lost through a leak in the underground reservoir, during the summer vacation.

5- New column resistors for the tandem

Over the years, the resistance of our column resistors had drifted towards 700 MΩ, and it was becoming increasingly difficult to stabilize the beam at low operating voltage. In view of the difficulty

of obtaining new resistors from HVEC, we decided to design and construct our own. Each column resistor was made up from 48 one-watt, 10 MΩ composition carbon resistor (Allen-Bradley) wired in series in groups of 12 and fitted in an insulating tube. A complete set of these new column resistors was installed in January of 1980 in the low-energy end of the machine, and they have performed beautifully for the last 11 months. The absence of spark gap does not seem to affect the reliability. In addition to the lower cost, our design presents the advantage of making repairs possible.

6- Belt problems

The last two belts purchased from HVEC have given us great difficulties; they appear to be mechanically very different from the previous belts. Upon initial installation, they require daily stretching for weeks, before they stabilize. They also damage very easily and wear out in some 1500 hours of normal operation.

7- Injection of the Dynamitron into the tandem

In 1978, we obtained a good transmission for the H^- ion beam from the Dynamitron into the tandem. We also realized at the time that the injection beam line would not transmit high rigidity beams. Since Oxygen, Nitrogen and Carbon beams of high energies are required for our experimental program, we have redesigned the injection beam line. The second quadrupole was changed in order to increase the focussing power; new deflectors were installed and the power of the old deflectors was increased. The optics has been recalculated carefully, following a series of beam emittance measurements. A double waist has been set halfway between the two quadrupoles, where it is observed with an NEC beam-profile monitor.

8- Beam pulsing

A magnetic beam pulsing with moderate risetime ($\sim$1μs) is under construction. Its installation is planned at the Dynamitron, where it will be used in connection with activation experiments. At the tandem, a similar system will be used to help in measuring the beam intensity, by switching the beam rapidly between the target and a calibrated Faraday cup, with a calibrated duty cycle.

9- Telemetry system

A telemetry system based on an open light link, similar to the one designed by Ed Burners of Notre-Dame, is under construction. It will link the terminal of the Dynamitron to the outside world through a convenient window in the tank.

Janzen: I have a question about the problem of the belt stretching. Does HVEC prestretch the charging belts before they are installed?

Goldie: Yes, we do. But the rubber seems to be softer than it used to be.

Letournel: We find that the belts are damaged only by electrical processes.

University of Notre Dame

Nuclear Structure Laboratory

Our lab has an FN Tandem with a charge-exchange ion source and a polarized ion source, and a 3 MV single-ended electrostatic accelerator which is used as a heavy-ion injector for the Tandem. The injector has a sputter source in the terminal.

We have built and installed a stripping foil changer with 70 foils on a conveyor chain. It is much like foil changers designed some years ago at Triangle Universities Nuclear Labs and Purdue University; both labs kindly sent us their drawings. The foil frames are made of nickel sheet, electropolished after machining. Each frame is held in place on the chain by a ceramic button magnet. Loading of foils is quick and easy.

When the Tandem tank was opened last May to install the foil changer, we found it necessary to re-align the accelerating tubes. During this time, while the tubes were at atmospheric pressure, tube no. 2 broke into two pieces and the broken ends dropped down onto the string shields. About ten days later, while tube no. 2 was at Potentials, Inc. being repaired, tube no. 1 also broke.

We decided immediately to have all four tubes rebuilt. The work was done by Potentials, Inc. Conditioning of the rebuilt tubes started on August 3. The tubes conditioned easily and are vacuum tight. The highest voltage reached so far is 8.95 MV, and we expect to go higher. Transmission is about 60% for light ions from the charge exchange ion source, and about 100% for 2.5 MeV heavy ions from the injector. (A more complete report on the performance of the rebuilt tubes will be given in Session VIII.)

All of our tubes now have a full set of string shields mounted underneath so that any future broken tubes will be caught before they can fall and damage something else.

Since December 1977 we have had a titanium sublimation pump in the terminal of the injector. The pump holds two 10 gram cartridges, NEC type TSC-10-6. The most recent pair ran for 554 and 676 hours consecutively, from 8 August 1979 to 23 April 1980. With the sputter source and the pump both on, the source pressure stays at 1-2 microtorr with about 200 watts to the sublimator.

After finding it impossible to obtain P-774-040 column resistors from HVEC, we bought 30 resistors from Potentials, Inc. and installed 24 of them at the time the rebuilt tubes were installed. So far, these resistors have survived and have been stable. They are made of 2-watt carbon resistors contained in a phenolic tube.

Saylor: What is the vacuum without the sublimation pumps?

Berners: We couldn't measure it before so I don't know for sure. My guess is that it was between 10^{-5} Torr and 10^{-4} Torr.

Burn: What is the cost of the 10-gram titanium sublimators and what sort of power supply do you use to run them?

Berners: The power supply is a home made one which is an aluminum foil wound step-down transformer fed by a variac. I don't know what the cost of the sublimators is off hand. Maybe someone from NEC can tell us.

Norton: The 10-gram sublimators are about $100.00. We also have a 20-gram sublimator which is actually cheaper (about $85.00) because we sell more of them and, therefore, make them in larger lots.

STATUS OF THE OAK RIDGE 25 MV TANDEM ACCELERATOR

N.F. Ziegler, E.G. Richardson, J.E. Mann, R.C. Juras,
C.M. Jones, J.A. Biggerstaff, and J.A. Benjamin

Oak Ridge National Laboratory
Oak Ridge, Tennessee 37830

Schedule

Installation of major components of the NEC 25 URC tandem accelerator was completed in January, 1980, and initial commissioning activities were begun at that time by NEC. In the interval since January, several milestones have been passed. The most significant of these were acceleration of beam through the machine and completion of beam acceptance tests at 7.5 MV and at 17.5 MV.

Beam was first accelerated through the complete beam transport system on May 12, 1980. Following conditioning of the acceleration tubes to about 17 MV, a 60 pnA (particle nanoampere) $^{16}O^{6+}$ beam at 108 MeV was obtained. This beam was produced by foil stripping a 750 pnA OH^- beam injected at an energy of 0.3 MeV. Due to the excellent voltage stability of the accelerator, tuning of the beam through the terminal and analyzing magnets was found to be straightforward and, in fact, was performed without use of the terminal potential stabilization system. Following further work required to commission the terminal gas stripper and terminal potential stabilizer, an absolute calibration of the lower range of the analyzing magnet, based on the reaction $^{12}C(^{16}O, \alpha)^{24}Mg$, was completed in June.

The first four system acceptance tests were performed successfully in July and August at terminal potentials of 7.5 and 17.5 MV. In these tests, analyzed beams of ^{127}I were demonstrated with intensities of 10 pnA (6×10^{10} particles/sec) and 1 pμA (6×10^{12} particles/sec) for terminal foil and gas strippers, respectively. Calibration of the upper range of the analyzing magnet was also completed in this interval.

On August 20, the accelerator vessel was opened for a maintenance period which ended October 6. Significant activities during this period included repair of all known vacuum leaks, baking (for 16 days) of the acceleration tubes, major modification of the corona voltage grading system, and installation of the ORNL Aarhus-type Penning source in the injector.

After acceleration tube conditioning, NEC plans to begin performance of acceptance tests at a terminal potential of 25 MV in late October or early November.

Problems

Two significant problems have been encountered to date: 1) Failure of tubing connectors on terminal Faraday cups and variable apertures has resulted in three incidents in which the entire terminal cooling system water supply (about 20 gallons) was released onto the column. Vacuum leaks associated with these incidents suggest that corrosion of aluminum gaskets may be caused by liquid water trapped in the space between flanges. 2) A significant SF_6 leak was discovered in the manway of one of the three storage tanks. Investigation showed that this leak was due to corrosion, probably induced by water originating from the terminal water leaks described above.

Modifications

A significant modification to the accelerator in August of this year was replacement of the enclosed corona grading system with an open point system. NEC proposed this change following poor experience with the enclosed system on the NEC 20 MV tandem accelerator in Japan. At present, the replacement is considered a test. A final decision on the type of corona grading system will follow the 25 MV acceptance tests.

A planned modification, yet to be implemented by NEC, is installation of a liquid entrainment separator following the SF_6 vaporizer. This modification is required to reduce the time for transfer from storage.

Gas Handling System

The present status of the gas handling system may be summarized as follows:

- 17 transfers completed without major incident or SF_6 loss
- No significant leakage from joints fitted with teflon-filled spiral wound gaskets
- Typical transfer times:

Storage-to-Accelerator (50 psig)	10 hr
Accelerator (50 psig)-to-Storage	9 1/2 hr

- Approximate SF_6 loss:

1979: 500 ± 500 lb

1980: $3000 \begin{smallmatrix} +500 \\ -1500 \end{smallmatrix}$ lb

Oxford University

Ken Allen

I shall be talking on Wednesday about the status of our folded tandem so I'll use these few minutes to tell you about the way the accelerator scene is changing in Oxford and how we're putting some of our other accelerators to new uses. For some 15 years we've operated a coupled system of a large single-ended vertical machine and a horizontal EN tandem. The vertical machine had an ion source in the terminal to produce positive ions. About 1975 we secured funds to convert the vertical machine into a folded tandem and that's now operating satisfactorily. From a nuclear physics standpoint it's at least as good and in many ways better than the old combination of two machines.

We will soon take delivery of a 3-MV Tandetron from General Ionex. That machine is going to be dedicated to ^{14}C dating and will be operated by Oxford's Laboratory for Archeology and the History of Art. We're providing some technical backing for the accelerator but at least initially we're not involved in any of the experiments. That then leaves the EN tandem relatively free. We propose to convert it to an ultrasensitive mass spectrometer for experiments not involving ^{14}C. It's very fashionable these days to use old equipment where you can and we're certainly doing that here. We're thinking of a very difficult and maybe impossible experiment on nucleon decay that involves the detection of ^{129}I. To convert the EN tandem for detecting ^{129}I we will install a high-resolution injector on it. We shall use a new surface-ionization source to produce the iodine beam.

The magnetic part of that injector is an old magnetic spectrometer that has been around for about 20 years. The dispersion of this magnet is considerable. Beams of ^{127}I and ^{129}I will be separated by about an inch in the focal plane. The magnet has a nonuniform field with $n = 1/2$ and a radius of curvature of 61 cm. At a mass-energy product of 13 the resolution is 850. At high fields it can get up to a mass-energy product of about 25. The beam will go through the accelerator with the terminal voltage stabilized with reference to a generating voltmeter. There is then a charge separator and finally a time of flight detector. Although we are initially concentrating the design around ^{129}I it will be suitable for the detection of virtually any isotope.

We have at Oxford for many years had a high-energy project called ISIS and for many years I've wanted to have a project called OSIRIS. I'm sure you've heard the story of Isis and Osiris. Osiris was murdered by one of his relations and he was cut up into millions of bits and distributed throughout the earth. Then his faithful wife Isis assembled them all together again and he sprang to life, in fact to eternal life. After many tries we finally found an acronym that seemed appropriate. We have called this project the Oxford Supersensitive Injector for Radioactive Isotope Separation.

Finally I will mention one other project. A small group at our laboratory has developed a proton microprobe rather successfully. With about 4-MeV proton energy the beam dimensions are down to about a micron. That's another program that we hope will be generating interesting results over the next few years.

Laboratory Report

University of Pennsylvania

SNEAP 1980

In the past year there have been no major modifications to the F.N. accelerator. The Tandem has run reliably with only one major problem which occurred two weeks ago. There was a belt failure caused by the charging screen frame making contact with the belt.

Maximum terminal operation was 8.75 M.V. for experimental work with an average of 7.8 M.V. during the year. Our machine is equipped with titanium electrode tubes which seem to give us a voltage edge of 250 to 300 keV increase in reliable operation over the aluminum electrode tubes.

One peculiarity with this type of tube is the rather large increase in vacuum pressure during operation. The base pressure will increase from 3×10^{-7} to 6 or 7×10^{-7} Torr. This pressure increase seems to be related to the temperature of the insulating gas.

Ion source work is progressing and we are now in the process of setting up an independent ion source test bench. There are three ion source injectors on the machine. One source is the dedicated alpha Li charge exchange source. The UNIS sputter source is set up at 90° with its own inflection magnet. And the third one is a dedicated 'Widow Maker' ion source used by the Univ. of Penna. Hospital in their Positron Emission Tomagraphy program. This source is used for proton and deuteron production.

University of Pittsburgh

Tillman Saylor

There have not been any major technical improvements at Pittsburgh in the last year, but we have finally achieved good, reliable 3-stage operation with our two EN tandems. I'd like to review some of the problems we have had with the injector and what things we have done to solve them. Some of the problems had been with us for three or four years since the installation of the terminal ion source in our first tandem. They involve the infrared light link control and read out system. The basic problem was that tank sparks caused components of the electronics to fail. Last year we terminated the column with its characteristic impedence of 50 Ω to keep reflected transient waves from constructively interfering at the terminal. There seemed to be some improvement but it did not solve the problem completely. After that we had a major down time to relocate some of the source electronics since many of the components that failed were in a certain part of the structure, specifically those that controlled the motors which turn the variacs. We replaced this with a more robust relay system in a separate aluminum box. We also rerouted several of the wires.

In the ion source we installed an electrode for focusing the Cs beam onto a bar cone. This feature was not on the source as it was supplied by Extrion. After these and some other changes we finally had a period of about 2 weeks during which everything survived a couple dozen tank sparks at voltages up to 5 MV. After one minor failure we've now had stable operation for almost one month. Unfortunately, the emotional scars from this period have been such that the decision has been made to open up in a few weeks to install a complete alternate control system and readout capability. This system will use the usual mechanical devices such as rods, strings, and meters. I hope we don't goof up the whole business by doing that.

In the second tandem we have spiral titanium-electrode tubes from Dowlish. The tubes have been to 7.1 MV. We have run a fluorine beam with 7 MV on the terminal and with 10 μA out of the machine. There were six instances over the year of what we call "X-ray increase". When the tubes are well conditioned the X-ray levels are typically < 1 mR/hr at the tank wall near the tube. But occasionally, with no perceptible change in pressure and without beam, the radiation level will reach 100 times the usual level. We have a strict rule to reduce the voltage as far as is necessary and recondition even if it stops the run for a long period of time.

The last thing I would like to mention is an accel-decel experiment for which we run both terminals negative, strip the beam between the machines, and again in the terminal of the second machine. In this arrangement the second stage is an accelerating stage and the third stage is a decelerating stage. We then make, for example, fully stripped oxygen in the second terminal. These ions then emerge as slow, fully stripped ions and are used for an atomic physics research program. I would like to ask whether anybody else has the ability to do this sort of thing and what their experience is with it.

Lindgren: We have done that with sulfur. We run the injector machine MP-6 positive. We had 72-MeV S^{8+} out of MP-6 which was injected into MP-7 with a negative terminal voltage of 7.75 MV. There we strip the S^{8+} to S^{16+} and the resultant energy and particle was 10-MeV S^{16+}. We cannot go any lower than 10 MeV because the inclined fields of the tubes at the output of MP-7 were defocusing the beam too much to yield any analyzed beam below that energy.

Saylor: How far did you bring that beam before you analyzed it?

Lindgren: To the target room.

Saylor: Did someone do an experiment with that beam?

Lindgren: They were trying to but I don't believe it was finished.

Den Hartog: How do you focus the beam when it comes out of the machine?

Saylor: We don't. But for the kind of experiment we're doing we decelerate fully stripped oxygen down to 250 keV. This is for an energy of 16 MeV in the second terminal. For certain kinds of charge change atomic physics experiments the cross sections are so high that even pA beams are sufficient.

Den Hartog: Of course we do not have two tandems, but there has been some interest in perhaps using the superconducting linac as a deaccelerator.

Saylor: Do you think it could be done?

Den Hartog: I don't know. Some people think so.

Larson: I'll put in a plug for beam transport. These problems are calculable and, of course, as the beam energy decreases transport becomes very important. So you need to study what is happening.

Noé: Can you comment on vacuum activity during conditioning?

Saylor: Yes, occasionally when we are conditioning the titanium tubes we will see very tiny increases in the pressure at the ends of the machine. With a base pressure of 3×10^{-7} Torr we see a rise of 1×10^{-8} Torr.

Noé: Is it a steady rise or an abrupt rise?

Saylor: It correlates with the X-ray activity. It is not a steady rise.

Larson: With regard to X-rays, if you get pulses be sure to measure the radiation with some kind of integrating device because the amount of radiation in a pulse may be very large. It may not be properly indicated on conventional non-integrating monitors.

Saylor: We sometimes see a steady low level of X-radiation with very large flashes. The flashes are very fast and the monitor recovers quite quickly, so we have no idea how high they really are. That situation we consider good as far as conditioning is concerned.

Larson: I'm speaking about the health aspect. If your monitoring instrument responds with say 1 count/pulse, it may be misleading you as to how much radiation exists in one of those pulses. You need an ion chamber or something that integrates the total effect.

(Unidentified): There can be a lethal dose of radiation in a burst.

Larson: That's not too likely, but it is a possibility.

Saylor: Goodness. Really?

Larson: Well, a lethal dose is unlikely, but I have seen cases at Brookhaven where on conventional meters we saw a kick every few seconds and it was actually a couple of R per hour.

Saylor: Which machine was that?

Larson: This was for negative operation.

Saylor: On our negative accelerator, any time we let the beam out and sometimes even when we don't we see as much as 10 R/hr from the electrons coming out. This was the conditioning in our positive accelerator that I was talking about. There the electrons are going toward the terminal and probably most of the X-rays are of low enough energy that there is substantial attenuation.

Larson: I realize that you were talking about positive operation and I was hesitant to bring this up. But I just wanted to remind people that in an X-ray flash there can be an awful lot of radiation that some monitors will register as one count.

Saylor: Thank you for the warning.

Noë: Do you see X-rays associated with the beam?

Saylor: Yes, often we see as much as 25-30 mR/hr at the tank wall.

Berners: Do you know what the duration of an X-ray burst is?

Saylor: No, but it is shorter than the response time of the instrument.

Jones: It's fairly simple to look with an oscilloscope for the time structure.

Saylor: We have not done that.

Jones: Have you looked at the energy spectrum?

Saylor: Not since we installed the Dowlish tubes.

Goldie: Charlie Adams pointed out that these tubes have an unusual outgassing characteristic that follows the temperature of the building. Do you see anything like that in your titanium tubes from Dowlish?

Saylor: No.

ACCELERATOR LABORATORY
DEPARTMENT OF PHYSICS, QUEEN'S UNIVERSITY
KINGSTON, ONTARIO, CANADA. K7L 3N6

by

H. JANZEN
ENGINEER IN CHARGE

Because of a rather heavy experimental program and considerable time lost in machine maintenance, not much new developmental work was accomplished in the past year.

A major up-grading project that was completed was a semi-closed loop cooling water system for the analyzing magnet and other water cooled beam line components. In the past, during humid weather when using direct city mains water for cooling, excessive condensation of water occurred on the magnet windings and pole pieces, and other devices, causing rusting of the pole pieces and increasing the hazard of insulation break-down in the magnet windings or other electrodes. The system is shown schematically in Fig. 1 and supplies essentially room temperature water to the magnet at all excitation levels thereby eliminating the condensation problems. This system has now been working well for about eight months.

The other major project was an attempt to fabricate and install a fiber optic control and data link to the high voltage terminal. This met with a catastrophic failure and will be described in a later talk.

Considerable time and effort has also gone into the development of several micro-processor based control systems for sophisticated control applications. The first of these, which will function to tune the analyzing magnet numar probe to track the field to a high degree of precision, is well under way and is undergoing preliminary tests. Later the probe will be used in a feedback loop to control and stabilize the analyzing magnet field under computer program control.

SEMI - CLOSED LOOP
WATER COOLING SYSTEM
SCHEMATIC

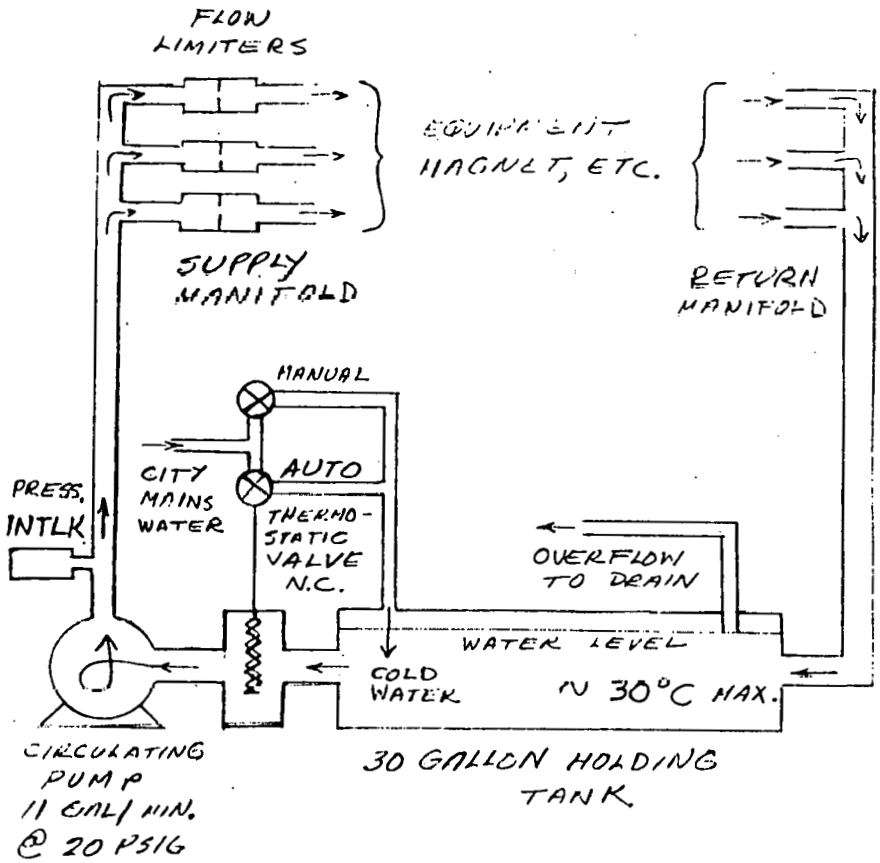


FIG. 1

Den Hartog: Are the microprocessor-based control systems you just described adaptable to other systems? Also, what type of numar do you have?

Janzen: At present we have a High Voltage numar system.that dates back to 1966. It has a servo motor for tracking that just is not very good. So we are going to replace it with a digital system with a stepping motor. Although we get a good numar signal that is easily balanced manually in the center of the scope display, trying to do this electronically is another problem.

Den Hartog: What percentage variation will you be able to achieve?

Janzen: We are aiming for about 0.1% or less.

Nuclear Structure Research Laboratory
University of Rochester

D. Kelly, T.S. Lund and T.E. Miller

A number of projects for the MP tandem and the experimental equipment at the Nuclear Structure Research Laboratory at the University of Rochester have been completed or are under construction since the last SNEAP meeting. These projects include: (1) an addition to the negative ion injector, (2) belt rollers for the charging system, (3) cryogenic plus ion pumps for the accelerator vacuum systems, (4) protection ball valves at the tank ends, (5) 40% SF_6 in the insulating gas mixture, (6) a recoil mass spectrometer, and (7) a laser spectroscopy system. The first two items are going to be covered later in the meeting and will only be mentioned here. The last two items are part of the experimental equipment of the laboratory and are projects under construction at present. It is planned that they will be available for experiments around the beginning of next year.

A new addition to the negative ion injector is under construction in order to expand the range of measurements in the ultrasensitive mass spectrometry program of the lab. The injector addition will also improve the heavy ion capability of the laboratory. Basically, the injector consists of a redesigned sputter ion source with improved sample changing capability, a high resolution inflection magnet and computer controlled power supplies.

We had a shutdown in April to install belt rollers based on the work of M. Letournel at Strasbourg. Also during shutdown, the new cryopumps and ion pumps were installed, and the protection ball valves based on the Chalk River and BNL designs were installed.

We have had some trouble with tubes 2 and 7 which are an older design of our spiral inclined field tubes. The glass insulators on these tubes do not have the reentrant profile of the newer versions of these tubes. Tube 7 was replaced after 32,000 hours of operation, and we are waiting for a replacement for tube 2 which will not hold much more than 3.0 or 3.2 MV at present. Tube #1 also gave trouble in the sense that it would spark at about 2.8 MV without any indication of conditioning or even any instability except the spark itself. We returned the tube to England to be repaired and are presently running on a back-up tube #1 which has worked well up to 11.6 MV on the terminal.

During the year we've installed shielded tube resistors on all of the tube sections, and modified several sections of column resistors to reduce the electrical stress on the column resistors. Additional SF_6 was added to the tank gas to obtain a mixture of 40% SF_6 at about 150 psia. This has been a significant improvement over the mixture of 20% at 120 psia that we've used for about 3 years.

Concerning some usual type breakdowns, the drive motor burned out during the first day of last year's SNEAP meeting. The motor had run for over 40,000 hours. We've run since August without any recirculating pump because of difficulties in obtaining replacement parts for the Roots blower. During this time, the machine ran as high as 11.6 MV without any signs of problems with the insulating gas.

In summary, it has been a busy year at Rochester with a lot of new projects being worked on, and overall the machine has been quite reliable, with the exceptions described above.

Rowton: What were the shielded resistors?

Lund: On the acceleration tubes we installed 300-MΩ Caddock* resistors between flat parallel aluminum plates. We have installed them on all eight beam tube sections. The column resistors are the stainless steel shielded resistors that we have had in for a number of years.

I have a few pictures of the equipment that I might show here. Figure 1 is a picture of the new injector addition in the corner of the accelerator room where it is set up for some initial tests. Figure 2 shows the Varian cryopumps on the end of the machine. The protective ball valves of the Chalk River and BNL design are built into the pumping tee above the pump. Figure 3 shows the difference between the two column shield versions. The original had a long tapered aluminum piece on the left which was intended to behave like a transmission line with two different characteristic impedences for some additional attenuation to protect the resistors. We had a problem with that though, with the spark gap way over on the end. One gets very high field gradients on the end of the resistor since the full voltage on the resistor appears across a relatively small distance. The epoxy chipped where the epoxy and the aluminum end of the resistor meet, so it seemed to make sense to convert it to a symmetric design with the spark gap in the middle. Figure 4 is a view looking up inside an MP terminal. The object in the corner (on the lower left) is a spring-loaded cable reel. It is a reel of 3/32-inch stainless steel cable containing a large flat coil spring.† A string fastened on the end of it can be pulled out of the terminal with a motor. By pulling it from the terminal towards the base end of the machine to a certain dead section, and then inserting a radial shorting rod, we can put voltage on only one section of the machine if we'd like. We have these in both ends of the machine. It allows us to test each tube section following a tank opening. Or we can condition the machine one section at a time.

* Model # MG785-15 resistors from Caddock Electronics Inc., 3127 Chicago Ave., Riverside, California 92507.

† The spring-loaded cable reels are available from Aero-Motive Mfg. Co., P.O. Box 2778, Kalamazoo, Michigan 49003.

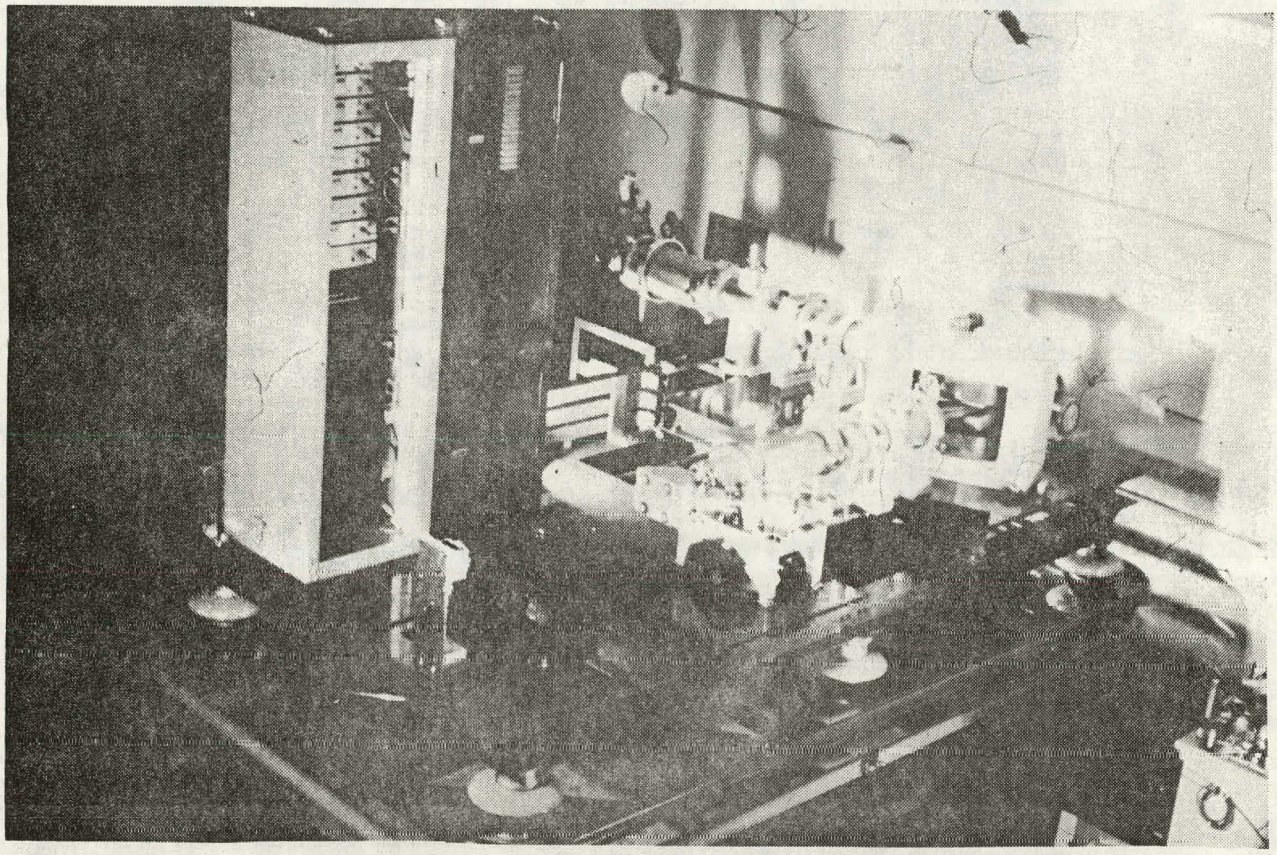


Fig. 1.

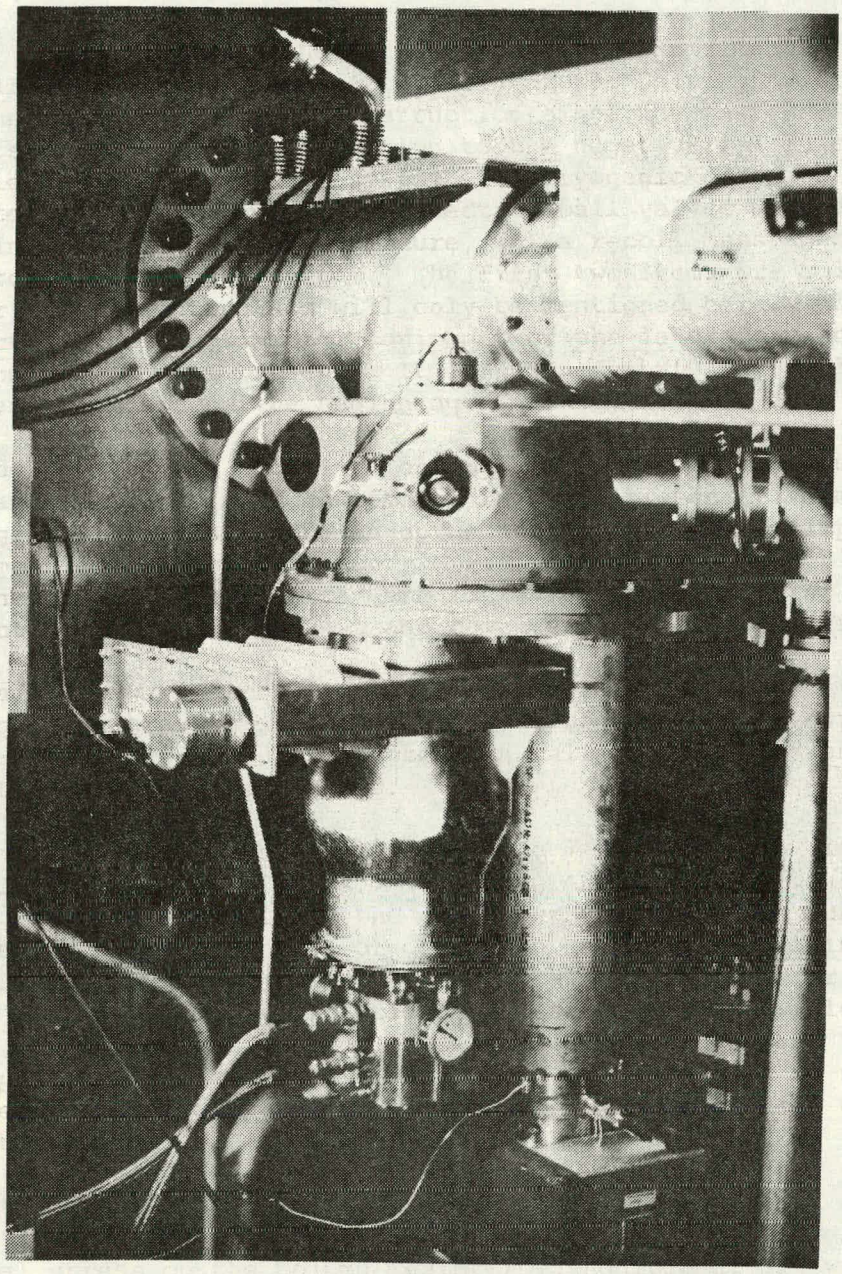
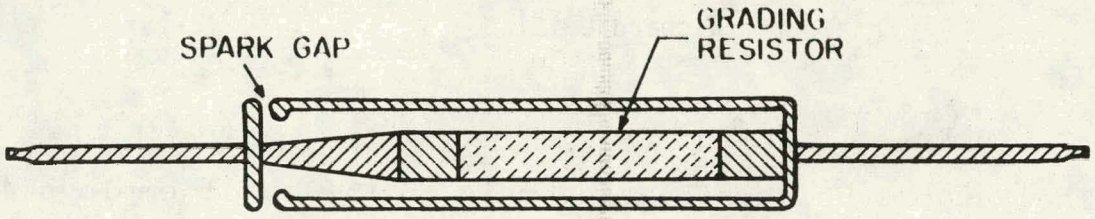
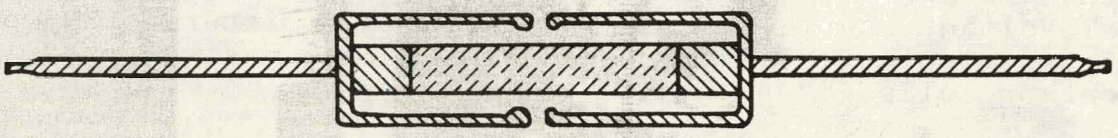


Fig. 2.



ORIGINAL SHIELD DESIGN



NEW SHIELD DESIGN

Fig. 3.

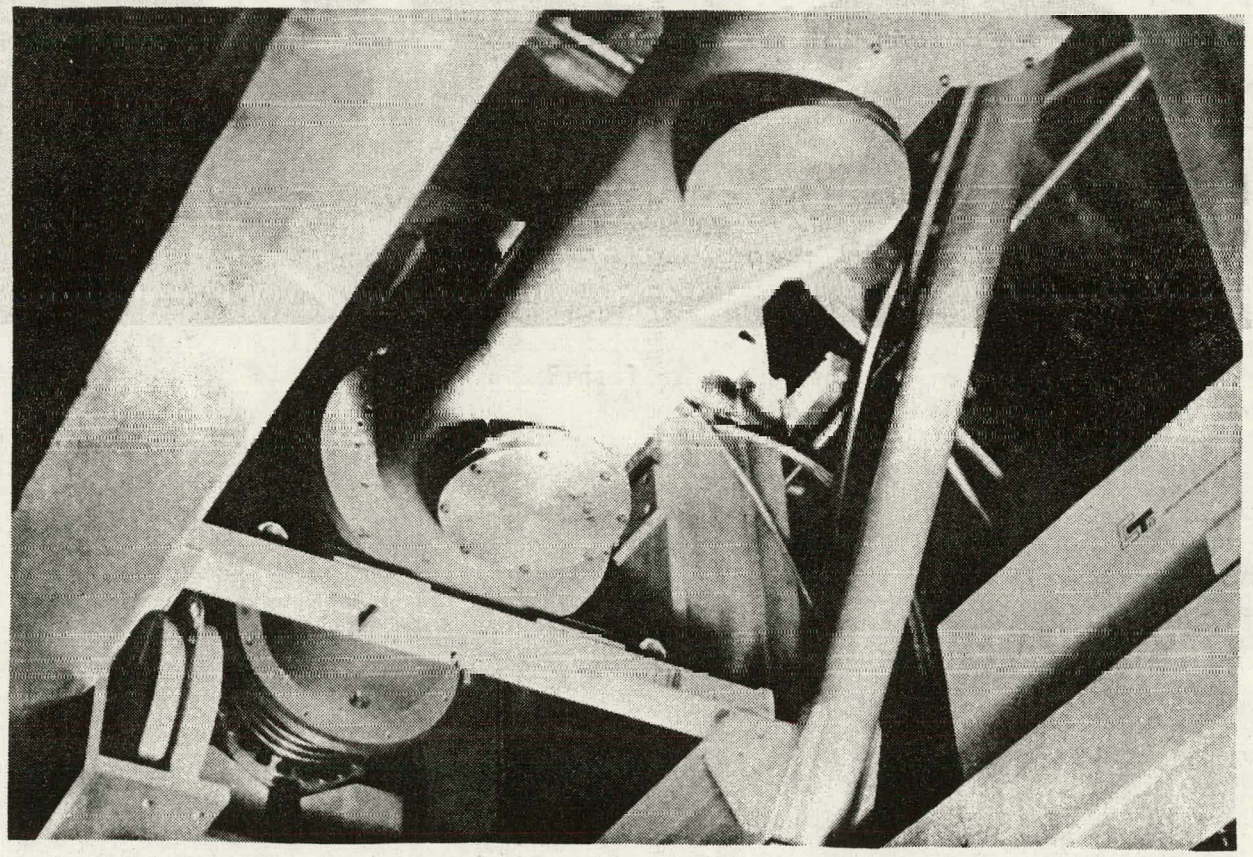


Fig. 4.

Rutgers University
Nuclear Physics Laboratory

Richard Leidich

Tandem - General

The Rutgers University tandem is an FN-1. It employs 4 ion sources including U.N.I.S., polarized source (ANAC), duoplasmatron, and a conventional helium source with lithium exchange. An inverted UNIS source is to be installed early 1981 (Chapman source).

The machine injects into 19 beam lines, one of which includes a spectrograph. Heavy ions consume approximately 90% of the machine's operating time of 160 hours per week. Maximum Terminal Voltage is 8.6 MV with insulating gas mixture of N_2 plus CO_2. Ion stripping is accomplished with both a gas stripper and solid carbon foils.

During the past year cryopumps have been installed at both the high energy station and the low energy station. Cryo-cool refrigeration units in conjunction with alcohol baths have replaced liquid nitrogen traps at the beam line pumping stations. Now we use no LN_2 on the machine vacuum systems. We do use LN_2 in the target rooms and on the polarized source.

Slackened stripper foils are currently being tested by means of our 2 MeV machine. We have also experimented with the manufacture of cracked ethylene foils. We have plans to install a stripper foil box that will contain more foils than our current unit, which contains 60 foils.

A microcomputer-controlled, 32 channel recorder will be installed in the near future as an integral part of the machine's control panel. It will use 25 channels to monitor machine parameters and has built-in alarm circuits which will be used as interlocks and/or alarms. The remaining 7 channels will be available for experimental use.

A wicking sodium canal has been designed to be used in our polarized negative ion source. Condensation of sodium at the entrance and exit of the exchange canal and on the lenses nearby has been a long-standing problem with the ANAC source. The ill effect is running time limited to 100 to 200 hours before a major maintenance effort of 3-4 man-days is required.

The current model has been thoroughly tested and appears very promising. However, it has yet to be employed during an experiment.

A new computer system has arrived and will be installed before the end of the year. This will be our on-line computer and promises to be much more versatile than our previous piece.

There are also plans underway to install a cryopump in the terminal. We currently use our gas stripper 10% of the machine time and feel that additional pumping will be beneficial.

Electronics

The Rutgers University electronics shop has undertaken the adaptation of our six major magnet supplies from Varian/Spectromagnetics/Alpha Scientific so that a nearly universal replacement can fill any slot. This effort was initiated because three of them failed within a period of three months, one of which was our analyzing magnet and which used an old Fairchild A-06 chopper amplifier. We were able to get that up by borrowing from one of our switching magnets, but we could then find no replacement for the A-06. We therefore looked at a replacement for the whole controller. When we found that they were $1500 and would be 6 weeks delivery, plus would require our old Spectromagnetics unit to enable adaptation of the new controller to the old wiring, we looked at making our own. The availability of a DC amplifier chip of low cost and very low offset voltage, and of chip references of very low drift, encouraged us. Because we would need two to replace the obsolete units eventually, we looked at making three, to provide one extra unit as a backup. Even with elegant lighted switches and a digital current meter, the total cost was under $300 each. The cost of the two main boards themselves was under $50 each.

Jones: At what point in the manufacturing process do you slacken the foil?

Leidich: First we float them onto the foil holder. Then we pull the vacuum on them immediately after we take them out and we leave the vacuum on them until they dry. We use a cylinder which has an o-ring seal that is bigger than the foil hole itself. There is an eye dropper on the other side of it. Our target maker tells me that for years he tried to get rid of the slackening in the foil. He kept getting a drop of water on top of them and he wanted to get rid of it. Now we have a little bit better control.

Burn: What do you do with the other eight hours a week if you operate for 160 hours?

Leidich: We are supposed to take maintenance every Tuesday. Lately, we have been giving up our maintenance time, however.

State University of New York, Stony Brook
Nuclear Structure Laboratory

John Noé

The past year has been an exciting and very busy one for us at Stony Brook, for in this year we have completed the major portion of our FN tandem upgrading program and have installed in final form the first elements of our superconducting LINAC booster. The emphasis has now shifted from installation of major components to performance testing of various systems and elaboration of details. The regular nuclear physics research program has just resumed at the start of October after an interruption of six months, and this program will of course have first call on machine time. A modest fraction of tandem running time will however be devoted to beam tests of the LINAC injection path and first accelerator module. The recent completion of our large 400-watt liquid helium refrigerator now makes it possible for the first time to carry out systematic operational tests of superconducting hardware.

Early in 1981 we expect to have a second somewhat shorter upgrading shutdown of about three months for installation of our new 400-kV open air ion-source table now being fabricated by General Ionex Corporation. The completed injection table will in turn be used to bring into operation a double harmonic beam buncher now being developed in-house. By the time of next year's SNEAP meeting about one-half of the twelve LINAC modules should be fully operational and beginning to come into use for experiments.

Last year at this time all of these developments were a rainbow ahead as we slogged through a mire of problems with the FN tandem. Voltage performance was limited to less than about 8.5 MV by the poor overall condition of our original HVEC aluminum tubes. The situation got somewhat worse between October and our shutdown in April, so that in early 1980 we were limited to 8.0 MV. We also suspect from our recent voltage test data that the relatively low 50-55 psig SF_6 pressure then used may have contributed to poor performance. Also troublesome in late 1979 were frequent tube-spring breakages. All these troubles ended with installation of an entire new set of springs. Evidently we had experienced the accumulated effects of years of metal fatigue and corrosion by breakdown products.

The tandem upgrading program began in January with the completion of repairs to the 300-SCFM Norwalk SF_6 compressor and an inspection and cleaning of the liquid SF_6 storage vessel. (Twenty-five pounds of rust was removed, but there was no structural damage.) Also around this time experiments with new column resistors were carried out. As will be discussed in a later session our efforts were spectacularly unsuccessful until a new mounting arrangement incorporating capacitive-bypass plates was devised.

Full time upgrading began in mid-April with the removal of our old

tubes for a 3 week program of voltage testing. The results are summarized in Fig. 1 showing the voltage distribution of sparks as a function of gas pressure. The expected linear dependence of sparking threshold on absolute pressure could be followed as high as 12 MV when the machine was in relatively good condition, but fell off to a less steep curve when there were column problems such as loose hoops. These data suggest that the basic column structure of the FN should be able to sustain 11 MV at 100 psia pressure. This testing program also provided a very effective shakedown for the new column resistors. Apart from a quite uniform drop from 800 MΩ to about 700 MΩ due to irreversible changes in the resistive material, there were no failures in the 100 prototype Caddock resistor units.

Next phase in the program was removal of the belt and installation (with the very effective assistance of Harold Hill from HVEC) of the Laddertron, the first such device in an FN. The Laddertron installation was concluded in a mid-June acceptance test in which 260 μA was delivered to the terminal at 10 MV. During these tests the terminal alternator driven by the Laddertron delivered 800 W, and the SF_6 pressure was 100 psia. Terminal ripple at a dominant frequency of about one Hertz was within ±1 kV, measured by capacitive pickoff. Especially impressive to us was the fact that long-term stability without feedback was within the same ±1 kV limits. To date this same excellent performance has continued, but it is of course premature to speculate on the long-term durability of the system.

The third and final phase of this upgrading program was the installation of the spiral inclined-field titanium electrode tubes from Dowlish Development. Installation was completed in early September, but a trivial pressure leak in an O-ring joint delayed the start of conditioning for several weeks, and the acceptance test was then carried out only in early October. Guaranteed performance of stable operation at 9.0 MV and greater than 60% (particle) transmission for ^{16}O ions was slightly exceeded with no difficulty. X-ray levels at 9 MV do not exceed about 1 mR/hour without beam. With 10 μA of ^{19}F ions at the tandem exit this level increases to 5-10 mR/hour, and there is no electron loading detectable by the current balance. An especially welcome and impressive result of converting to the Laddertron and spiral-tube geometry is the exceptional positional stability now achieved, in both horizontal and vertical planes. This feature (and of course the improved energy stability) will greatly enhance the effectiveness of the tandem as a heavy-ion injector for the LINAC.

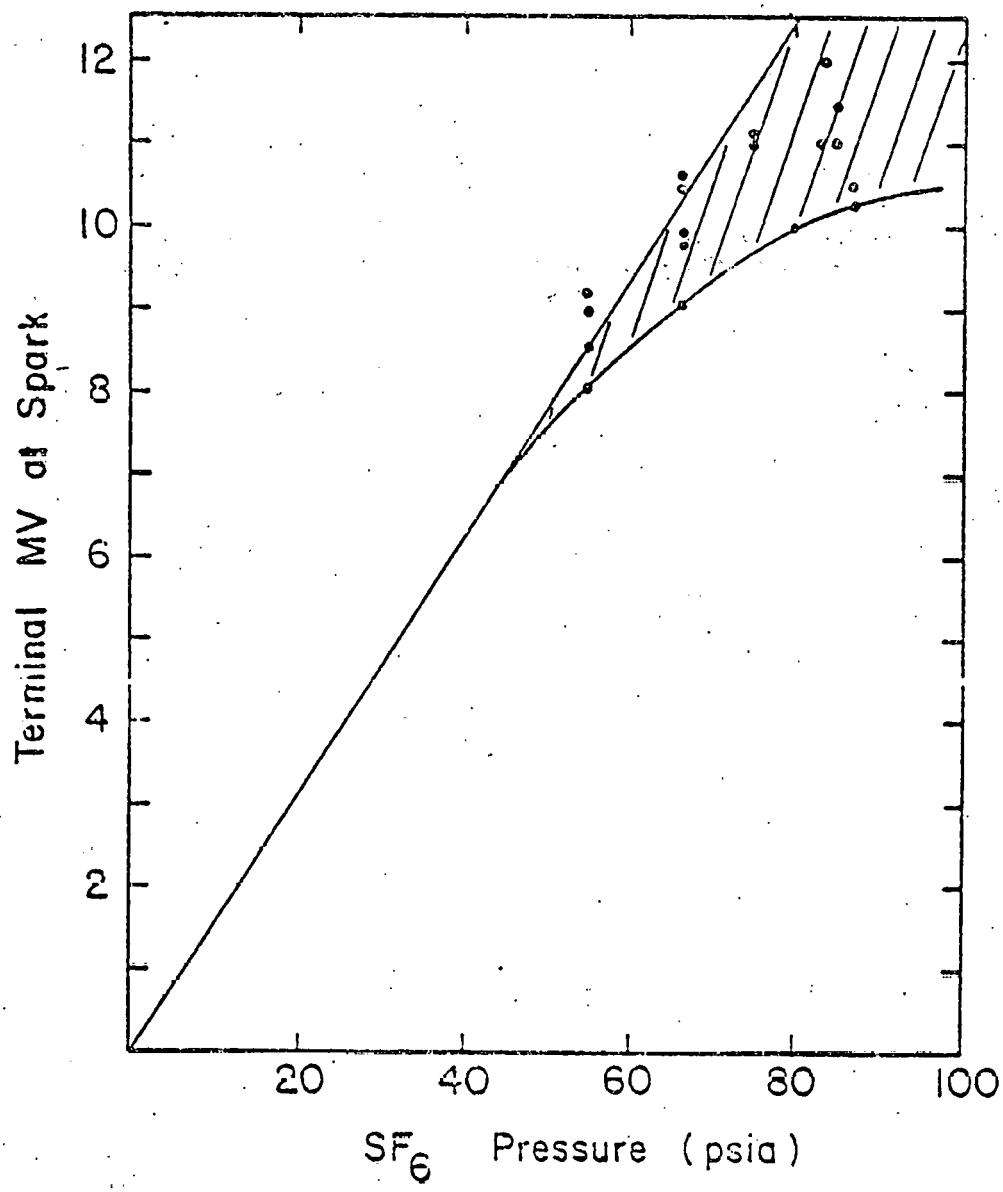


Fig. 1. Voltage performance of tandem column with belt (but without tubes).

Connolly: When you speak of a week of conditioning, does that mean continuous operation?

Noé: That was under very close control by the staff, so it was mostly daytime. It was not 24 hours a day.

Stanford University

Edward A. Dillard

During the past year our laboratory has been engaged in several projects. They include: a major modification to our polarized ion source, activation of the 3 MeV 'K' accelerator, involvement in Carbon-14 dating, extensive use of the General Ionex Corp. sputter source, and some comparisons of the carbon deposition stripper foils with those made by the glow discharge method.

The major emphasis for the laboratory has been the replacement of the ionizer section of our polarized source. This source is the atomic beam type manufactured by ANAC. This new ionizer permits profile shaping of the magnetic field which surrounds the filament and other electrodes. It is hoped that the modification will increase our beam intensity and polarization. Beam tests are scheduled for mid-October.

The 'K' model accelerator, which has been at our facility for many years, has been modified to provide dedicated service to the Electrical Engineering Department of Stanford University. The E.E. Department utilizes the accelerator for Rutherford scattering experiments and crystal lattice channeling. An all stainless steel beam-line was installed along with two sets of defining slits used to produce a 1 mm square beam with less than .03 degrees of angular resolution. Future modifications will include an improved vacuum system and microprocessor control of the operation.

Our facility is presently engaged in the measurement of time intervals between ancient earthquakes, a seemingly appropriate project considering the area in which our machine is located. The dating is being done by the Carbon-14 direct atom counting using the Tandem. It is necessary to stabilize the machine through feed-back using the generating voltmeter, which was mentioned last year. Improvements of the regulation system is planned for the future.

The General Ionex sputter source now in use was, at the last meeting of SNEAP, just installed. To date the source has proved to be very reliable and has produced many species of ions. In some cases this beam has been too intense at the normal reservoir temperature (> 100 μa) and cooling of the oven was necessary (to < 50°C) to achieve an acceptable beam for the Tandem. The shorting-out of the button heater shield to ground had been a reoccuring and annoying problem which was solved by the addition of a wire to hold the shield in place.

Since our last conference, in which glowing reports of the slackened poloyethene stripper foils were presented, our laboratory has made some comparisons; however, we still have insufficient data to reach any conclusion. We have previously done studies using standard carbon foils containing silicon. These differed markedly in behavior during ion bombardment. Experimental results were reported at the 1978 International Nuclear Target Development Society Conference.

University of Washington Nuclear Physics Laboratory

Bill Weitkamp

Our accelerator is a three-stage FN tandem built by HVEC in 1964. The machine can be run as a two stage machine with three ion sources: a direct extraction ion source, a cesium sputter source, and a polarized hydrogen ion source. When run three stage, a direct extraction ion source in the terminal of the negatively charged injector is used. We can normally run the injector terminal to about 7 MV and the tandem terminal to 9 MV.

The four major research groups in the Laboratory place relatively stringent requirements on the performance of the accelerator.

1. The fundamental symmetries group specializes in doing very high precision measurements. Their measurements of the parity violating asymmetries in nuclear interactions have a precision of about one part in 10^5. Most of these measurements use the polarized ion source. A great deal of work has been put into stabilizing nearly every property of the polarized beam including position, incident angle, intensity, and polarization magnitude and direction.
2. The gamma-ray capture group uses the polarized ion source also, and requires good beam position stability to miminize background and good energy resolution to resolve narrow resonances.
3. The heavy ion group requires as intense beams as possible of many different nuclear species.
4. The geochronology group is developing specialized hardware and techniques for determining the age of materials by measuring either ^{14}C or ^{10}Be concentrations. They require that the machine produce intense beams of carbon or beryllium, and that the machine run stably and reliably even when the beam consists of a few particles per minute.

In addition, there is a user group from Battelle studying radiation effects on materials. This group requires a beam of 10 μA of deuterons held constant to about 5% in intensity, with no interruptions for 24 hours.

Time and space do not permit a detailed description of more than a few of the projects carried out during the past year in support of these and other reseach activities in the Lab. Three of the major technical projects will be described in more detail here at S.N.E.A.P.: the improved generating voltmeter, by H. Fauska; the beam energy regulation system, by Tom Trainor; and the new tandem proposal, which I will talk about later. Four other projects will be described briefly; in each case, my colleagues who have been active on these projects are here at S.N.E.A.P., and I am sure they are willing to discuss them in more detail.

1. Polarized Ion source

It is a pleasure to talk about the polarized source, because it is the only one of these projects which is complete, works well, and causes relatively few problems. Bill Ingalls and Tom Trainor are responsible for its development and upkeep. We have a Lamb-shift source, with a spin filter to do the actual polarization, and a Wien precessor to align the spin in any desired direction. For high precision experiments, it is advantageous to

reverse the direction of the spin at a frequency higher than any of the intrinsic frequencies of the accelerator. Consequently our source has a 1 kHz fast flip system that is so simple to use that it is always used regardless of the precision required by the experimenter. For the parity violation measurements, it was also necessary to stabilize the spin direction. This is done by feeding back an error signal generated by a special four-detector beam polarimeter to a mechanism which rotates the Wien precessor. The spin direction can be held to a few hundredths of a degree over several hours with this system.

The intensity and polarization of the beam from our source are not up to the standards established recently here at Wisconsin, but it is hard to overstate the importance of this source in our experimental program. The source is used for almost 40% of our operating time. A polarization over 80% and a maximum current of 150 nA have been adequate for most experiments, including some double scattering measurements.

The souce is also used to make helium negative ions. We have a recirculator so we can produce ^{3}He economically, with up to 4 μA of beam.

2. Beam Tube Replacement

In December of 1978, after about a half a year of trouble at high voltages with the beam tubes in our accelerator we decided to replace the tubes. The old tubes lasted nearly 61,000 hours, and served us well. The symptom which led us to replace them was an electron discharge inside the beam tube which produced a very high flux of X-rays and which loaded the terminal erratically. The X-ray flux observed outside the tank near the terminal was up to 1 R, even when no beam was being accelerated. This phenomenon became known as the "plague", and initially could sometimes be conditioned away. Flooding the beam tubes with gas, up to 10^{-3} Torr, with the terminal at high voltage would sometimes "cure" the plague. We found that adding magnets to the beam tubes would also sometime suppress the phenomenon. But eventually, it prevented any operation above about 5 MV.

Because the standard HVEC aluminum inclined-field tubes had worked so well in the past (until the onset of the plague, we could run at 9 MV at will) we purchased reconditioned tubes of the same kind. Within two weeks we were back at 9 MV with the new tubes. However, after about 16 months of operation the plague has reappeared. In addition, we now observe a new disease: when running with very intense beams of deuterons we see a rapid breakdown of a substantial fraction of the column, apparently inside the beam tube. This discharge happens often enough to discourage the experimenter, and seems to depend on the position of the beam in the low energy tube. No physical damage is visible either inside or outside the tube or on the column and the resistors are in good condition. Very recently Tom Trainor developed a technique using the new generating voltmeter to locate the source of the discharge, but as of now we have not applied the technique as quantitatively as we would like.

Clearly, this project is not complete. We are particularly interested in learning about experiences with beam tube diseases in other labs.

3. Heavy Ion Acceleration.

We have had difficulty accelerating heavy ions beams with sufficient intensity to satisfy a number of the experimenters. A multi-pronged attack has been launched over the past several years to try to improve both source output and accelerator transmission. Derek Storm has directed much of the work. The major emphasis has been on the acceleration of carbon beams from the sputter source, partly because of the needs of the geochronology group. We started with a maximum particle transmission of about 15%. Below are some of the prongs of our attack.

A. We have worked over our Extrion sputter source to improve reliability and intensity. The cesium boiler, ionization region , and insulator shielding have been reworked. A reflected sputtering geometry using six holes has been developed. The source now runs for about 600 hours without dismantling for maintenance and produces somewhat more beam. We have, however, not floated the sputter source at high voltage. The importance of this step is a matter of heated controversy in the Lab.

B. After studies of the low energy optics, we determined that we might improve the transmission by adding a gridded lens at the entrance to the beam tube. This has been done. The gridded lens removes most of the dependence of the low energy optics on terminal voltage, and hopefully serves as an inexpensive alternative to floating the sputter source.

C. At the same time, we installed a new 3 in. aperture electrostatic quadrupole triplet at the entrance to the accelerator. This replaced a smaller diameter quadrupole doublet lens which might have been intercepting the beam. Both the gridded lens and the triplet together increased the transmission by about 50%.

D. We have attempted to improve the vacuum in the beam tube. We have added an ion pump in the beam tube near the ion source, reducing pressure by an order of magnitude. We have added a cryopump at the tandem high energy end.

E. We have built a completely new sputter ion source, using the Bocum sputtering geometry. At comparable cesium temperatures, this source has produced many times more beam than our older source. We are presently measuring the emittance of this source and the Extrion source to compare potential transmissions. The source still needs more development to improve reliability before it can be installed on the accelerator.

After all these changes, we have seen particle transmissions of 50% for carbon, improved, but still lower than the 95% transmission we routinely get with the direct extraction ion source. The transmission is also still disturbingly low for heavier ions, e.g., a few percent for Ni.

4. Momentum Filter/Spectrograph

The fourth major project is the construction of a new magnetic momentum filter/spectrograph. This instrument, which will rest on a Navy 5 in. gun mount, is a sequence of 6 magnets, quadrupole-4 dipoles-quadrupole, streching over a flight path of about 6 meters. The design sacrifices high resolution for large solid angle, for an isochronous flight path for particles of a particular momentum, (all path lengths through the instrument are of equal length to 1%), for the ability to take a large spread in particle energy, and for an achromatic focus at the end of the instrument. design is complete and the magnets are on order. Derek Storm, who is supervising this project, has more information for those who are interested.

UNIVERSITY OF WISCONSIN

Tandem Operations.

The nuclear physics group at Wisconsin operates a Pelletron-charged EN tandem accelerator. Pure SF_6 has been the insulating gas since the installation of the Pelletron in 1971. As of October 1, 1980 the machine had logged a total of 119,581 hours of operation, 3698 hours since October 1, 1979. This relatively low utilization of the tandem (42%) compared to previous years was not related to any major problems with the accelerator. Several breakdowns on our 14-year-old DDP-124 computer caused excessive lost time because repair parts are no longer available. In addition, we spent 15 days installing the new negative-ion beam line. The pressure vessel was opened only 8 times during the past year for various reasons. Carbon stripper foils were replaced three times and minor maintenance of the charging system was required during two of the tank openings.

During October of 1979 we experienced some problems with excessive radiation outside the pressure vessel on the low-energy side of the terminal. The problem at first was intermittent and difficult to diagnose, but we eventually traced it to a single beam tube section about two-thirds of the way up the low-energy end. In order to diagnose such problems we had some years ago installed on both columns an apparatus for shorting individual column sections one at a time. The arrangement consists of a stainless steel ball attached to a dial cord that runs along the bottom of the column rings between a pulley in the terminal and one at the base of the tank. By using selsyn motors one can position the ball to short any column section while watching for a change in the radiation level. After we permanently shorted the offending tube section the problem disappeared.

Upgrade of the Low Energy Beam Line.

The beam line from the ion sources to the tandem was completely rebuilt. In addition to relocating the direct extraction and He^- ion sources, a new rotating mirror, electrostatic quadrupole triplet, and einzel lens were installed. The design and position of lenses which optimize transmission through the tandem were based on calculations of beam transport obtained with J.D. Larson's computer code OPTIC. The quadrupole triplet allows displacement of the horizontal and vertical waists of the beam and improves matching of the beam to the ion optical properties of our inclined field tubes. The calculations predict that with the new lens system beam transmission will not decrease as rapidly with decreasing terminal voltage as it had previously. For terminal voltages of 3.5-5 MV we have measured 85-90% transmission of proton beams from the direct extraction duoplasmatron. We have obtained 35% transmission of a deuteron beam at 1.4 MV, a remarkable improvement over previous results. Transmission of polarized beams has improved from about 50% with the original lens configuration to 80% with the new system.

Colliding-Beam Source.

The Wisconsin colliding-beam negative polarized-ion source is now in routine operation. The source has on occasion produced beam currents in excess of 3 μA for both H^- and D^-. For prolonged experiments approximately 1 μA beams have been available. The main components of the source (except for the atomic-beam apparatus) are shown in Fig. 1. A 20-40-keV Cs^+ beam from a porous tungsten ionizer is neutralized in cesium vapor. The Cs^o beam (of up to 3 particle- mA) collides in the solenoid with thermal polarized hydrogen atoms. The negative ions produced are extracted by a 0.5V/cm electric field and accelerated to 14 keV at the entrance of the double-focussing spherical-plate inflector. They are then accelerated to ground, focussed, and deflected through 90° by an electrostatic mirror. A Wien precessor determines the final spin direction.

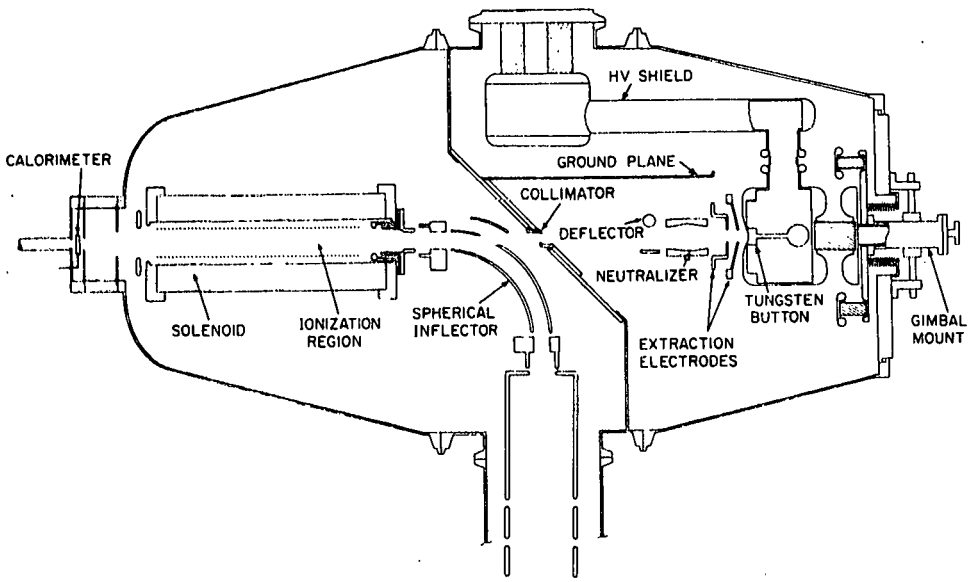


Figure 1. The Cs^o and ionization portions of the Wisconsin colliding-beam polarized-ion source.

Beam polarization for both protons and deuterons have been measured using calibrated polarimeters. For magnetic field of 1000 gauss in the ionizer the proton vector polarization was $P_z = 0.91 \pm 0.01$. This value is 96% of the maximum polarization possible due to the remaining coupling between nuclear and electron spin at that magnetic field. The polarization $P_z = 0.63 \pm 0.01$ for deuterons is also greater than 95% of the maximum possible for ionizer fields of either 400 or 600 gauss.

Negative Ion Source Development.

Both versions of SNICS (Source of Negative Ions by Cesium Sputtering) have been used routinely both by the Nuclear Engineering radiation damage studies and by Nuclear Physics students. Our major effort has however been on improvements for the He^- source. The RF shielding has been completely redesigned and rebuilt to confine the RF to the immediate vicinity of the quartz bottle. As a result we can now use thermocouples to sense and control the temperatures of the rubidium oven and the exchange canal region and we should see improved performance. The better RF shielding also permitted reliable emittance measurements for the first time. On the test bench we've seen as high as 14.5 μA of He^- and find that 80% of a 4 μA beam has an emittance better than 5.5π mm mrad $MeV^{\frac{1}{2}}$. Sputtering from the extraction canal has been the main factor limiting operating lifetime of the He^- source to 100-200 hours. Recently we have made measurements with a graphite canal and observe sputtering rates about a factor 8 less with no difference in beam current or emittance. However the sputtered graphite is very hard to remove from the quartz bottle. It seems to be in a form which does not react with the usual chemical reagents for removing carbon.

Positive Ion Accelerator.

The Wisconsin Nuclear Engineering Department has recently installed a Model AN-700 electrostatic generator for use by their radiation damage group. This single-ended machine will provide 700-keV beams of H^+ and He^+ ions for preinjecting metal specimens with hydrogen or helium gas atoms before subsequent heavy ion irradiation at the EN tandem laboratory. The machine has only recently passed acceptance tests and produces beams of 100 μA of H^+ and He^+.

Electron Accelerator Operations.

The radiation damage group also operates a Model 2UEH electron accelerator. This machine has seen little use in recent years because of a lack of personnel. Recently the 2-MeV electron beam has been employed to produce order-disorder transitions in ordered alloys.

Yale University
A.W. Wright Nuclear Structure Laboratory

K. Sato

1. Present Gas Mixture (October, 1980).

(SF_6') = 12 % (< 1 yr.)
(SF_6'') = 20 % (2-3 yrs.)
} $SF_6' = SF_x$, $x \leq 5$ 32 % (Depleted SF_6).

N_2 = 57 %, CO_2 = 11 %, H_2O = 5 - 25 ppm.

We have been experiencing a difficulty to maintain the moisture content at the optimum value when it is recirculating continuously.

2. SF_6 Consumption Rate (How to minimize SF_6 consumption rate).

2-1. Present Estimate of SF_6 Consumption Rate.

HWY Driving Conditon (Minimum) = 1 1A size cylinder SF_6 / week.

City Driving and High Speed Driving Condition (Maximum) = 1 1A size cylinder SF_6 / day.

Average Condition = 2 1A size cylinder SF_6 / week.

2-2. F^- Production Cross Section out of SF_6.

Fig. 16 shows the variation of the F^- negative ion current out of SF_6 with electron energy. At the lowest energy (0.6 eV) F^- current appears to be rising rapidly with decreasing energy. The current falls to a minimum value at 2.6 eV and is folloed by a series of three well-defined current onsets leading to maxima. At any point F^- current does not fall to zero, as contrasted with the SF_6^- and SF_5^- currents, and this special feature of F^- current might be a major contribution to the "critical gradient discharge".

2-3. Average Electron Energy in the Tank Gas Mixture (Effect of Vibrational Cross Section on Electron Distribution Function).

The large size of the vibrational cross sections in N_2 exerts a strong influence on the electron energy distribution function in nitrogen discharges by acting as a "barrier" for the gain in energy. Electrons thus experience difficulty in reaching energies in excess of about 2.5 eV as long as the vibrational temperature is low.

The high energy tail also increases when E/N (the ratio of the electric field to the gas density) is raised, but the sharp cutoff is still present up to an E/N of 1.5×10^{-15} V cm^2, which corresponds to an average electron energy of 2.25 eV. The value of E/N of 3×10^{-16} V cm^2 corresponds to an average electron energy of 0.72 eV.(1)

$\bar{\epsilon}$ = 2.25 eV, E/N = 1.5×10^{-15} V cm^2, F/P_{N_2} = 50 V cm^{-1} torr^{-1}, P_{N_2} = 60 psia, $V_T MP$ = 15.7MV,

$\bar{\epsilon}$ = 0.72 oV, E/N = 3×10^{-16} V cm^2, F/P_{N_2} = 10 V cm^{-1} torr^{-1}, P_{N_2} = 60 psia, $V_T MP$ = 3.1 MV.

(1). Nighan, W. L., 1970, Phys. Rev., A 2, 1989 (1970).

2-4. How to Minimize SF_6 Consumption Rate.

We should try to maintain F/P within a range 20 V cm^{-1} $torr^{-1}$ ≤ F/P ≤ 50 V cm^{-1} $torr^{-1}$ and keep the average electron energy around the minimum F^- production electron energy i.e. around 2.6 eV by changing the tank gas pressure according to the required V_T.

It may be better to use the SF_6 mixture for V_T over 10 MV, and use the N_2/CO_2 mixture for V_T below 10 MV, even though it requires two storage spaces for two different gas mixtures.

3. Optimum N_2/CO_2 Ratio.

The most efficient laser system designed to date, namely, the N_2-CO_2 laser and the CO laser, rely on vibrational transitions for laser action. In the N_2-CO_2 system the large vibrational cross section, near 2.3 eV in N_2, resulting from a resonance, causes the electron distribution function to be sharply cut off. This results in an efficient population of vibrational levels of N_2, with subsequent transfer to the upper laser state of CO_2 (i.e., the asymmetric stretch mode).

Since N_2 has a zero permanent dipole moment, the nitrogen molecules excited to vibrational levels of the ground electronic state cannot decay to the v = 0 vibrational level through electric-dipole radiation. Thus the effective lifetimes of these states are governed by deactivation through cilliciona with other molecules and walls. Very strong quenching of N_2*(v=1) by CO_2 has been reported.

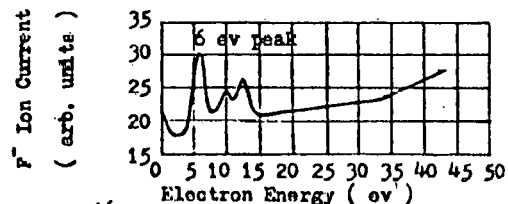


FIG. 16. F^- ion current as a function of electron energy, observed by Ahearn and Hannay (1953). Note that ordinates do not start at zero.

References: Hickam, W. M. and Fox, R. E., J. Chem. Phys. 25 (1956) 642.
Ahearn, A. J. and Hannay, N. B., J. Chem. Phys. 21 (1953) 119.

	Electron Energy at Peak	Max. Cross-sections
$SF_6 + e = (SF_6^-)^* = SF_6 + e$		
$= SF_6^-$	0.03 ev.	10^{-15} cm^2.
$= SF_5^- + F$	0.43 ev.	10^{-16} cm^2.
$F + e = F^-$		
$= SF_5 + F^-$	(1) 0.6 ev.	
$= SF_4 + F + F^-$	(2) 6 ev.	10^{-18} cm^2.

Fig. 1 shows that N_2*(v=1) at 2330.7 cm^{-1} is in very close coincidence with the 001 vibrational level of CO_2 at 2349.16 cm^{-1}. The energy discrepancy is~10 cm^{-1} (to be compared with average thermal energy, kT, of the molecules, which at room temperature is about 210 cm^{-1}). Thus a collision of the second kind which may be written as

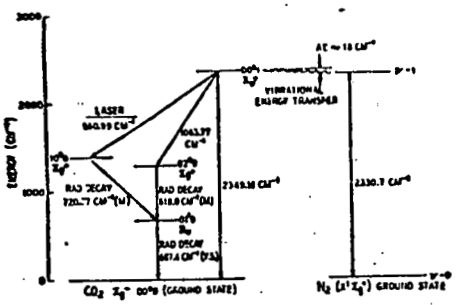


FIG. 1. Energy-level diagram showing pertinent vibrational levels of CO_2 and N_2. Rotational levels for the vibrational levels have not been shown for simplicity.

$$N_2^*(v=1) + CO_2(000) \rightarrow N_2(v=0) + CO_2(001) - 18\ cm^{-1} \quad \text{----- (1)}$$

can have a large cross section because of the near-perfect coincidence.

Fig. 2 shows the excitation efficiency for various laser mixtures in discharge excited CO_2 lasers and suggests the N_2/CO_2 ratio could be increased to 7 instead of our generally accepted value around 4.

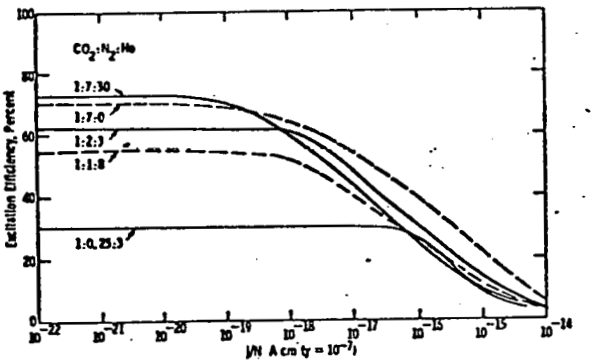


Fig. 2. Excitation efficiency for various laser mixtures in discharge excited CO_2 lasers.

We have found a chance to measure an optimum value of N_2/CO_2 in our tank gas mixture using no follow-up discharge as a criterion. Our result was;

Optimum $N_2/CO_2 \leq 5$ in our tank gas mixture.

4. Optimum H_2O Content.

Among several H_2O contributions to our tank gas mixture we have had an opportunity to determine an optimum H_2O content for its function to deactivate CO_2* molecules. CO_2* (its mutual relaxation time = 10^{-6} sec.) is deactivated by H_2O via a Leonard-Jones (12,6) interaction and a coupling between the permanent dipole moment of H_2O and the induced dipole moment of CO_2.

Observed optimum H_2O content = 115 ppm for deexcitation of vibrationally excited CO_2 molecules and completion of $N_2^* \rightarrow CO_2^* \rightarrow H_2O^*$ deactivation channel.

5. Future Gas Projects.

5-1. SF_5^-/SF_6^- Ratio.

Through a collaboration with the Atomic Physics Group of the Engineering and Applied

Science Department of Yale University we are planning to compare the SF_5^-/SF_6^- ratio of our tank gas mixture to the SF_5^-/SF_6^- ratio of pure SF_6 utilizing their monochromatic low energy electron source and the negative ion measuring facility, and confirm our estimates of the SF_6 consumption rate.

5-2. SF_6^- Stabilization by Addition of H_2O.

Through the above mentioned collaboration we are planning to study the effect of H_2O molecules to stabilize the SF_6^- ions and to prevent them to go to the dissociative attachment decay channel. We expect a final relaxation time in our gas mixture ($SF_6/N_2/CO_2/H_2O$) in an order of 10^{-8}sec., as opposed to the 10 us lifetime of $(SF_6^-)^*$ and a few hours lifetime of excited state in N_2^*. This 10^{-8}sec. deactivation time is directly compatible with the lifetime of the $(SF_6^-)^*$ predissociation state leading to ($SF_5 + F^-$) and this directly inhibits negative "F^-" production.

5-3. How to Maintain H_2O Content under the Continuous Recirculation.

We have been struggling to find the best way to maintain the H_2O content in our tank gas mixture around 115 ppm with the continuous recirculation through our gas filtering system, but we have failed to do so and our present H_2O content is 5 - 25 ppm.

6. Corona Control Signal Transit Time (Fig. 3).

We have measured the signal transit time between the corona point to the H.V. terminal at various V_Ts and the tank gas pressures and plotted as an exponential function of F/P_{N_2} (V cm^{-1} $torr^{-1}$) in Fig. 3.

$$T \text{ (ms)} = 119.86\, e^{-0.05(F/P_{N_2})} \quad (F/P_{N_2} \text{ in V cm}^{-1} \text{ torr}^{-1}) \quad \text{----- (2)}$$

(For a gas mixture = 32 % SF_6 + 57 % N_2 + 11 % CO_2 + 5 - 25 ppm H_2O).

7. Perfect Beam Condition (Near 100 % Transmission).

Since we started using the UNIS (sputtering) source we have been struggling to improve its emittance to match our up-graded MP's accelerator tube acceptance and to improve the heavy ion beam transmission.

Finally we have succeeded to reach the stage which we may be able to call it as a perfect beam condition.

For V_T = 5 MV - 10 MV, Estimated Beam Transmission = 70 % - 100 %,

Tank Press. - 135 psia - 105 psia,

Kind of Negative Ion so far = O^- out of S_1O_2,

N_1^- out of N_1 metal cone,

Injected Negative Ion Current = 10 NA - 2 μA.

The contributed factors to this perfect beam condition might be as follow:

Enhanced reflected beam mode with a 0.5 kV biased extraction electrode,

Double Einzel Lens System,

Higher preacceleration voltage over 200 keV (Better vacuum by the Interface Pumping Station),

Better running vacuum: L.E. = 1 x 10^{-8}torr, H.E. = 1.5 x 10^{-8}torr with an optimum stripper gas: H.E. Vac. Rise - 0.2 - 0.3 x 10^{-8}torr.

In addition we have stopped to observe any pressure sensitive leak and out-gassing from fresh Epoxy glue which we used for fixing the pressure sensitive leaks.

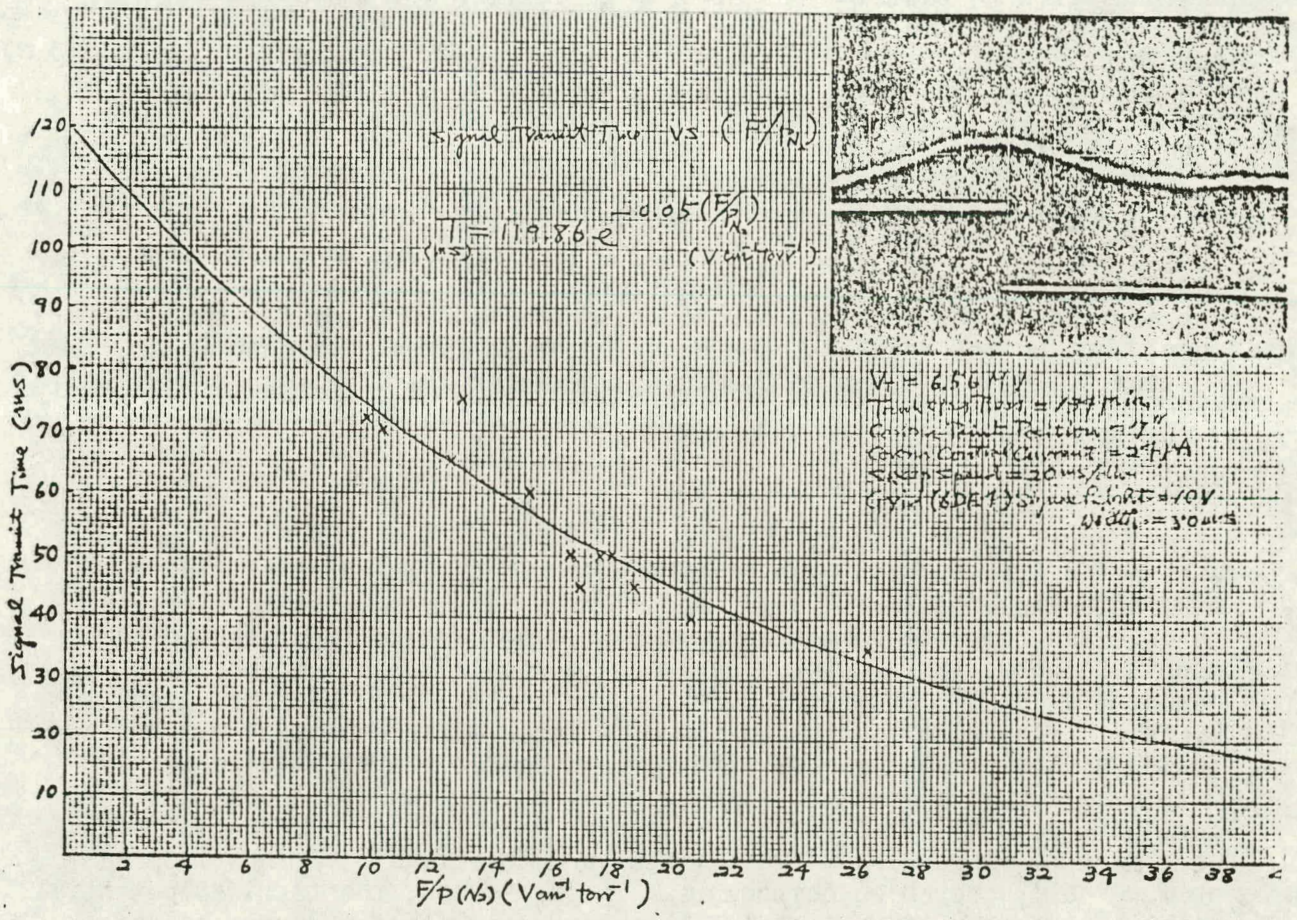


Fig. 3

STATUS OF THE OAK RIDGE 25 MV TANDEM ACCELERATOR*

N. F. Ziegler, E. G. Richardson, J. E. Mann, R. C. Juras,
C. M. Jones, J. A. Biggerstaff, and J. A. Benjamin

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Introduction

The 25 MV tandem in Oak Ridge is a 25URC Pelletron constructed by National Electrostatics Corporation. Installation of major components was essentially completed during the latter part of 1979. During 1980, component installation, modification, calibration, and testing continued. Presently, the accelerator is being conditioned for 25 MV beam tests.

1980 Operation

Operation of the accelerator during the past year can best be indicated by the bar graph in Fig. 1 showing pressure in the Pelletron tank as a function of time. Voltage, roughly proportional to pressure, was applied to the terminal during the periods of pressurization, but it should not be inferred that the accelerator operated continuously at voltage during these periods. During the intervals at atmospheric pressure, column equipment was installed, modified or repaired.

Goals Achieved

Several noteworthy goals were achieved during the year, the major ones being acceleration of beam through the machine and completion of beam acceptance tests at 7.5 MV and 17 MV. Beam was first accelerated through the complete beam transport system on May 12, 1980. Following conditioning of the accelerating tubes to about 17 MV, a 60 pnA (particle nanoamperes) $^{16}O^{6+}$ beam at 108 MeV was obtained. This beam was produced by foil stripping a 750 pnA OH^- beam injected at an energy of 0.3 MeV. Tuning of the machine was straightforward and it operated quite stably with beam; in fact, this initial test was accomplished without the terminal potential stabilizer. Following this initial success, efforts were made to improve beam transmission and to activate various inoperable, or marginally operable, components. In particular, the terminal potential stabilizer and NMR fluxmeter for the energy analyzing magnet received attention since these devices were required for calibration of the magnet.

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*Research sponsored by the Division of Basic Energy Sciences, U.S. Department of Energy under contract W-7405-eng-26 with the Union Carbide Corporation.

On June 25, the energy analyzing magnet was calibrated using an $^{16}O^{2+}$ beam incident on a carbon foil. The energy of α particles at approximately 180° from the $^{12}C(^{16}O, \alpha)^{24}Mg$ reaction was measured in a surface barrier detector which could be calibrated with an ^{241}Am source. This absolute energy calibration indicated an incident beam energy of 27.443 ± .034 MeV. The calculated terminal voltage was 9.236 MV while the GVM indicated 8.8 MV. Following the absolute calibration the magnet field was adjusted to select ^{16}O charge states 3, 4 and 6 while maintaining the terminal voltage constant. From this measurement the mass-energy product of the magnet was determined as a function of field using the equations $ME/q^2 = kB^2$ and $E = V_i + (q + 1)V_T$. Results are shown below in Table I.

TABLE I

q	V_T(MV)	B(kg)	E(MeV)	$\frac{ME}{q^2}$(MeV)	$k = \frac{ME}{q^2B^2}$
2	9.236 (calc.)	9.034	27.443	109.873	1.3463
3	↓	6.989	36.667	65.284	1.3365
4		5.864	45.913	45.984	1.3373
6		4.619	64.385	28.677	1.3441

A summary of accelerator beam acceptance tests is presented in Table II. The first four tests, at 7.5 and 17 MV, were completed in

TABLE II

Test #	Term. Pot.(MV)	Ion Mass(1)	Analyzed Beam(pμA)	Term. Strpr.	Max. Xmiss. Factor(2)	Duration (min.)
3.3	7.5	M	.01	foil	n.s.(3)	10
3.8	"	↓	1.0	gas	12.5	60
3.4	17		.01	foil	n.s.	10
3.9	"		1.0	gas	12.5	60
3.2	25	L,M or H	.001	n.s.	12.5	60
3.6	↓	L	.01	foil	n.s.	10
3.5		M	↓	↓	↓	↓
3.7		H				
3.11		L	1.0	gas	12.5	120
3.10		M	1.0	↓	12.5	60
3.12		H	0.5		15	60
3.1	27.5	n.s.	.001	n.s.	n.s.	20

(1) L: $12 \leq$ mass ≤ 50, M: $100 \leq$ mass ≤ 130, H: mass ≥ 195

(2) Ratio of inj. beam (pμA) to analyzed beam (pμA)

(3) Not specified

July and August. For tests 3.8 and 3.9 the measured transmission factor was about 10. I^{10+}, I^{4+}, I^{13+} and I^{6+} beams were used to perform tests 3.3, 3.8, 3.4 and 3.9, respectively. Also during this interval further data, using ^{127}I beams, for calibration of the energy analyzing magnet were obtained and the results are listed in Table III.

TABLE III

q	V_T(MV)	B(kg)	E(MeV)	ME/q^2(MeV)	k
10	7.573	8.875	83.51	106.02	1.346
6	↓	11.807	53.30	187.94	1.348
5		13.100	45.73	232.18	1.353
4		14.919	38.16	302.71	1.36

On July 29 an attempt was made to inject an I^{5+} beam (45 MeV) into ORIC (Oak Ridge Isochronous Cyclotron). A beam of about 60 pnA was deflected into the ORIC beam line, but the test was inconclusive because an obstruction prevented viewing the quartz inside the cyclotron.

Problems Encountered

During the year there were 13 tank openings, twelve of which were necessitated by failures. A bar graph of problems is presented in Fig. 2. It is obvious that the number of failures exceeds twelve, but there were occasions when more than one problem was found during a tank opening. In particular, water leaks were usually accompanied by one or more vacuum leaks. This coincidence indicates that water may cause corrosion of the aluminum gaskets, but the evidence is not conclusive. All of the water leaks were caused by failure of tubing connectors on Faraday cups or variable apertures. One tank opening was necessitated by a broken coupling on one of the rotating shafts. During another tank opening, a noisy shaft bearing was replaced. Failure of electrostatic quadrupoles occurred twice because of marginal electrical contact between cables and feedthrough connectors. During one tank opening the stripper gas (N_2) cylinder was replaced, inadvertently, with a cylinder having a high content of helium. A second tank opening was then required to replace the contaminated cylinder. The other failures listed in Fig. 2 are, more or less, self-explanatory. Column electronic equipment has proven quite reliable. Only one tank opening could be partially attributed to failure of the control system.

One serious problem occurred this year in the SF_6 storage system. In July a leak was discovered in the manway port of storage tank #1. Fortunately, the tank contained little, or no, liquid since the accelerator tank was pressurized. The standard procedure for filling the Pelletron tank results in tank #1 being emptied of liquid. It was thus deemed an opportune time to repair the leak, and the vapor in the tank was transferred to the Pelletron. Removal of the manway cover revealed corrosion in one of the O-ring grooves in the cover and a considerable amount of rust in the tank itself. A "bathtub ring" was also evident in

the tank at an elevation corresponding roughly to the top of the liquid level. A photograph of this ring is shown in Fig. 3. It is assumed that the ring is a result of water floating on top of the SF_6 liquid. The tank was cleaned by wire-brushing and vacuuming. The manway cover was machined to clean the O-ring groove and the port was resealed. The tank was then evacuated and refilled with SF_6 vapor with no evidence of leakage.

Modifications

A significant modification to the accelerator in August of this year was the replacement of the enclosed corona grading system with an open point system. NEC proposed the change following poor experience with the enclosed system on the NEC 20 MV tandem in Japan. At present, the replacement is considered a test. One disadvantage of the open system, of course, is the restricted range of adjustment of terminal voltage; however, the shorting rod could be used for gross adjustment with small variations being provided by charging and corona probe adjustments. The final decision on the type of corona grading system will follow the 25 MV acceptance tests.

Several minor modifications were made in the gas handling system for the accelerator. The by-pass valve around the compressors was moved to a higher elevation near the water-cooled condensers. This change, in conjunction with a slight modification in operating procedure, seems to have eliminated the problem of liquid SF_6 expansion which produced very low temperatures in the by-pass piping. The by-pass valve is required to match the compressor pumping speed to the vacuum pumps during a transfer to storage. A block valve around the vacuum pumps was modified to serve as a flow-controlling by-pass which has simplified the transfer from compressor-only pumping to vacuum pump-compressor pumping. A small scavenging compressor has been installed which pumps any leakage from the main compressor's shaft seals into an accumulator tank. One modification still remaining is the installation of a liquid separator following the SF_6 vaporizer. This change will involve some heavy pipe work and has not yet been scheduled. The modification is required to reduce the time for transfer from storage.

The present status of the gas handling system may be summarized as follows:

- 17 transfers completed without major incident or SF_6 loss
- No significant leakage from joints fitted with teflon-filled spiral wound gaskets
- Typical transfer times:

Storage-to-Accelerator (50 psig)	10 hr
Accelerator (50 psig)-to-Storage	9 1/2 hr

- Approximate SF_6 loss:

  1979: 500 ± 500 lb

  1980: $3000 \begin{smallmatrix} +500 \\ -1500 \end{smallmatrix}$ lb

- significant residual problems:

  1) through-leakage of dryer valves after thermal cycling
  2) liquid transport through vaporizer

Summary

The present schedule for completion of the 25 MV tandem is shown on the flow diagram in Fig. 4. As stated previously, the tandem is now being conditioned for 25 MV acceptance tests.

A design study of the feasibility of increasing the energy gain of ORIC is in progress at ORNL. In essence, the boost would be achieved by increasing the magnetic field by a factor of 1.73, thus increasing K from 100 to 300. The increased field would be achieved by installing superconducting main field coils. A comparison of the energy capabilities of the proposed cyclotron and tandem with existing machines is shown in Fig. 5.

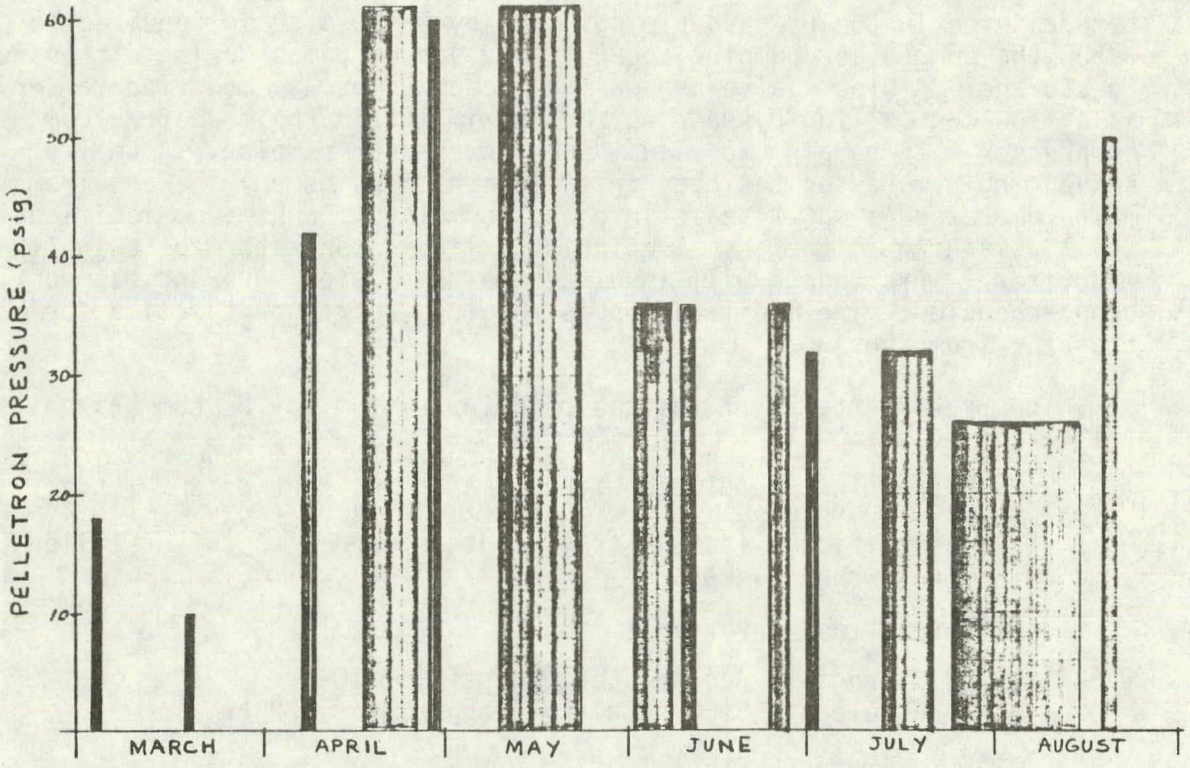


FIG. 1- PELLETRON PRESSURIZATION in 1980

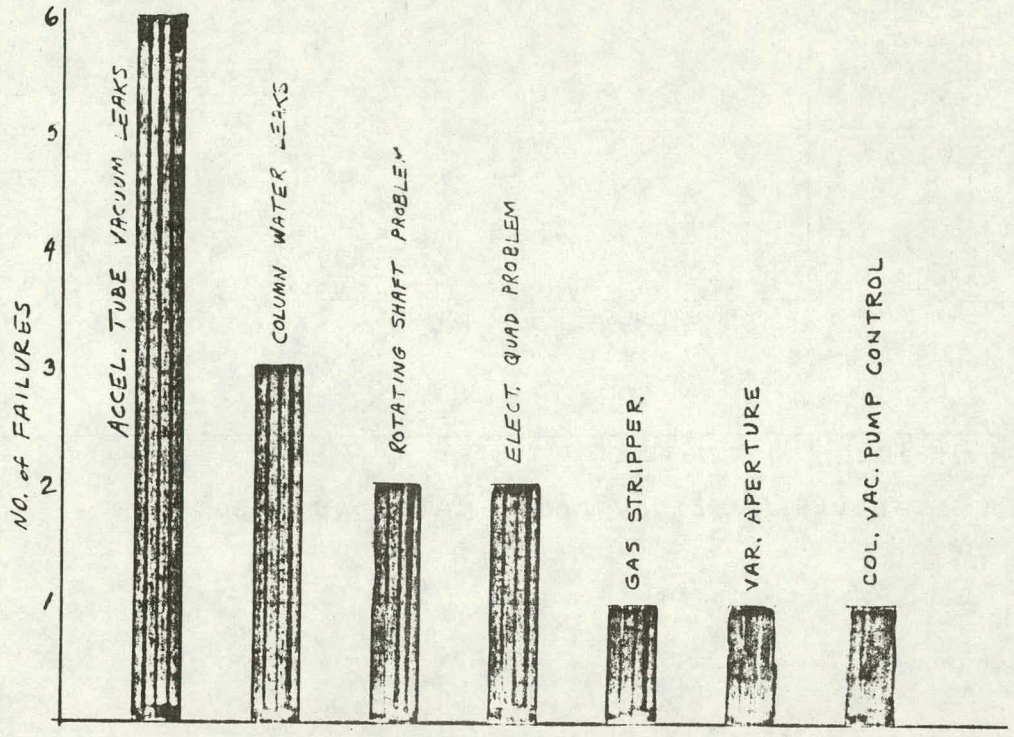


FIG. 2- PELLETRON PROBLEMS in 1980

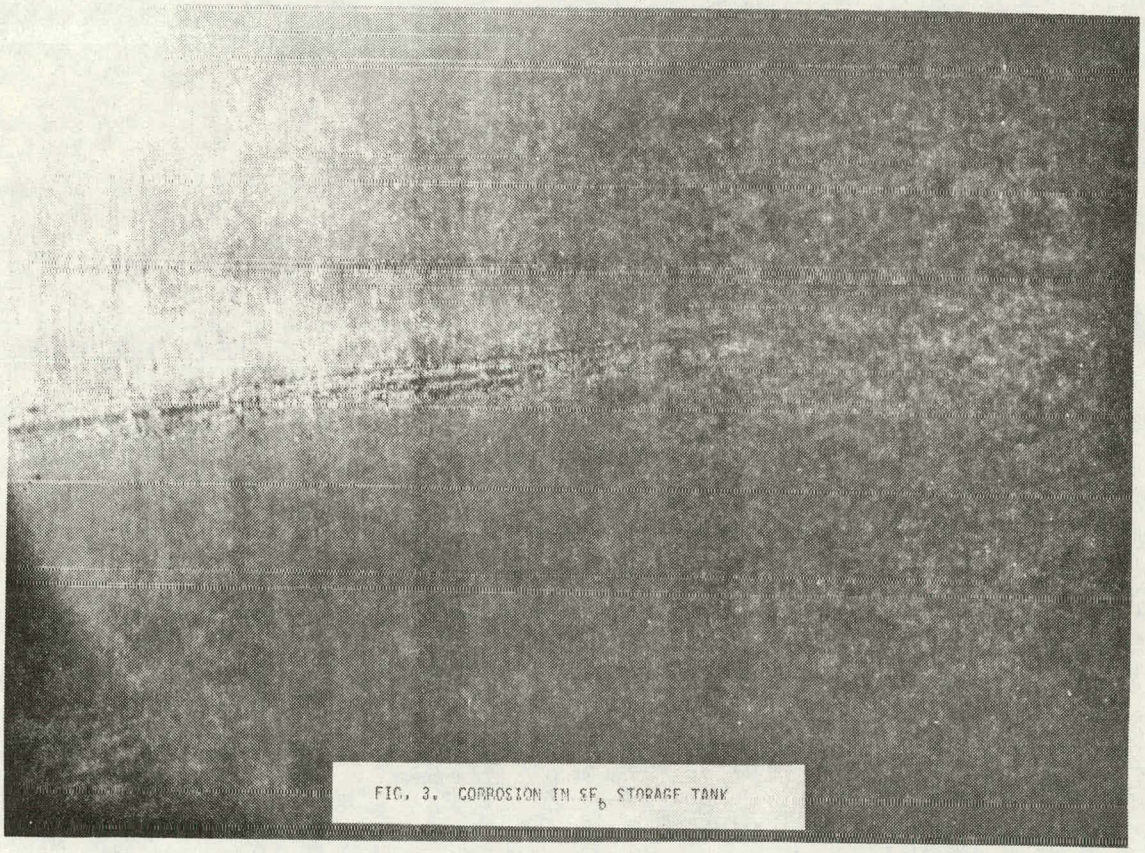


FIG. 3. CORROSION IN SF_6 STORAGE TANK

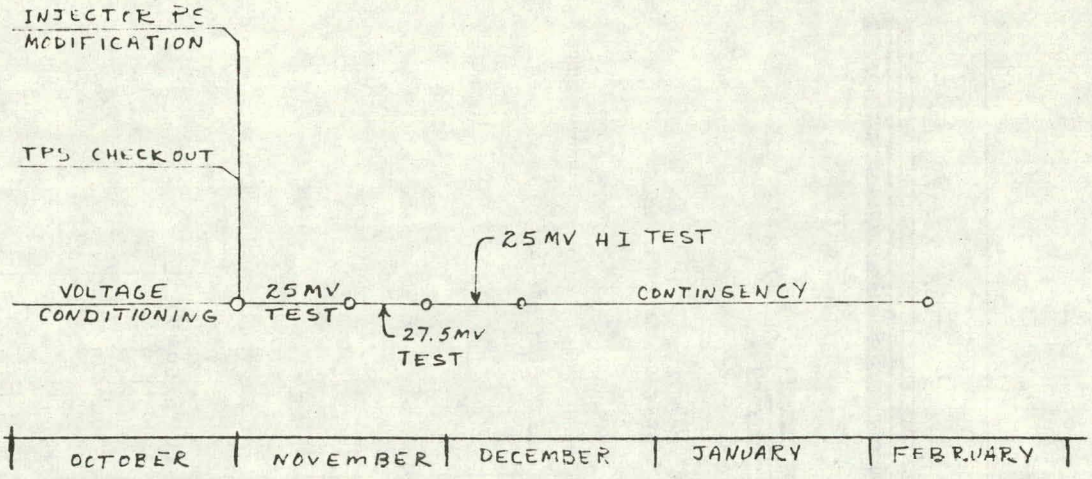


FIG. 4 - SCHEDULE for 25 MV ACCELERATOR (9-15-80)

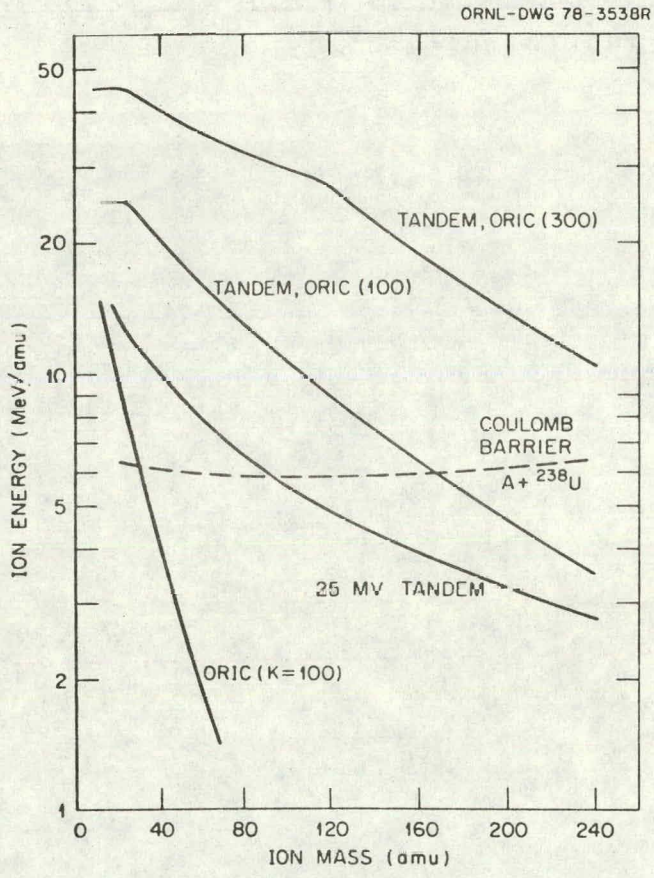


FIG. 5. ACCELERATOR SYSTEMS PERFORMANCE FUNCTIONS

Den Hartog: Where did all that water come from? Was it from a water leak?

N. Ziegler: Yes, I think so.

Den Hartog: What is the influence of the open corona needles on your operating capabilities?

N. Ziegler: It will, of course, restrict the range over which we can vary the voltage.

Den Hartog: Do you have any idea what the range will be?

Jones: About 10%. In the best case with shorting rods fixed in position it might be possible to vary the voltage ±10%.

Noé: Were the vacuum leaks in the connections or in the tubes and other components?

N. Ziegler: There have been leaks in the aluminum gaskets in the tube itself. There have also been leaks in flanges for Faraday cups and variable apertures.

Noé: Another question. What is the motivation for going to an open corona system?

N. Ziegler: In Japan on the NEC machine the closed corona system has been damaged by sparks. So NEC wants to try the open system on this machine. Is that right, Robert?

Rathmell: I'll mention that in my talk.

Goldie: Does that open corona system provide a separate system for the column and for the beam tube?

N. Ziegler: Yes.

Goldie: Are both open?

N. Ziegler: Yes.

Rathmell: Norv, do you know what a 5% error in the GVM reading does to the original column voltage estimate up to 32 MV?

N. Ziegler: I don't think we could say definitely.

Rathmell: You're not able to extrapolate?

N. Ziegler: Not yet.

Allen: Do you know whether the beams follow the correct trajectory around the 180° magnet?

N. Ziegler: I think it pretty well has to. The slits are in the magnet.

Allen: The slits are tight, are they?

Jones: The slits are not very tight. We modeled the ion optics of the machine very carefully and we find that the lens settings which are found empirically agree with the model calculations with remarkable accuracy. That implies that the beam is doing what we think it is doing.

Allen: We have some trouble with the fringe field not being exactly what we thought it should be in our system. So the trajectory around the 180° magnet was not quite right and that led to relatively poor transmission.

Jones: The fields for all of our magnets were measured beforehand, and they were more or less what they should be.

Rathmell: The beam comes around with very little steering in the terminal, and since the beam tubes are six feet apart its just about got to go through the magnet correctly to get from one tube into the other.

Larson: I'll disagree with that last comment. It doesn't have to go through the magnet the way you intend it to, but the fact that it must pass through the slits at 90° means that it is pretty close. Of course, it does depend on how tightly the slits are set. But the fact that the beam passes through at all without the slits having to be wide open suggests that the beam is close to the desired trajectory and that the magnetic field is nearly uniform.

Rathmell: As a matter of fact, the slits were wide open at plus and minus one-quarter inch.

Walker: Where are the slits for the 180° magnet?

Larson: The slits are at 90°. That is, they are in the center of the magnet at the top of the beam's trajectory.

A Proposal for a New Tandem at the University of Washington

Presented by William G. Weitkamp
University of Washington Nuclear Physics Laboratory

It is not clear how this paper got in a session labelled "Accelerators in Progress" because we have no money yet. There are some indications that money may be on the way, but in the meantime you should realize that I'm just sharing our dreams with you. With that in mind, I want to briefly describe various parts of our proposal. I will start with the rationale, say a few words about accelerator design, about the building, and finish with a little about the cost and the construction schedule.

Over the past decade or so there has been a deterioration in the support available for large nuclear physics facilities at universities. Instead, there has been a strong trend toward centralization of facilities at a few national laboratories. Of all the new tandems described in this session today, none (except ours) are at universities, and in fact only one is in the U.S. So, when we sat down to write this proposal, the first question we had to answer was whether we should buck this trend and continue to maintain an in-house facility for nuclear physics research or not.

The answer to this question was clearly yes, for a number of reasons. The first was that there are many experiments that are difficult to do as users at a centralized facility. I have listed several classes of such experiments:
1) Experiments that require a modification of the accelerator. Users normally cannot command the kind of attention of the technical staff that is required to change the accelerator for a specific experiment.
2) Experiments that require a complicated setup. One has to haul trailers around the country, and that is not good for the electronics.
3) Experiments that need to be done quickly. Getting proposed experiments through a program advisory committee and getting accelerator time scheduled can result in a given experiment becoming obsolete before it is ever done.
4) Experiments that are a little bit crazy. Program advisory committees tend to stick to things they know will work.
We forsee that many of the experiments we want to do in the 80's and 90's will fall into one of these categories. It is consequently important for us to try to maintain an in-house facility to do those experiments.

A second reason for maintaining an in-house facility is that we are an educational institution. To continue to produce graduates, we must attract new students to our program. It is difficult to attract students to a user program. For one thing, user research interferes with classwork. Students have to run off to some other lab every few months; it is hard to maintain good grades with all that travelling. For another thing, if a student is going to be a user at a big facility, why not go to Fermilab and do it right?

A second question we had to answer in preparing our proposal was why this machine should be built at the University of Washington instead of some other university. I want to make it clear that we hope that this is not the only nuclear facility to be built at a university in the near future, but rather that it represents a first step in the general rejuvenation of physics facilities at universities. But I would be less than candid if I didn't tell you that we think we have a very strong program at the University of Washington. I want to mention four areas where we feel we are particularly strong.

1) We have a very vigorous research program, including experiments of fundamental significance.
2) We have a good technical capability.
3) We are active in applied nuclear physics research; for example, we have a program in fast neutron cancer therapy research.
4) We have been able to graduate some very good students. For example, the leaders of the two largest electrostatic accelerator projects in the world are both graduates of our lab.

A third question we had to answer in preparing this proposal was what kind of accelerator we should propose. Some of the criteria we used in making our selection follow:

1) The accelerator must be flexible enough to be useful in a wide variety of experiments, including both light and heavy ion physics.
2) The accelerator should offer some unique characteristics not available elsewhere in the U.S.
3) The accelerator should be a qualitative improvement over our existing facility.
4) The accelerator should be relatively inexpensive to operate. This is particularly true for electric power costs, even though they are still low in the Pacific Northwest.
5) The accelerator should be on a technical scale that can be effectively managed by our laboratory.

After holding a number of possibilities up to these criteria, we concluded that we should propose the largest tandem electrostatic accelerator that would stand a reasonable chance of being funded. The cost scales roughly as the cube of the terminal voltage, and after testing the water a bit with a 20 MV machine, we proposed an 18 MV machine. In order to have a facility that has truly unique characteristics, we intend to couple a new high-current polarized ion source to the accelerator. The 36 MeV direct-current polarized proton and deuteron beams will in fact be unique in the U.S.

I will not spend a lot of time describing the machine that we have proposed because it is very similar to the machine NEC is building at Oak Ridge, which you have just heard about, and the JAERI machine, which you will hear about in a few minutes. The maximum terminal voltage is somewhat lower, however. Fig. 1 shows how we intend to arrange our machine. It will be conventional except that we will have two negative ion injectors, one holding a polarized ion source, the other holding either a direct extraction ion source, a sputter source, or an alpha source.

There is one special feature we have to include in our machine. I guess everyone knows there is a certain amount of seismic activity in Washington State. So this machine has to be able to withstand earthquakes. However NEC has an earthquake protection kit, and one of their machines has withstood a severe earthquake at Tsukuba, so this does not seem to be a problem.

Next, I want to describe the building required for this machine and how we intend to work it into our existing laboratory. Fig. 2 shows the laboratory as it presently exists. Fig. 3 shows how the tower containing the new tandem will be attached to the existing building. Fig. 4 shows a cross section through the tower and the existing building. I want to point out a couple of features of the layout in Fig. 3. First, there are only three tunnels connecting the tower to the existing building. Furthermore, the new accelerator can be completed without disturbing the existing accelerator. Consequently, we anticipate there will be at most a three months interruption of research operations and that only because workmen will have to go into the accelerator vault to set the forms for the tunnels.

Note in Fig. 3 that the beam from the new accelerator will be injected into existing beam lines so we will use all of our equipment. We will use our 90 degree magnet to convert the area labeled "New Cave Area" into a new experimental area. In the future we hope to install a new switching magnet so we can add additional beam lines.

In the future we can also hook up the existing tandem as an injector to the new machine through the third tunnel.

One of the major problems with building an accelerator on a university campus is that you have to worry about the architecture. A team of distinguished architects came to campus to review this proposal, and their recommendation was to treat the tower as a simple vertical cylinder and clad it with some kind of a shiny metallic material. As you might expect, we are somewhat concerned that it might end up looking like a big beer can.

To finish, I want to say a little about costs and the construction schedule. I will quote costs with some trepidation. Most of you are familiar with the cost of accelerators in the 1960's. The cost of accelerators in the 80's will come as a shock. Furthermore, costs are time dependent because inflation continues at a rate of something like 10% per year. Our estimate of accelerator costs are given in Table 1, in 1980 dollars, and without any contingency allowance. The building, which we have requested the State of Washington to fund, will cost 3.5 M$ in these terms.

U. W. 18 MV Tandem Cost Estimate
(In thousands of 1980 dollars, no contingency)

A.	Accelerator Column	5,280
B.	Pressure Vessel	1,350
C.	Low Energy Transport System	880
D.	High Energy Transport System	240
E.	Gas Handling System	520
F.	Gas Inventory	360
G.	Engineering	240
	Total	8,880

As we presently envision the schedule for construction of this project, funding should begin in Fiscal Year 1982, which means we can begin final design work in October 1981. The entire project should take four years so that first research operation with the new accelerator should begin in October, 1985.

Now, where do we stand with this project? The Nuclear Science Advisory Committee of the Department of Energy has put this project in the funding queue. At the present time we don't know whether that means we get any money because the D.O.E. has not yet released the FY 1982 budget. We are hoping that they will see fit to fund this project.

Larson: I have a comment. I think the price of accelerators is coming down in very round numbers. The Oak Ridge accelerator and building and associated equipment was about 50% more dollars than the Brookhaven double MP facility which had a substantially larger building included in the project. Yet, in the decade between those two accelerators, inflation must have been roughly a factor of 2, especially if one includes the fact that the Oak Ridge accelerator was priced in a period of very high inflation. The project cost over a 3- or 4-year period went up much more rapidly than did the cost of the Brookhaven project. Since those two projects were comparable in price, perhaps the Oak Ridge accelerator is actually cheaper than the Brookhaven installation.

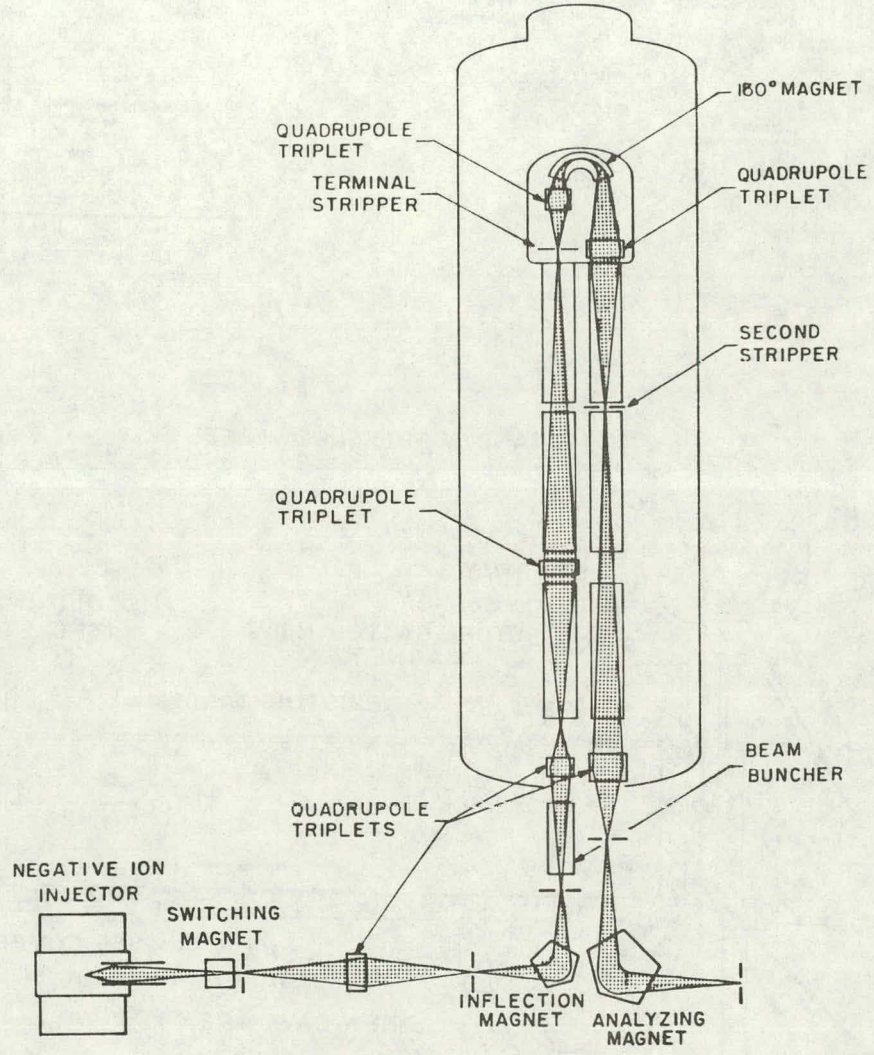
Allen: I suppose you must have considered the merits of this completely new machine against removing one of the tandems and putting in some kind of postacceleration booster.

Weitkamp: Yes. We even submitted a proposal two years ago for a room-temperature linac booster. That had several disadvantages though, one of which was that it was quite expensive. Another disadvantage was that, although it was a very innovative design, it represented a lot of engineering. This would have required us to add substantially to our staff.

Also, it was basically a heavy ion machine. We would have been able to accelerate polarized protons and deuterons, but nobody was very happy with the beam currents that we would be likely to get.

Chapman: We have similar beam currents to yours, and I would suggest that there is one other type of experiment that can be done at a university-run facility. And that is one that takes a very long time and is very difficult to make.

Weitkamp: Yes, I really meant to include that in my "complicated" category.



18MV TANDEM BEAM OPTICS

Fig. 1.

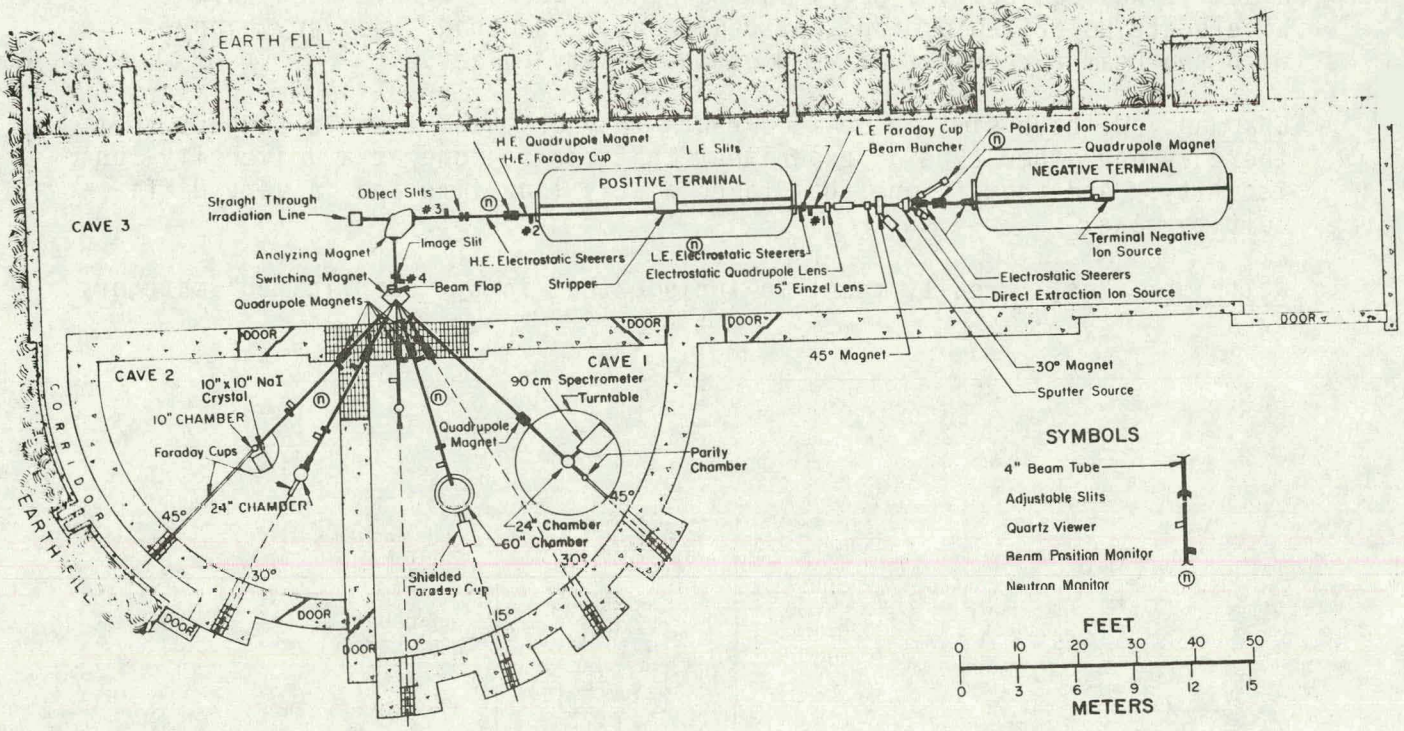

EARTH FILL
H.E. Quadrupole Magnet
H.E. Faraday Cup
L.E. Slits
L.E. Faraday Cup
Beam Buncher
Polarized Ion Source
Quadrupole Magnet
POSITIVE TERMINAL
NEGATIVE TERMINAL
Straight Through Irradiation Line
Object Slits
#3
#2
#1
CAVE 3
Analyzing Magnet
Image Slit
H.E. Electrostatic Steerers
L.E. Electrostatic Steerers
Terminal Negative Ion Source
Switching Magnet
#4
Beam Flap
Stripper
Electrostatic Quadrupole Lens
Electrostatic Steerers
Quadrupole Magnets
5" Einzel Lens
Direct Extraction Ion Source
DOOR
CAVE 2
CAVE 1
10" x 10" NaI Crystal
90 cm Spectrometer
Turntable
45° Magnet
30° Magnet
Sputter Source
10" CHAMBER
Quadrupole Magnet
Parity Chamber
Faraday Cups
24" CHAMBER
24" Chamber
60" Chamber
45°
30°
15°
10°
Shielded Faraday Cup
CORRIDOR
EARTH FILL
SYMBOLS
4" Beam Tube
Adjustable Slits
Quartz Viewer
Beam Position Monitor
Neutron Monitor
FEET
0 10 20 30 40 50
0 3 6 9 12 15
METERS

Fig. 2.

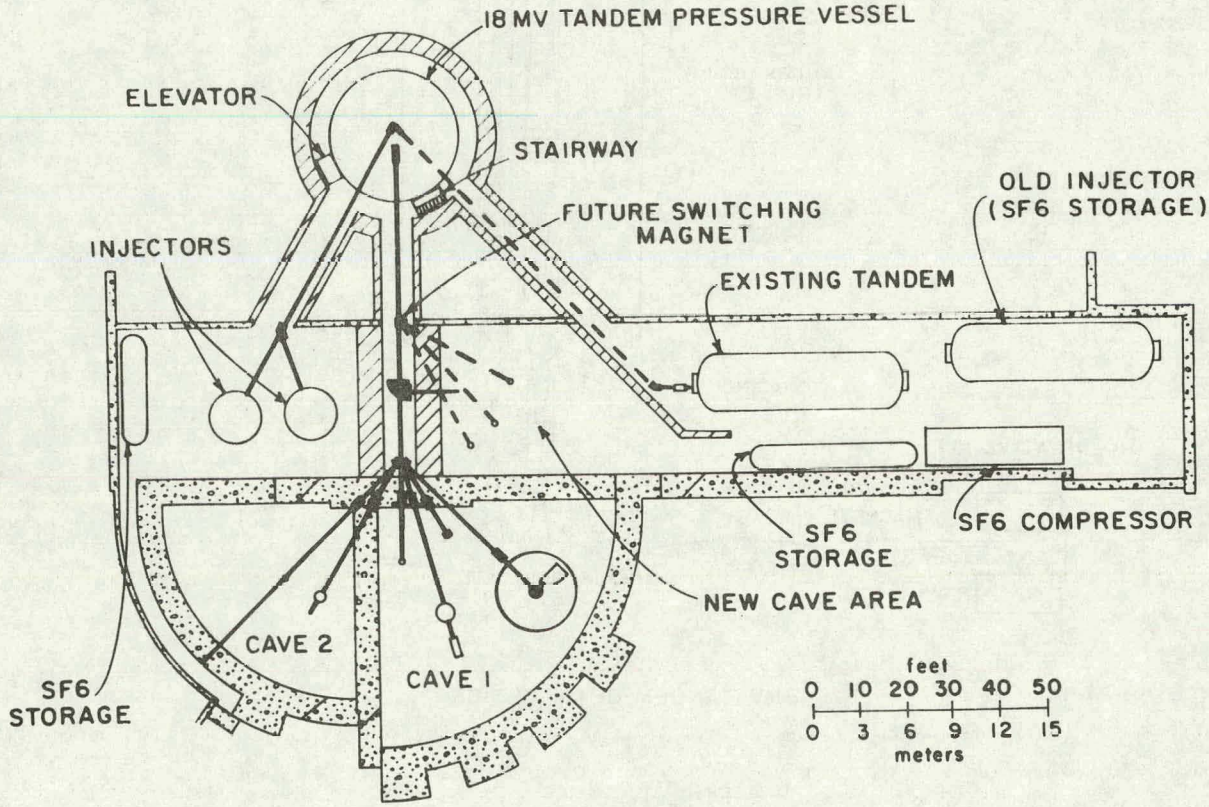

18 MV TANDEM PRESSURE VESSEL
ELEVATOR
STAIRWAY
OLD INJECTOR (SF6 STORAGE)
FUTURE SWITCHING MAGNET
INJECTORS
EXISTING TANDEM
SF6 COMPRESSOR
SF6 STORAGE
NEW CAVE AREA
CAVE 2
CAVE 1
SF6 STORAGE
feet
0 10 20 30 40 50
0 3 6 9 12 15
meters

Fig. 3.

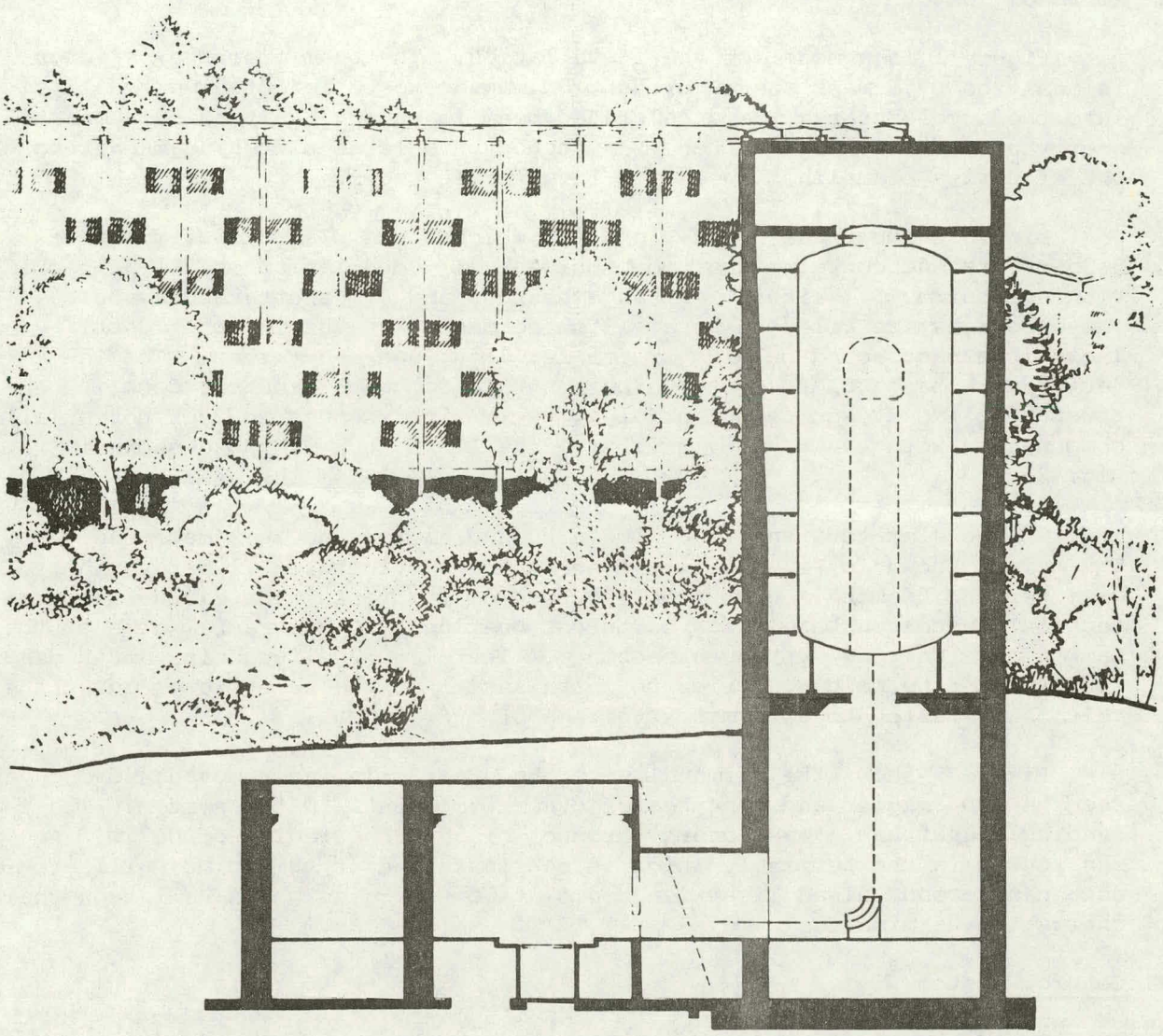


Fig. 4.

STATUS OF THE 20UR AT JAERI

R.D. Rathmell, R.E. Daniel, and M.L. Sundquist
National Electrostatics Corporation

Introduction

The 20UR is a 20-MV folded tandem manufactured by NEC for the Japan Atomic Energy Research Institute in Tokai, Japan, about 150 kilometers north of Tokyo.

Figure 1 is a model of the installation. The beam from the injector is analyzed by a mass energy product 80 magnet to be injected vertically into the accelerator. The accelerated beam is energy analyzed by a mass energy product 400 magnet then is switched by a large switching magnet to one of twelve beam lines in one of five target rooms.

Figure 2 shows the 300-kV injector which has a platform diameter of 3.35 meters and contains four ion sources: 1) duoplasmatron, 2) Heinecke Penning source, 3) lithium exchange source, and 4) sputter-cone source. These sources are selected by a switching magnet on the injector deck. There is a nanosecond pulsing system for light and heavy ions. The injector deck has a CAMAC crate which controls components on the deck. The power supplies at source potential (~ 50 kV with respect to deck potential) communicate with the crate via fiber light links such as those shown in Fig. 3.

The accelerator tank is 8.2 meters in diameter and 26.5 meters high. The column shown in Fig. 4, is a free-standing structure 2.1 meters in diameter and 20 meters high from the base of the tank. For earthquake protection the column base rests on thrust bearings and has radial arms to the tank with a damping system on each arm. There are 20 live units and 2 dead sections in the column. Prior to installation of the accelerator tubes the column was tested to a terminal voltage of over 23 MV.

Figure 5 shows the terminal with the upper spinning removed and one can see the 180° magnet and a number of other components in the system. The bending magnet has a mass energy product of 55 and a radius of 0.7 m. An ion source in the terminal, which is not installed at this time, will produce nanosecond pulsed light ion beams at low energies. Six Pelletron chains charge the terminal.

Control System

The control consoles, the accelerator and the beam lines are interfaced to an Interdata 7/32 minicomputer via CAMAC and a DMA interface. This system forms a communication link from the operator to the accelerator but does no closed loop control except for one case which will not be discussed at this time. There are 17 CAMAC crates in the system including two in the main console and one in the source console. A total of 760 data points must be updated twice per second and console controls consist of a smaller set of data points updated 25 times per second.

CAMAC crates in the terminal and in the dead sections of the column also communicate with the serial highway back to the computer via digital light links shown in Fig. 6. These light links transmit infrared light at 2.5 megabits per second radially through windows on the tank. All of the electronics inside the tank are doubly shielded and an interface on each CAMAC crate provides spark protection for all of the signals coming into the crate. The first sparks occurring at high terminal voltage damaged the 180° magnet power supply and some other terminal electronics. The EMI had gotten onto the terminal AC power from the accelerator tube heater plates. Since that problem has been eliminated, only a few sparks have caused failures and these have been easily corrected by improving shielding or spark protection of the offended device. Some complications have arisen from operating electronics in high pressure SF_6 or with 400-Hz AC power, but these were easily corrected.

No more than one tank opening was caused by a failure of CAMAC inside the column. The heart of the console is the interactive CRT which displays pages of parameters as shown in Fig. 7. By using a track ball to move a cursor around the CRT the operator can carry out status controls and assignment of selected parameters to console knobs and meters. At the bottom of the CRT page are brief operating instructions for the parameters on that page. As shown in Fig. 8, three large diagrammatic displays above the console give the status of beam line components, vacuum system, and the gas handling system. The console has six assignable knobs, nine assignable meters, two assignable slit meter arrays, nine dedicated meters, and light annunciators for error reporting.

Beam Tests

The first beam tests attempted were those for protons, and a number of minor mechanical problems arose which slowed progress. All of these problems are well on the way to being corrected now. Running protons in the folded tandem is somewhat more difficult than in the straight through tandem. Loading which affects the charge balance and the voltage gradient can be caused by x-rays which are generated when protons strike apertures and cups. This loading is easily tolerated while getting the beam to the terminal and around the 180° magnet. When the beam enters the high energy tube, however, one can by focusing or steering direct the primary beam onto tube electrodes, which can produce high loading because it upsets the tube gradient and electron suppression no longer works correctly. This loading can change the terminal voltage enough so that the 180° magnet does not allow the beam to enter the HE tube. It is helpful to first adjust all optical elements through the accelerator with a beam of about 1 μA. The beam can then be increased without significant loading difficulties. At the specified current of 5 μA at 13-MV, however, a glitch may deflect the beam into the tube requiring the operator to insert a cup to allow the terminal voltage to come back up. This problem will be solved by making use of apertures at the entrance to the high energy tube and the slits at the 90° position of the terminal magnet to keep the primary beam from striking the tube electrode. These components were not in operation until the last maintenance period. The proton specification at 2.5 MV was completed but since the 13-MV test proved more difficult, protons were temporarily abandoned in favor of heavy ions.

The good news is that the folded tandem is an excellent configuration for a heavy ion accelerator. Beam tests for chlorine and iodine were routinely carried out at 5 and 13 MV. The transmission from the low-energy injection cup to the end of the target room beam line at 13 MV was about 10% of the injected particles and was about 6-8% of the injected particles at 5 MV. Of course, most of the incident beam is lost to other charge states and to scattering in the terminal stripper. With 5 μA of I^- injected at 13 MV the lost current was only about 15 μA and there seemed to be no need to try to use the electron traps installed in each of the dead sections. The heavy ions do not produce x-rays when they strike the accelerator tube apertures, so the loading due to this effect is much smaller than that produced by protons. For heavy ion beams loading may also result from electrons produced on axis by stripping of the beam in the residual gas. This effect was of no significance since the vacuum in the tube was generally in the 10^{-8}-Torr range. The gas stripper worked well and was pumped by one pair of sublimators in the stripper pump. The total number of sublimators should be adequate for at least six months continuous operation.

The complication of getting beam around the 180° magnet is something most tandems don't have to contend with, but it is not a difficult problem. It helps to have approximate values for setting the 180° magnet current and electrostatic quadrupoles in the terminal to start with. This is especially true for the 20UR because it has a relatively small buncher tube on the high energy side of the terminal that is used with the in-terminal ion source. The Faraday cups and variable apertures in the dead sections were seldom needed to get beam from the injection cup to the terminal or from the terminal out of the machine. It was not difficult to locate a beam on a Faraday cup with a little tuning through the entire length of the accelerator tube. After the beam had been tuned for transmission through the accelerator, the source output can be turned up and the accelerator accepts the higher currents without complaint. The beam can be stopped on a Faraday cup in the pre-acceleration beam line or in the terminal and will instantly reappear on the target room cup when the cup is extracted. The terminal potential stabilizer (TPS) holds the terminal voltage stable in the generating voltmeter mode so that the beam can be passed around the 180° magnet and the 90° analyzing magnet at which points the TPS is transferred to slit mode. Terminal stability during the beam tests was typically less than ±400 Volts as measured on the capacitor pick-off.

Accelerating Tube Conditioning

Early conditioning of the accelerating tube showed that the corona tubes which had good life in the Argonne FN and Israel 14UD were susceptible to spark damage in the 20UR. To locate damaged points more easily and to help correct the problem we installed a complete set of open points on the column and tubes using a different type of point. We staggered the location of the points as much as possible so that a spark would be less likely to propagate down many points in series. These points survived much longer than the previous ones, and there has been no apparent spark damage to the new points.

Another setback in conditioning the accelerator occurred last February while the terminal was at 18.6 MV. Someone opened a valve in a target room to the atmosphere and the resultant discharge sputtered some tubes on the high energy side. Since those were replaced the voltage has been up to about 19 MV, but a number of small problems kept us from reaching 20 MV. Conditioning one unit at a time revealed some problems that have since been corrected. The on-site crew has just finished maintenance on a variety of vacuum-system components and soon they will be pushing for high voltage again. While we have not yet reached 20 MV, the limiting factors appear to be well understood and there is every reason to expect success in the near future.

Conclusion

When we first considered the folded tandem configuration for these large machines some of us were concerned about the complexity of the dead sections and the terminal, specifically about the reliability of the 180° magnet. We also worried about the digital control system that was specified because machines that operate at even lower voltages are notorious destroyers of electronics. The experience to date strongly indicates that the above features are among the strengths rather than the weaknesses of these accelerators.

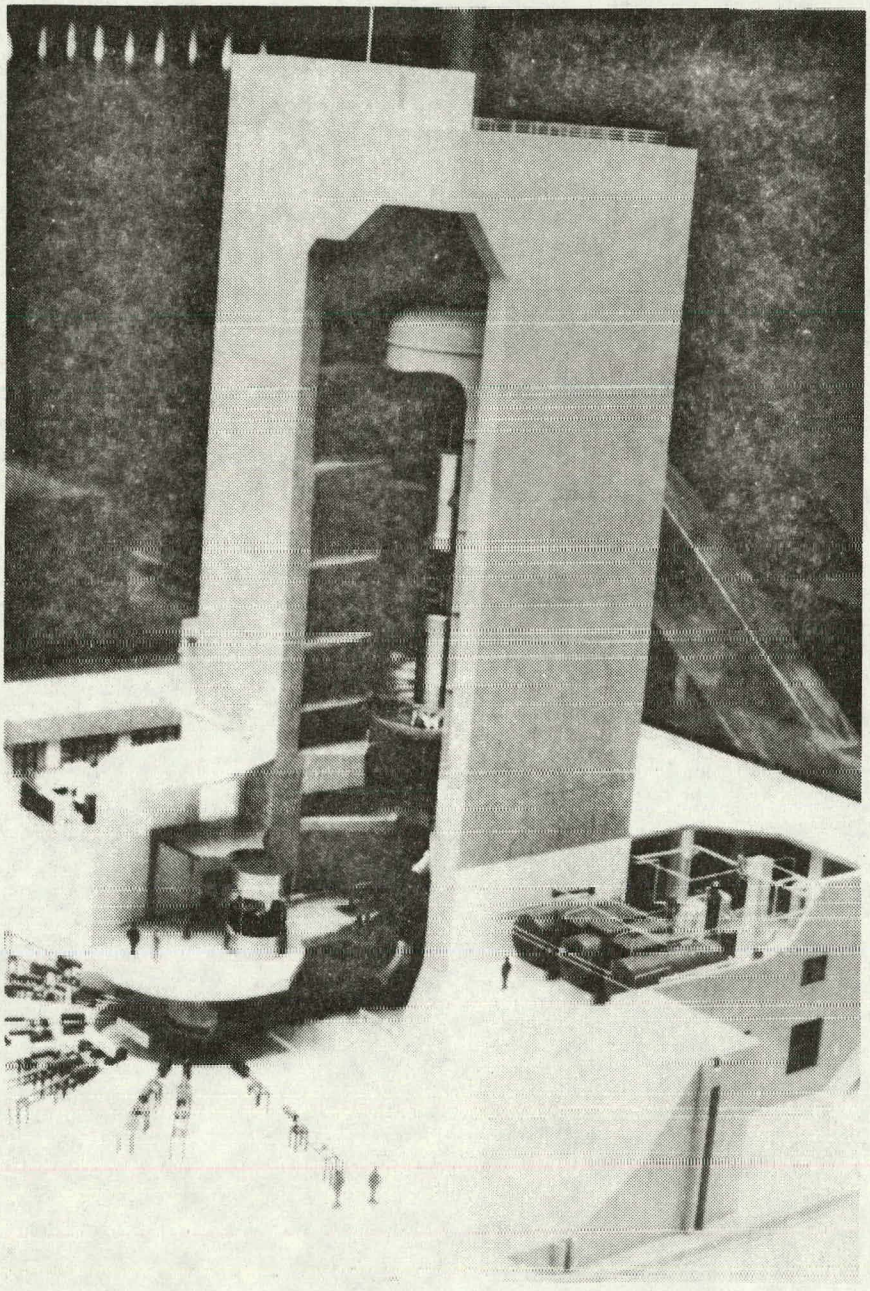


Fig. 1. A model of the 20UR Pelletron installation with cut-aways of the building and tank.

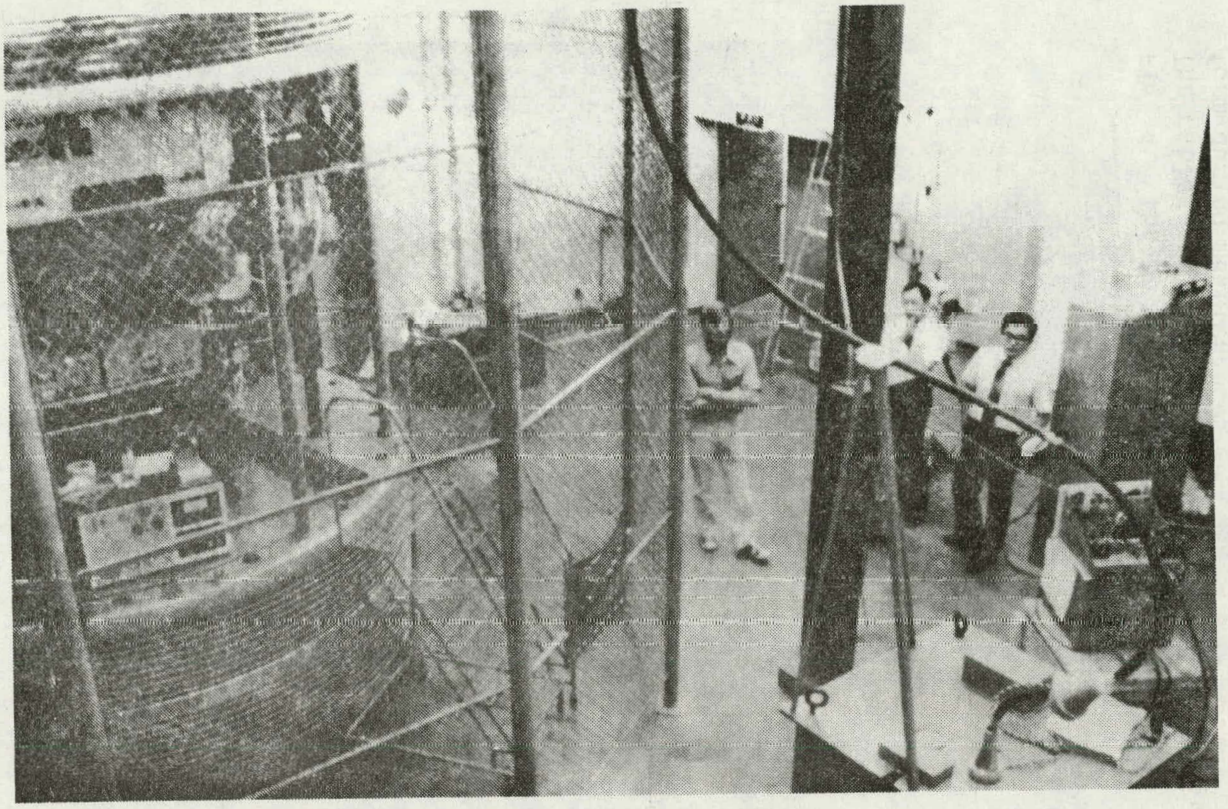


Fig. 2. The 300-kV injector.

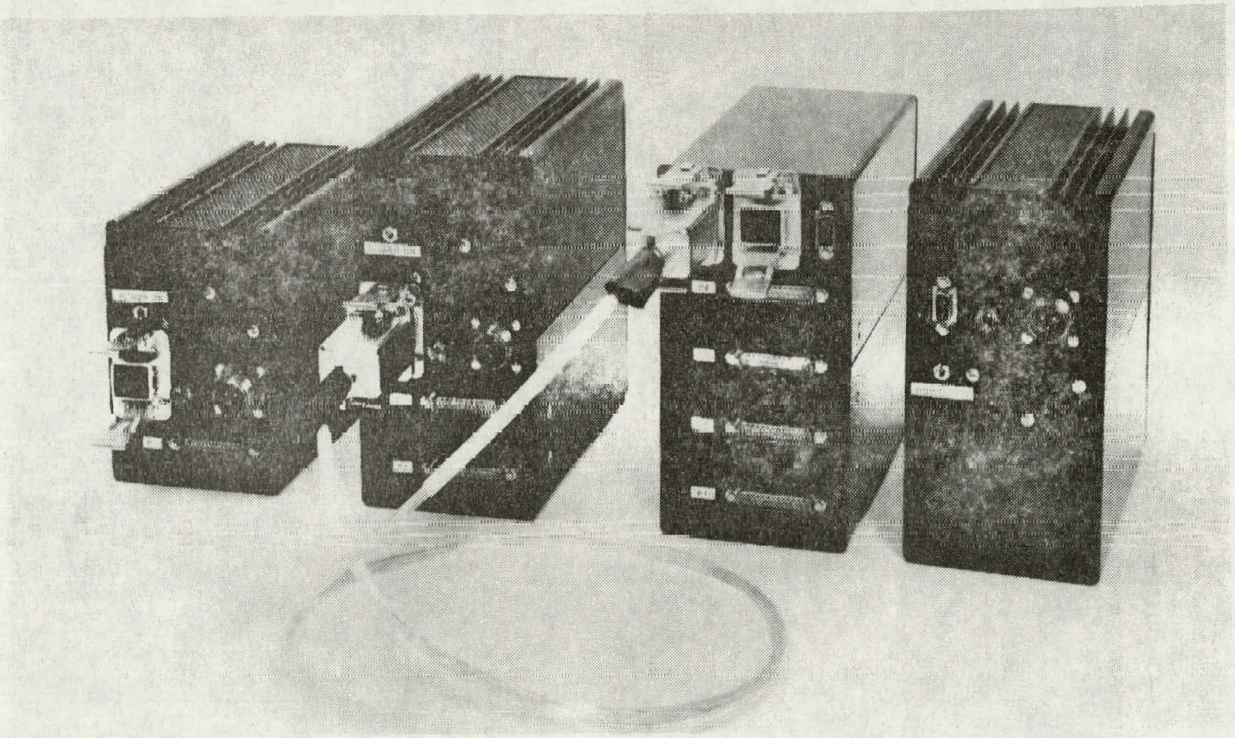


Fig. 3. Fiber light link packages for communication to the ion source power supplies.

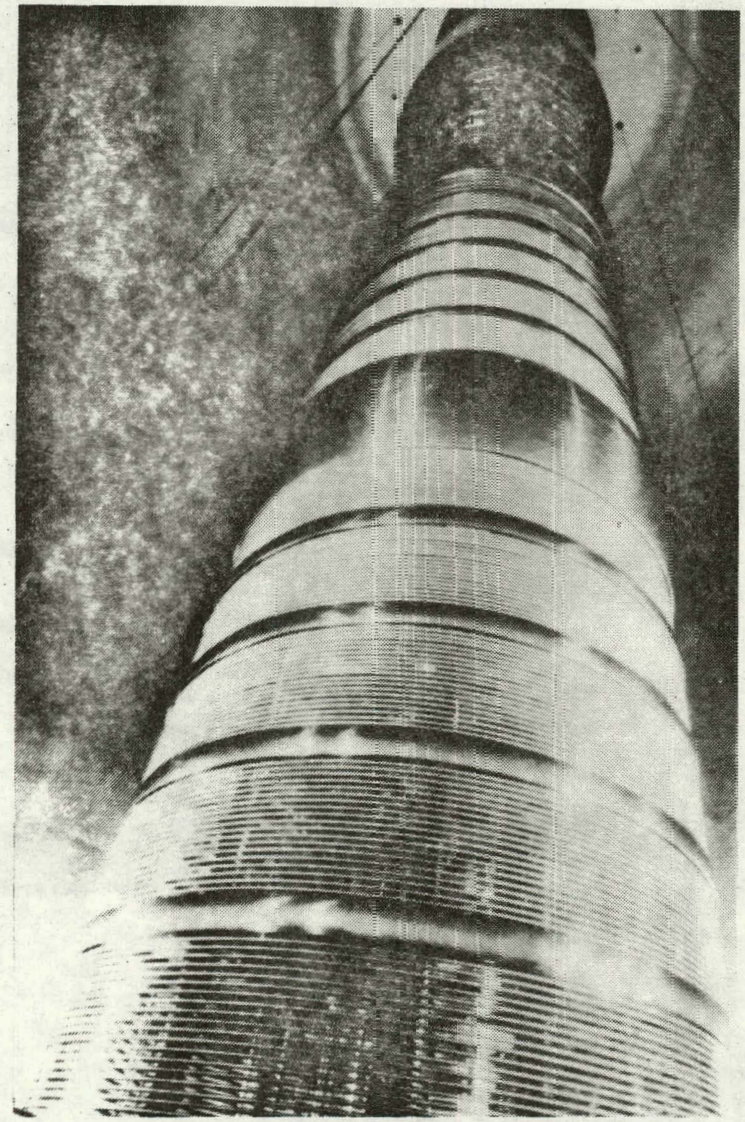


Fig. 4. The 20UR column inside the tank. The cables support the annular service platform during maintenance.

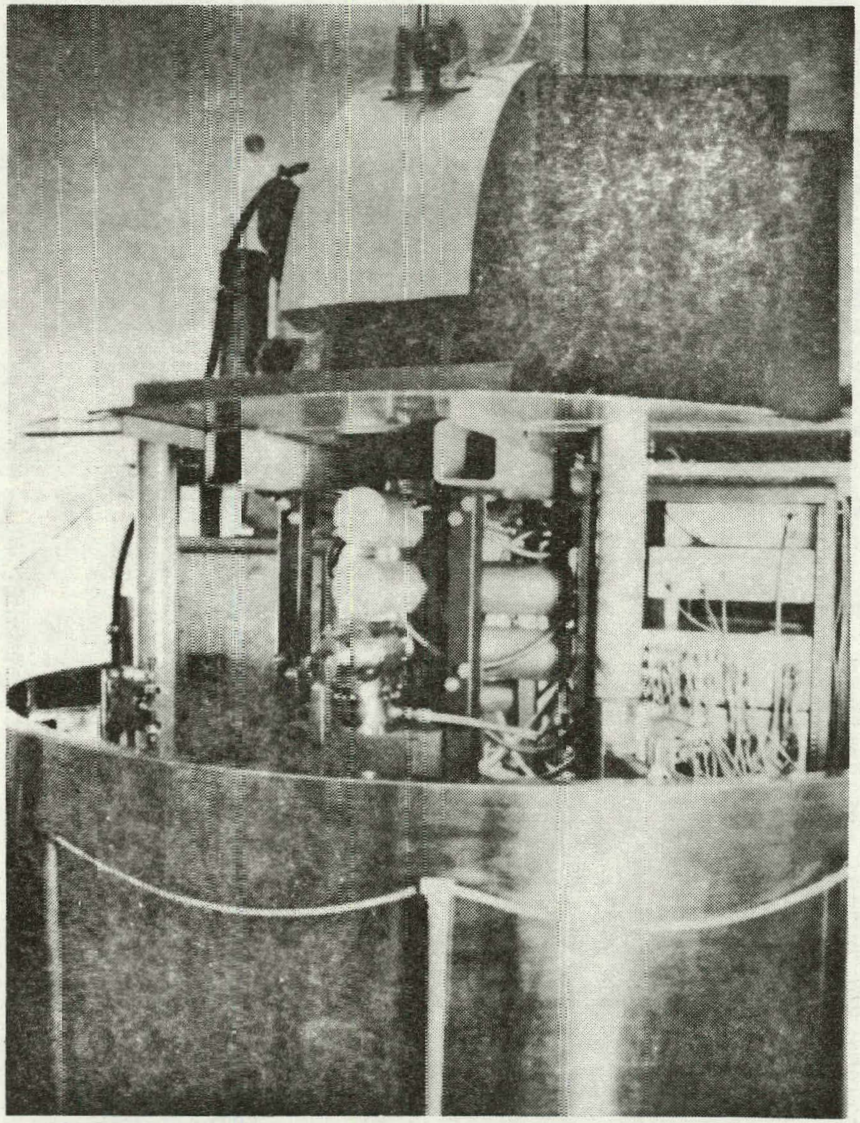


Fig. 5. The terminal with the upper spinning removed.

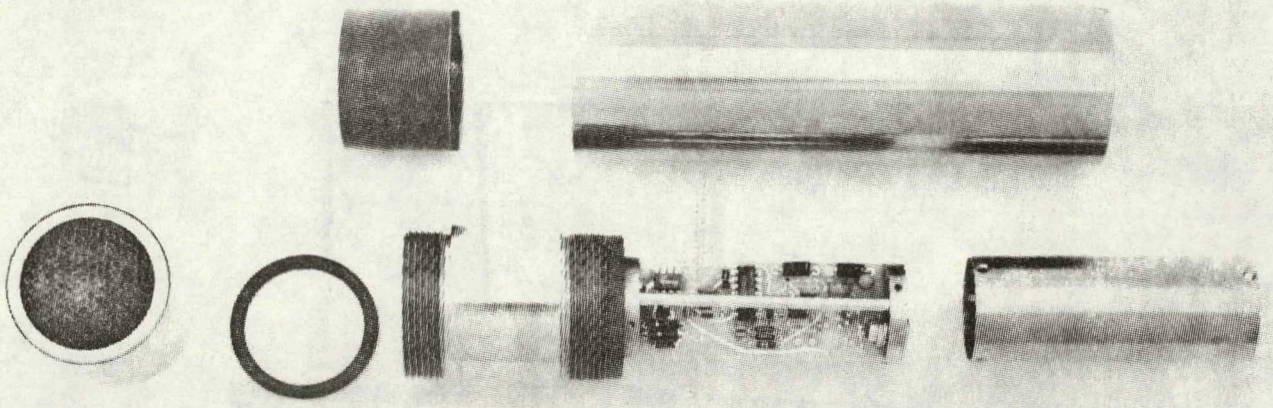


Fig. 6. The direct-optic digital links showing double shielded construction and honeycomb window.

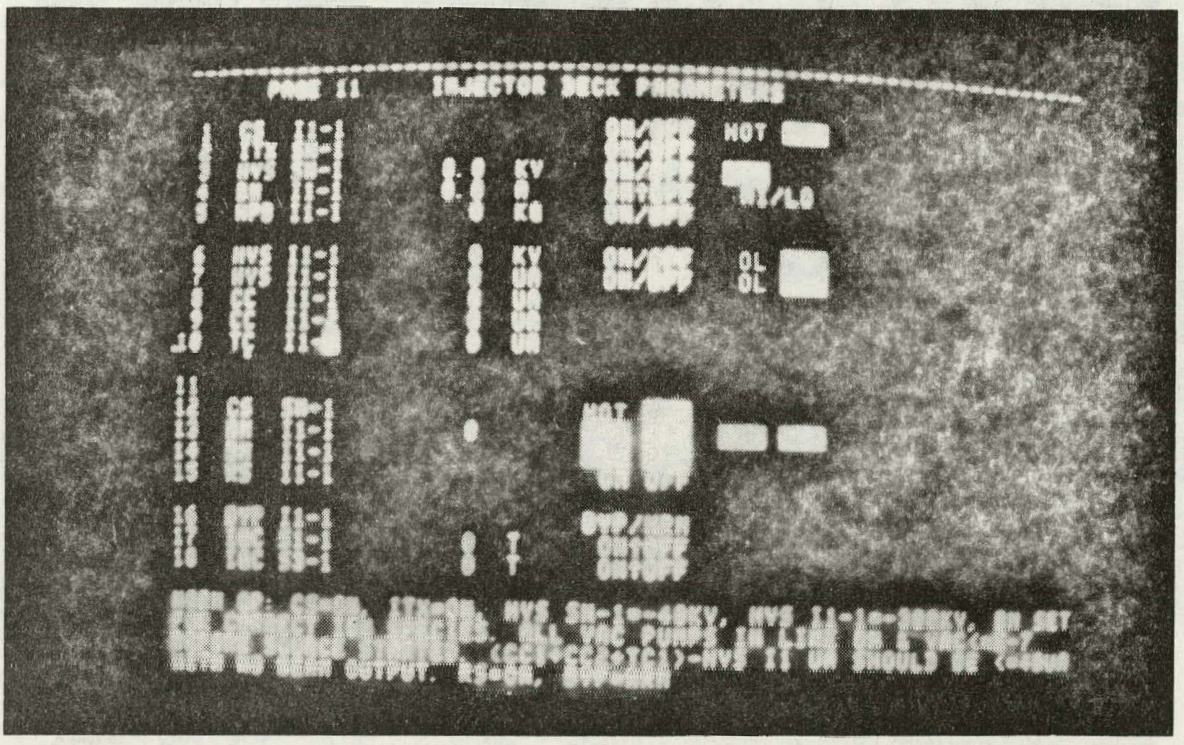


Fig. 7. Console color CRT interactive display of accelerator parameters.

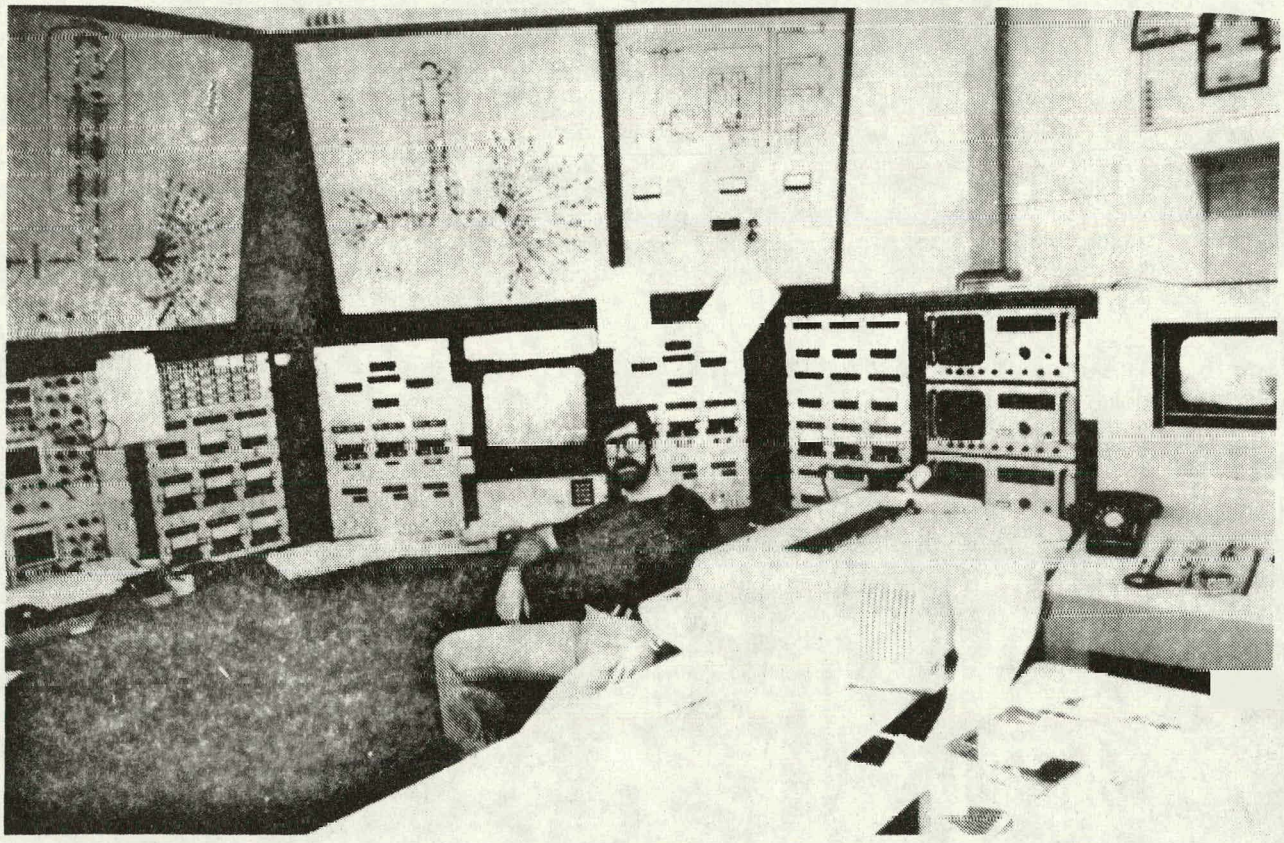


Fig. 8. The control console and diagrammatic displays of accelerator components, vacuum systems, and the gas handling system.

Walker: Robert, how do these new open corona points compare with your old open corona points?

Rathmell: The last version of old open corona points had the same kind of needle that is used in the enclosed corona point system. There were three needles in a plane. The new open corona points have a larger diameter needle. The larger diameter needles helps prevent melting of the needles when they are subjected to very large currents.

Chapman: You said if I remember correctly that one of the sources was to be a Heineke source. What beams was that designed to produce?

Rathmell: Yes, it is a Heineke Penning source that we used for chlorine and iodine beams. The sources were specified by the customer. Specifically what they intend to do with it I don't know. It is a very nice source for oxygen, chlorine, and iodine; it's very stable and very reliable.

Billen: The console controls look very much more complicated than the Oak Ridge controls. Was that a customer option?

Rathmell: The customer for both machines specified in pretty great detail what the control systems would be like. When first specified I thought these were terrible control systems to put on machines. But now after some years it is starting to look like they are not so bad. I'm rather late in seeing their wisdom.

Allen: How is the magnetic field in the 180° magnet stabilized? Is it off a sensor or do you just stabilize the current?

Rathmell: It is a current-regulated power supply. There is a rotating-coil gaussmeter to read out the field, but it is not used for control.

Noé: You referred to 15 μA of lost charge. Is that in excess of the charge that you expect to lose because of the beam?

Rathmell: Yes, that is taking the beam into account.

Noé: Could you describe the tests that remain to be done? Is there a voltage test at 20 MV without beam?

Rathmell: There are mainly voltage tests with beam at 20 MV. There are also the proton tests at 13 MV which we were not quite able to complete. We also must do iodine tests at 13 MV in addition to a variety of pulsed-beam tests. They have a pulsed in-terminal ion source and there is nanosecond pulsing on the external beam. So there is quite a list of beam tests remaining.

THE TU TANDEM INSTALLATION AT LEGNARO

by

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The TU for Legnaro is similar to the MP tandems with several major modifications and additions including the following:

1. A considerably larger diameter tank. Twenty-five feet instead of the MP's 18 feet.

2. Column modifications to give some improvement in the electrostatics and to relocate the accelerator tube 10 inches below the tank centerline instead of 6 inches as in the MP.

3. A longer high-voltage terminal, 10 feet compared to the MP's 8 feet, and with a solid skin shell substituted for the rods and hoops.

4. Daresbury Laddertron - adapted to a horizontal configuration with supporting idler wheels in the dead sections.

5. Munich type insulating power shaft - extrapolated to 6 kVA.

6. 150-kV Injector purchased from General Ionex.

7. A terminal assembly with an electrostatic triplet lens and a vacuum system similar to Chalk River's. This includes a titanium sublimator, 2 getter ion pumps, and shut-off valves.

The following photographs provided by the Legnaro Group illustrate some of the above new features.

The large diameter tank is shown in Figure 1. From the beginning the TU was designed to be an SF_6 insulated machine so the tank was designed for a maximum pressure rating of 135 psig, considerably below that of the MP tanks. The re-enforcing hoops are necessary to provide the stiffness necessary to insure against buckling when this tank is evacuated.

Figure 2 is a photograph of the column and terminal and shows the solid skin terminal shell. The shell is 81 inches in diameter and consists of 2 end cowlings and 18 panels for the cylindrical section, all fabricated from stainless steel sheet. The factory tests of the XTU indicate that this terminal shell provides no terminal to tank voltage holding advantage over the MP's rod and hoop structure. The column hoops are 75 inches in overall diameter with a cross-section diameter of 2 inches. They are mounted on a 3 inch pitch.

The contractual requirements for the Legnaro TU were set before factory tests of the Laddertron were made. These requirements called for 2 chains with a total maximum short circuit charging current of 800 microamperes. The Daresbury design of chains was followed closely with all major dimensions maintained. The critical insulating link is an exact copy. The method of construction of the conductor was changed for economy of manufacture in the U.S. Adaptation to a horizontal configuration requires shielded support idlers in the column dead sections. A set of twin support idlers is shown in Figure 3. The chains were installed initially at Legnaro to run in opposite directions to minimize gradients produced by the charge.

The power shaft is an extrapolation of a design first executed by Hartmut Steffens of Munich. This general design features the use of a relatively small diameter Lucite shaft operating at low torque and high speed with frequent support bearings along the column. The shaft is shown in Figure 4.

There are 4 bearings in each insulating section with an average spacing of about 17 inches. This shaft was factory tested in MP-0 and the results of those tests paralleled the original experiences at Munich. The bearings in the insulating sections are attached directly to the Lucite shaft and it's important that these bearings are cool in operation. The trick is a preliminary run-in of the lubricated bearings before installation in the machine with the Lucite shaft. Following this procedure no bearing difficulties were encountered in the factory tests. At Legnaro, however, there were early on a number of bearing failures that we think were associated with improper lubrucation. The shaft is directly driven with a two-pole motor at full line frequency, i.e. for 60 or 50 cycle power 3600 or 3000 RPM, respectively, less the induction motor slip. The drive motor is a 15-hp unit allowing for future increase in isolation power including possibly installation of dead section power. The Munich design utilized in-line custom built double-ended shaft alternators. We instead use standard alternators-cog belt driven.

The power shaft was also a contractual requirement. However, during factory tests of a single Laddertron chain in MP-0 we found that a single chain could deliver enough mechanical power to drive a 5 kVA terminal alternator in addition to charging the accelerator terminal. Using the chain to develop the terminal isolation power did, however, slighly degrade the basic terminal voltage stability.

Figure 5 shows the critical components of the General Ionex injector, in particular the high resolution 90° mass analyzing magnet.

At Legnaro the 2 chains, running in opposite directions, satisfactorily met the 800-microampere short circuit charging requirement. There seemed to be little, or no, interaction between the chains. We would have liked to observe performance with the chains running in the same direction but time was not available.

Starting in December of last year (1979),the machine was operated

without tubes to demonstrate the 20-megavolt capability of the generator. We had far more difficulty in this phase than we had anticipated based on the factory testing of the original XTU. At first the machine was limited in the neighborhood of 12 to 13 megavolts by sparking thought to be within the columns. Lengthy tests were made in an attempt to pin down the faulty sections; however the cause, or causes, of the problem were never determined. Throughout the various runs performance simply improved gradually without any specific remedial action.

Figure 6, also provided by the Legnaro group, illustrates the voltage performance achieved in May, 1980. At first the gas pressures used were lower than those required for 20-megavolt operation but the voltage limit in the beginning was, judging from past experience, never accounted for by low gas pressure. It seems most likely that it was just a case of particulate material raining out of the column.

A breakdown of one chain occurred during the period of voltage testing. This chain was removed and the tests continued with one chain only. The charging capability of a single chain is entirely adequate and servicing access in the column is much improved. Consequently, the second chain has been left out permanently and available as a spare. Mechanical failure of an insulating link was the cause of failure.

Since May of this year, the tube system has been installed. The first pressurization after the tube installation indicated a pressure vacuum leak. This was traced to a weld on a bellows assembly, has been repaired, and the machine is ready to be pressurized again. In this machine we have installed a system for pressure checking all the internal gasketed seals with helium before closing the tank. These joints are all either equipped with double seals or means for installing an enclosure which can be pressurized. Pressure-vacuum leaks in gasketed joints have always been one of the most time consuming problems in our tandems. The only pressure-vacuum leak so far encountered was in an area not covered by the system. We still hope to be able to eliminate a major fraction of this type of delay.

We expect to apply voltage to the tubes and make preliminary beam runs within the next week or two.

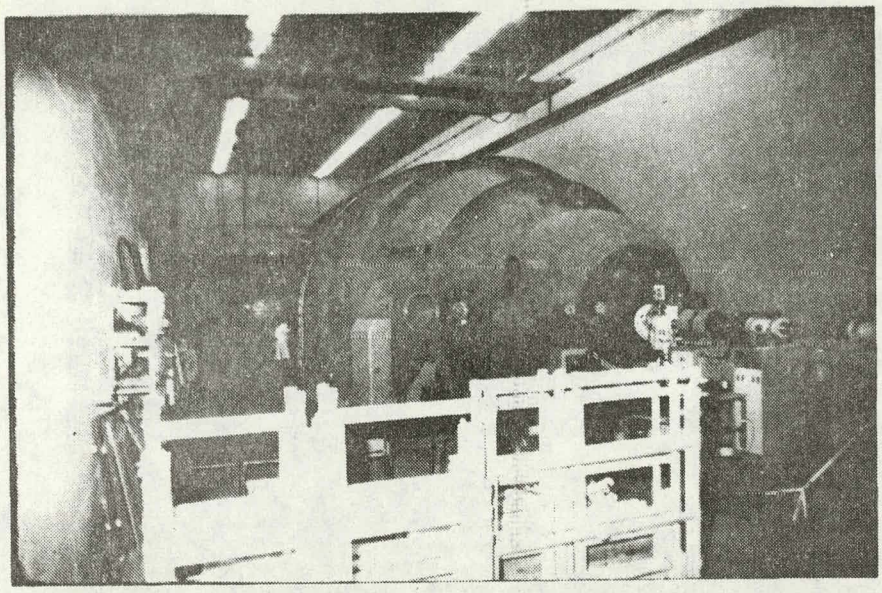

Fig. 1.

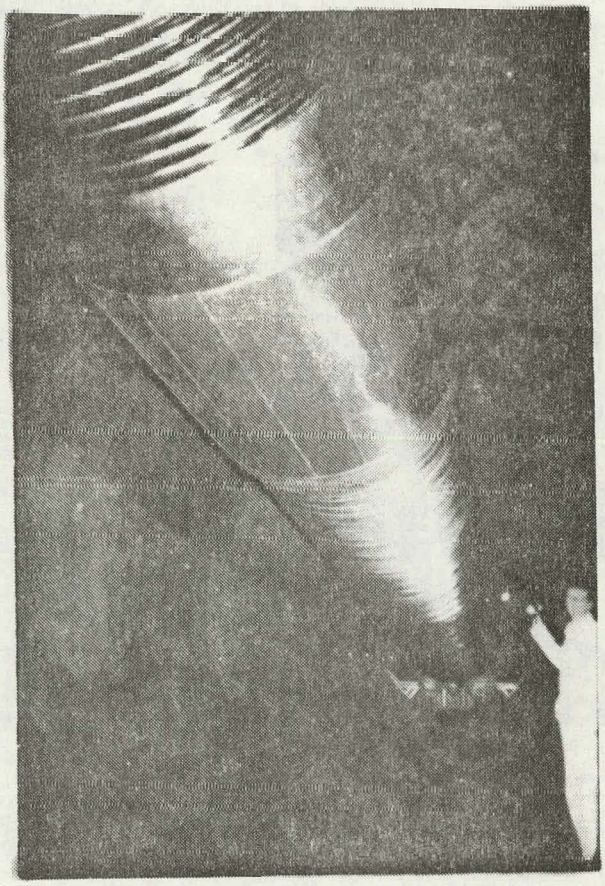

Fig. 2.

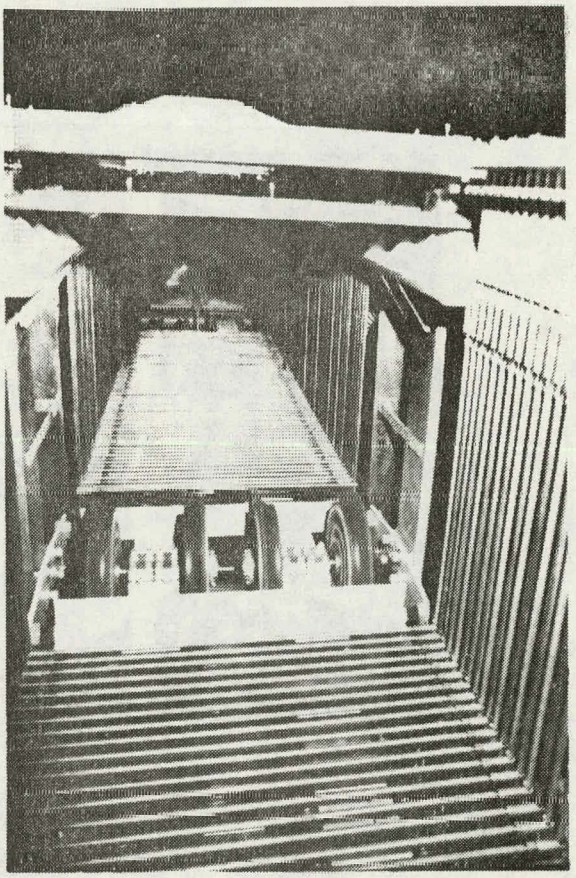

Fig. 3.

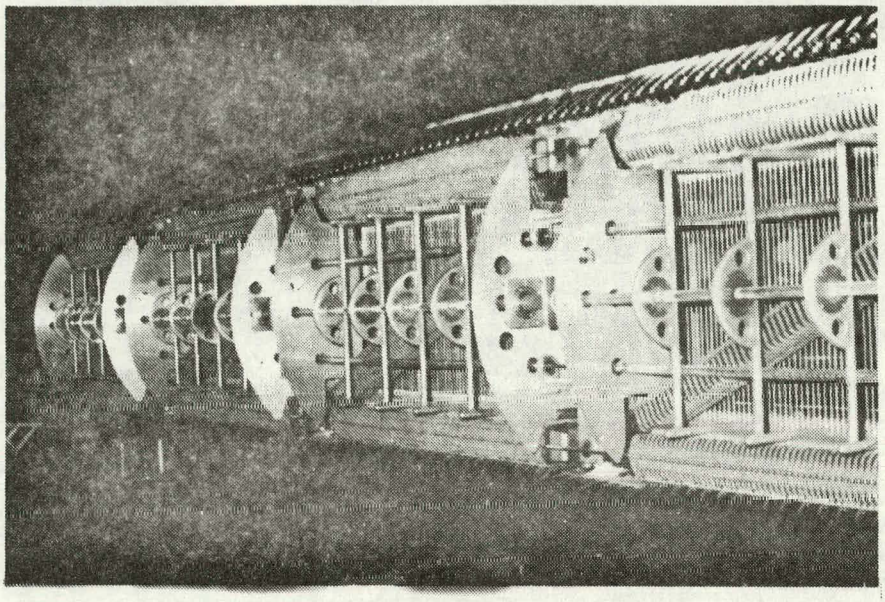

Fig. 4.

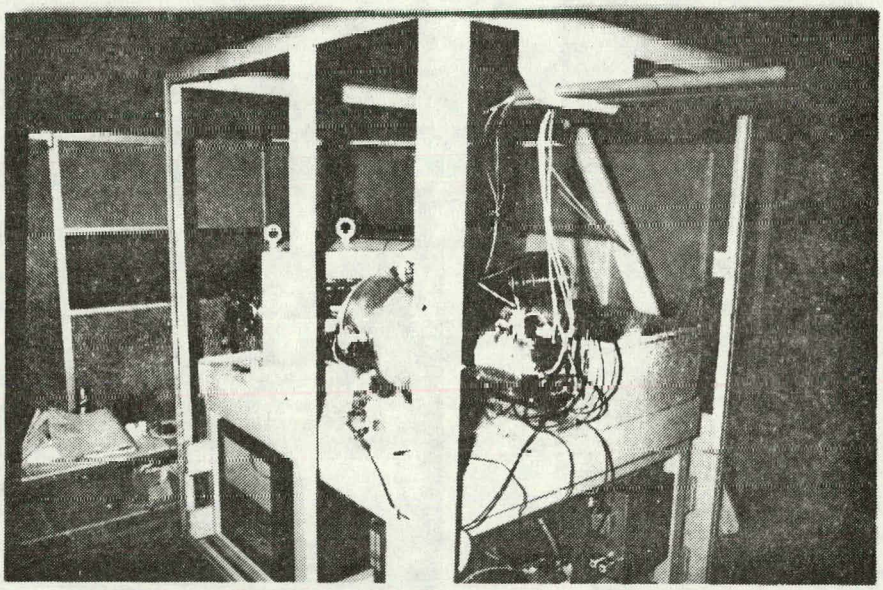

Fig. 5.

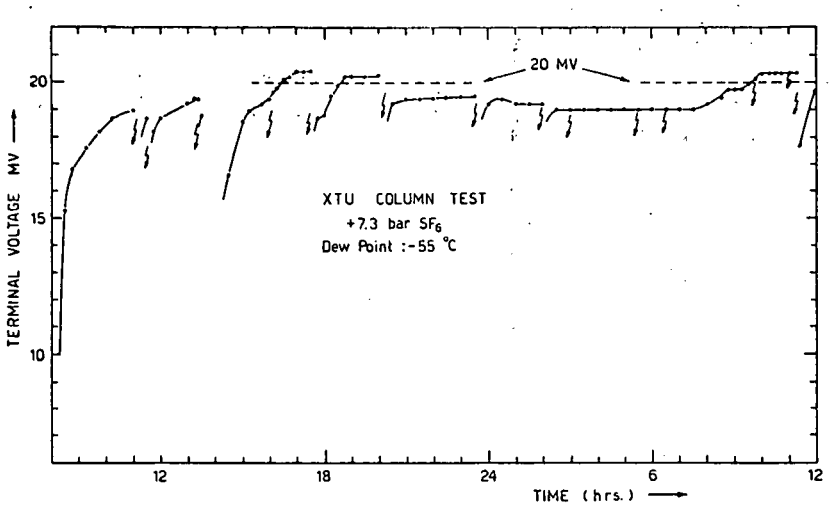

Fig. 6.

Jones: In your picture of the column there are two other lucite shafts. I assume those are control rods.

Goldie: Yes, we still are using old fashioned mechanical controls to the terminal. There are a great many of them, I suspect about 12 or 13.

Mourad: Do you have any charge selection in the terminal?

Goldie: No. All we have in the terminal is an electrostatic triplet lens which also incorporates vertical plane steering. To some extent you can get some charge selection with that if you use apertures downstream, which we do. But the apertures are not very small and the resolution of the system is not very high.

Trainor: What does it take to remove one of those bearings on the power shaft? Do you have to pull the shaft?

Goldie: Yes, you do. You have to pull all that one section.

Noé: Is the breakage in one of the laddertrons understood?

Goldie: I didn't mean to leave that out and I'm glad you reminded me. I said that when we did the short-circuit tests we ran into a lot of time, finally getting the machine to run to voltage. Late during the voltage tests we had an unexplained break in one of the chains. The break actually occurred on one side of the chain only. The machine was then shut down and when we went to start up again the rest of the chain separated. No

damage occurred to any other part of the machine. This particular break is still unexplained. It was simply broken mechanically. We had a breakage also of the chain in Orsay which marginally was explainable. It looked to be that that link was electrically damaged, but there was no such indication on the one at Legnaro.

Noé: Was it a nylon piece that separated?

Goldie: A nylon piece separated.

Progress of the 20 UD at CNEA, Argentina

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Comision Nacional de Energia Atomica
Buenos Aires, Argentina

1. Introduction

The 20 UD Tandem accelerator for the Physics Department of the Atomic Energy Commission (CNEA) to be installed in Buenos Aires, Argentina, is currently reaching the final stage of construction at the National Electrostatics Corporation factory in Middleton, Wisconsin. This paper presents an outline of the TANDAR Project and a report on the status of the accelerator facility.

2. The TANDAR Project[1]

The original scope of the Physics Department of CNEA was construction of a major new facility for Nuclear Research based on a 20-MV straight through tandem accelerator built by National Electrostatics Corporation. This project, known as TANDAR, was subsequently extended and has now evolved into a research center which will house several other activities. It will include the Physics Department with its Nuclear, Solid State, Theoretical and Engineering Divisions, the Chemistry Department, the Radiobiology Department and the Department of Special Projects concerned primarily with the study of nonconventional forms of energy. Following this evolution the TANDAR project is being carried out in two phases. Phase I consists of all activities leading to the operation of the accelerator as an instrument for nuclear physics research. It includes:

a.) -construction of the building to house the accelerator and its ancillary equipment.
-construction and tests of the accelerator vessel.
-construction of the gas storage tanks and gas handling system.
-implementation of the experimental beam lines and associated facilities.
-construction of offices for physicists and engineers.

b.) -construction of the accelerator at the NEC factory.
-transport to Argentina.
-installation at Buenos Aires.
-acceptance tests.

Phase II includes all remaining activities related to subjects other than Nuclear Physics research. Figure 1 shows a model of the Project. The remainder of this paper presents a description of the scope and status of Phase I. Fig. 2 shows the actual schedule for its completion.

3. Building

3.1 Accelerator Building

The structure that will house the accelerator, the analyzing magnet and the injector, is a tower with a dodecagonal base, 15 m in outer diameter and 73 m high. The walls have been designed to protect all working areas from radiation produced by a 20-μA beam of deuterons accelerated to the maximum tandem energy. The shielding thickness is 2 m at the analyzing magnet level and decreases towards the top of the tower. Access to different levels of the main structure is provided through an adjacent service tower which houses a freight elevator and a fast passenger elevator.

3.2 Experimental Areas

The experimental area occupies a sector of approximately 270° around the analyzing magnet at ground level and a room in the basement situated directly below the magnet. Two principal rooms, approximately rectangular in shape with an area of 22 m × 23 m each, occupy a 180° sector of the experimental surface around the magnet. Their walls are shielded partly by concrete and partly by an earth embankment. Two auxiliary doorways, closed with removable concrete blocks, allow communication to the outside of the building and access for future heavy or large equipment.

A high radiation area has also been planned. Its design meets the requirements for installation of the on-line electromagnetic isotope separator facility. Two unshielded rooms complete the experimental area at this level. In addition there is a room in the basement directly below the analyzing magnet covering the entire area of the tower. For use in this room, the beam is deflected 15° from the accelerator axis. Figures 3 and 4 show drawings of the ground and second levels, respectively, of the building as designed for Phase I of the Project.

4. Tandem Accelerator

The configuration of the 20 UD Tandem accelerator has been described previously[2,3,4]. The basic layout of the system is shown in Fig. 5. It is a straight-through standard NEC design, the tallest they have ever built. The column is 34.84 m high and has a diameter of 2.15 m. It consists of twenty 1-MV modules on each side of the high voltage terminal along with one major and three minor dead sections. The high voltage terminal is a smooth cylinder 4.88 m high and 2.44 m in diameter. Two carbon foil changers and a gas stripper are located in the terminal. Different charge states are separated and selected just after stripping by means of a displaced electrostatic quadrupole triplet followed by a variable-diameter aperture. A second foil stripper is in the middle minor dead section. The beam is focused on the terminal stripper by a system of two electrostatic quadrupole triplets and X-Y steerers located at the entrance of

the low energy tube and at the upper major dead section. After charge selection in the terminal the beam is transported to the high energy tube by a matching lens.

The charging system is composed of two independent groups of two Pelletron chains running to the terminal from the top of the column. The accelerating tubes are of the standard NEC design. The vacuum equipment in the accelerator column includes sputter-ion pumps and titanium getter pumps.

The control system for the accelerator will follow an intermediate concept, using a CAMAC crate in the terminal for read out of all parameters inside the column and a mixed system (digital and lucite rods) for operation. Fiber optics will be used to communicate between the dead sections and the terminal crate, and between the injector and the local and main consoles. Communication between the terminal and the outside of the accelerator vessel will be by light links. The digital control system will be operated by two PDP 11/23 computers.

Figure 6 shows the column structure at the NEC factory in June 1980. At the present time it has been dismounted and is now ready to be shipped to Argentina. The injector and the ion source modules which includes a direct extraction duoplasmatron, a sputter source, and a radiofrequency source, are complete and undergoing final factory tests.

5. Accelerator Vessel

The accelerator vessel (7.6 m in diameter and 36.6 m high) has been designed in Argentina following NEC specifications. The wall thickness of 38 mm allows a maximum working design pressure of 10 kg/cm^2 absolute. This value is 2 kg/cm^2 higher than the operating pressure specified by NEC to achieve 20 MV on the terminal. Three side manways provide access to the annular service platform or to the accelerator. Two additional ports at the top and the bottom of the vessel are for ventilation and for lowering of heavy elements to the service platform. A large upper port provides easy access for assembly of the column structure and major service operations. A large number of other ports have been provided for viewing windows, cables entries, light links, probes, etc. The supporting skirt has been designed to carry the weight of the vessel (310 metric tons) plus 1500 m^3 of water during hydraulic tests. The skirt may also be realigned if necessary. Because of its size the vessel has been fabricated on site. Its construction has been completed and it is now in position in the tower structure. The pressure tests are presently underway. Fig. 7 shows the building and the pressure vessel as they were at the beginning of September 1980.

6. Conclusion

The status of the TANDAR project is, with minor normal differences, within the approved schedule for Phase I of the Project. We hope it will continue in this way until its completion by the end of 1982.

Acknowledgments

The TANDAR Project Group is deeply indebted to Dr. Charles M. Jones and to the staff members of the Holifield Heavy Ion Research Facility for their kind hospitality and valuable help.

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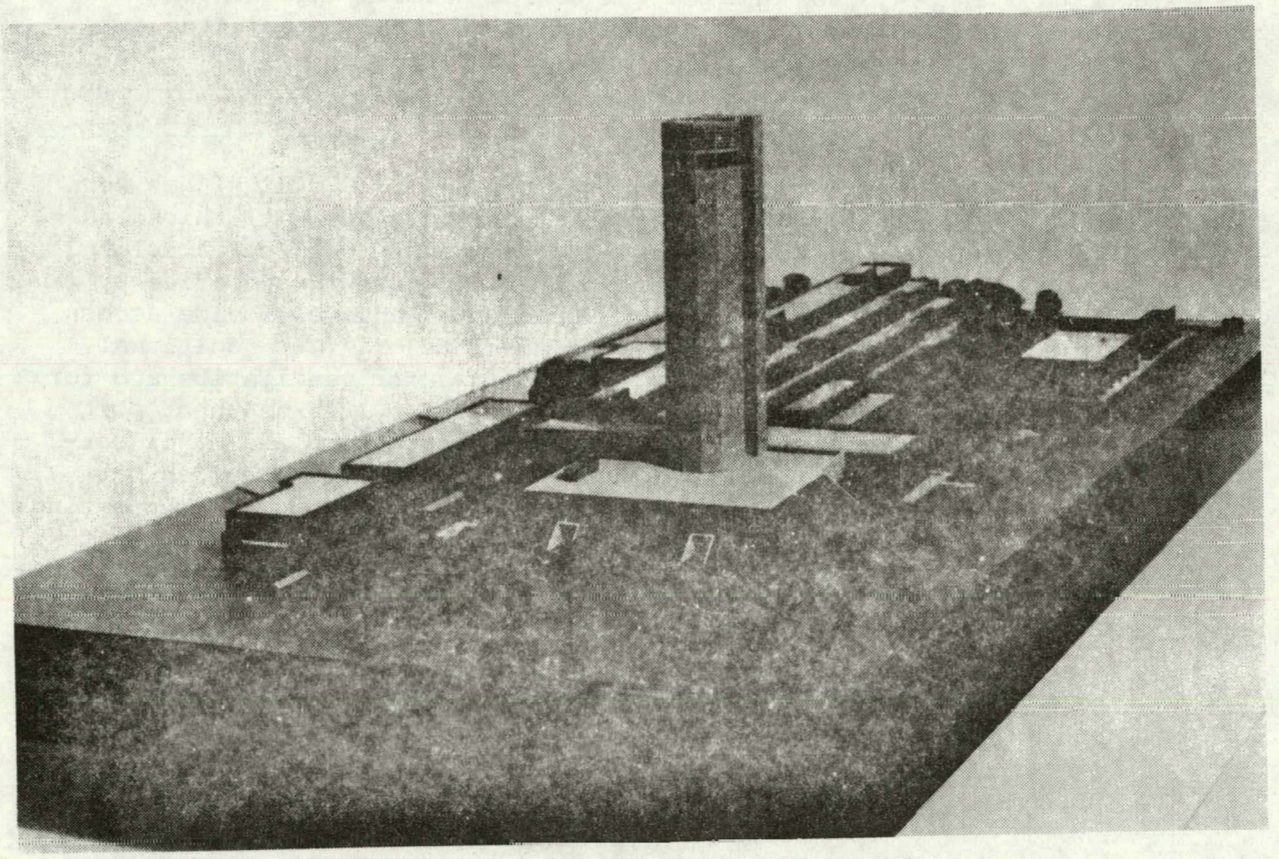
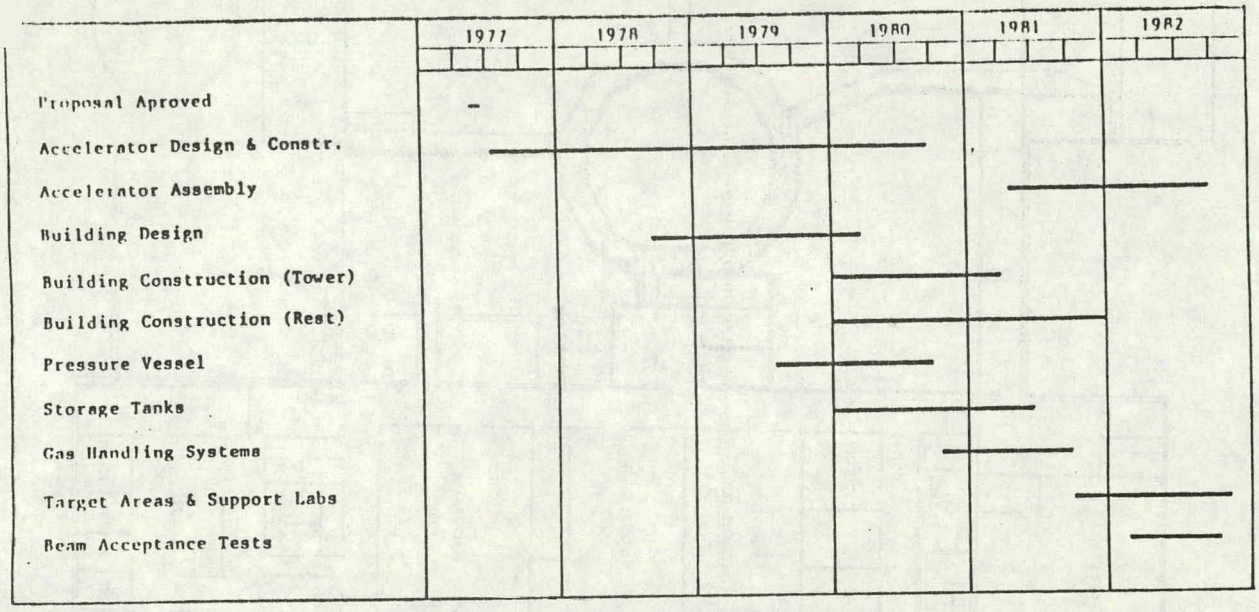


Fig. 1.

TANDAR PROJET - SCHEDULE 1st STAGE



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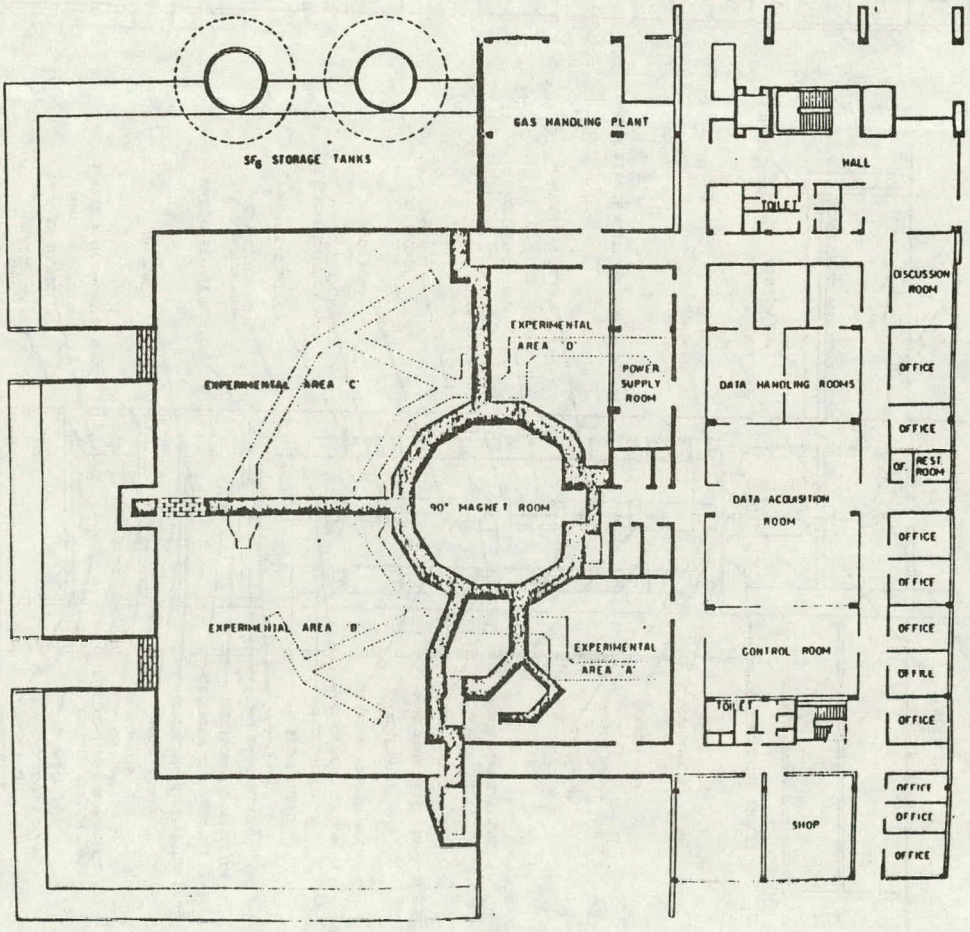

Fig. 3.

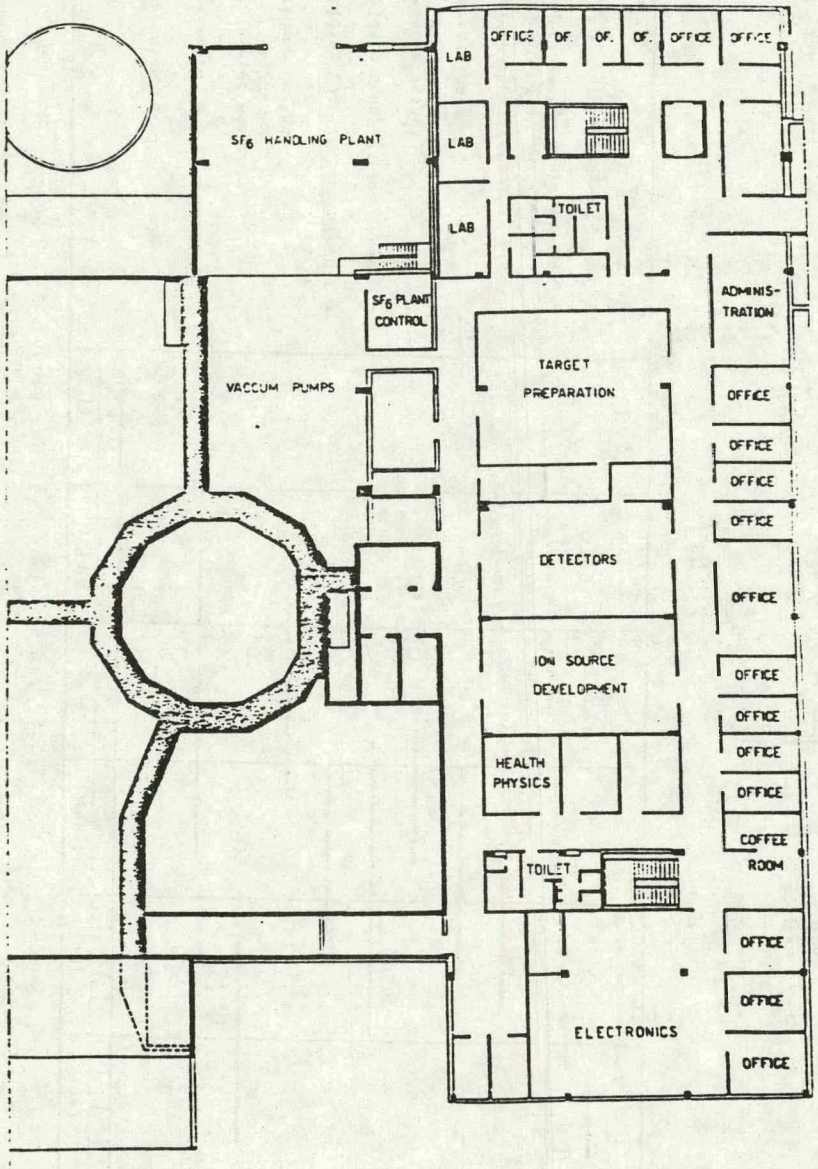
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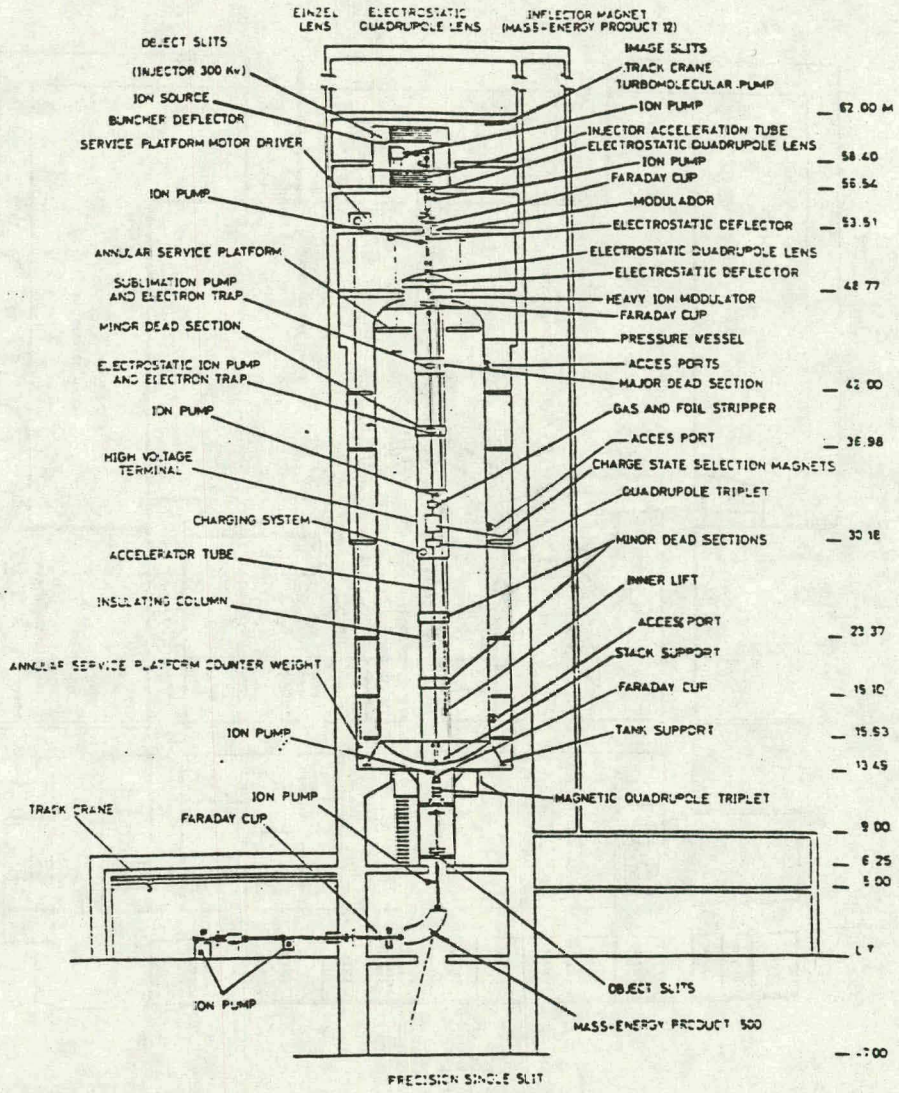
Fig. 4.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INFLECTOR MAGNET (MASS-ENERGY PRODUCT 12)
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(INJECTOR 300 Kv)
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SERVICE PLATFORM MOTOR DRIVER
ION PUMP
ANNULAR SERVICE PLATFORM
SUBLIMATION PUMP AND ELECTRON TRAP
MINOR DEAD SECTION
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HIGH VOLTAGE TERMINAL
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PRESSURE VESSEL
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MAJOR DEAD SECTION
GAS AND FOIL STRIPPER
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CHARGE STATE SELECTION MAGNETS
QUADRUPOLE TRIPLET
MINOR DEAD SECTIONS
INNER LIFT
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Fig. 5.

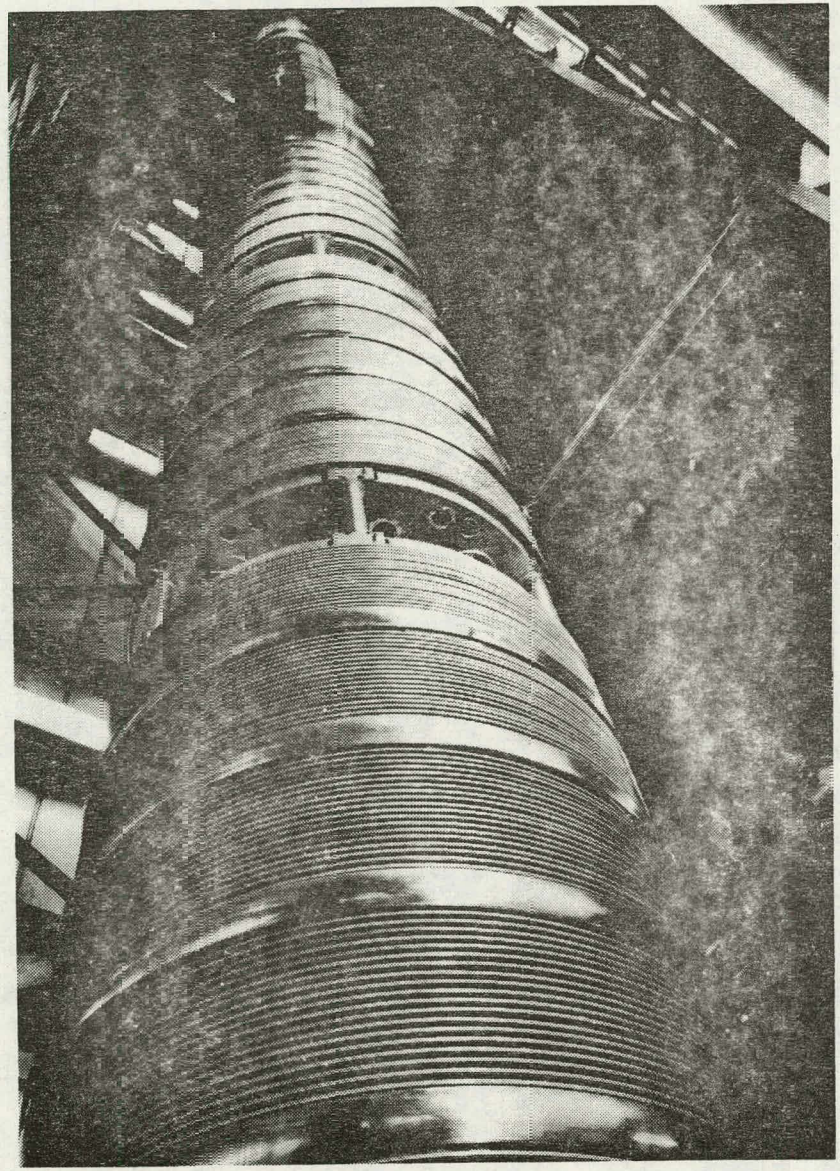

Fig. 6.

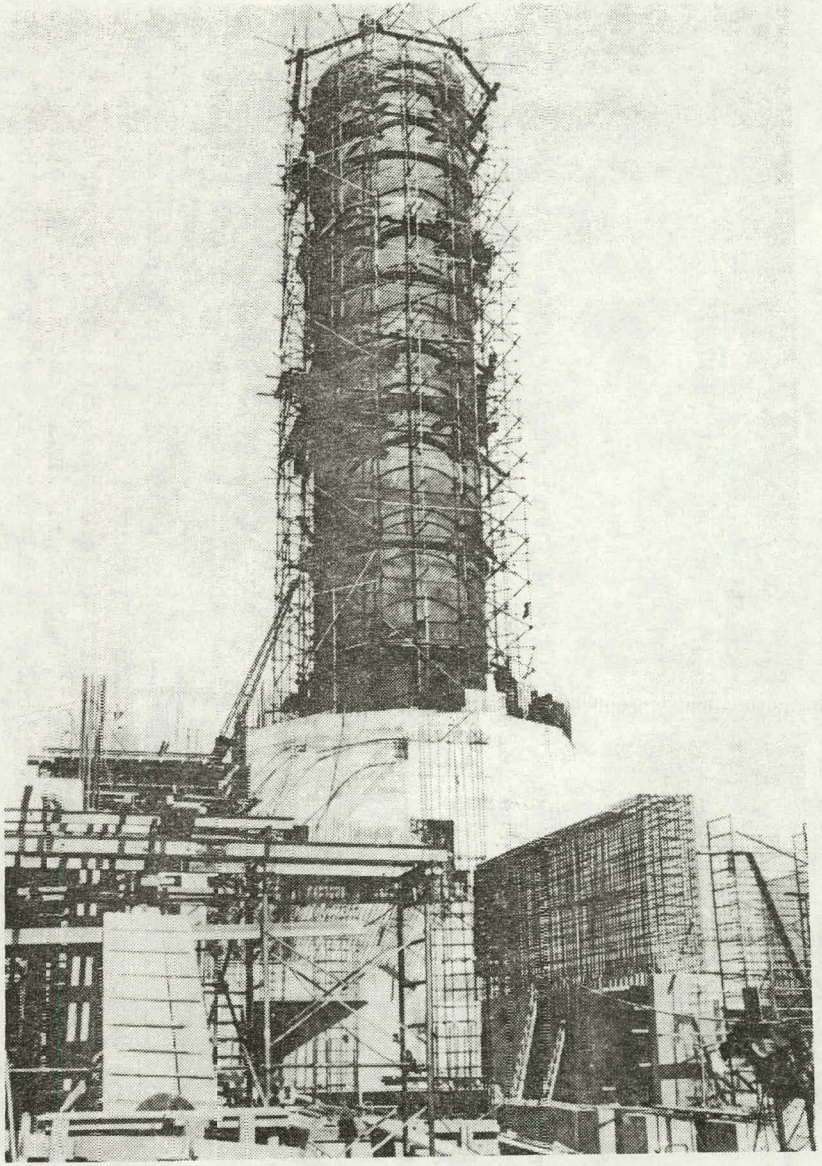

Fig. 7.

PROGRESS IN THE ART OF PRODUCING POLARIZED IONS

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1. Introduction

The purpose of this paper is to give a short review of the progress that has been made during the last two years in the design and construction of polarized-ion sources, and to summarize current research efforts on new types of ion sources. This report is based largely on the papers given at two very recent international symposia, one on "Polarization Phenomena in Nuclear Physics" at Santa Fe (August 1980), the other on "Polarized Beams and Polarized Targets in High Energy Physics" in Lausanne, Switzerland (November 1980). My presentation here will omit a detailed introduction about the basic principles of polarized ion sources based on the atomic-beam principle and on the Lamb-shift. This background material can be found in several review papers, some of which discuss the physical principles in detail.[1]

In the following discussion, the emphasis is on the production of polarized *negative* ions, but positive ions are mentioned because in the conventional atomic-beam ion source, negative ions are obtained by charge exchange of a positive polarized beam. While originally beams of polarized negative ions were developed for tandem accelerators, negative ions have important advantages also for synchrotrons and cyclotrons, because the stripping of H^- to H^+ in a thin foil can be used either for multiturn injection or for ease in extraction.

2. The Conventional Atomic Beam Polarized-Ion Source

The source of the atoms is a RF discharge tube (for H_2 or D_2) or an oven (for alkali atoms). Atoms emerging from the source aperture are collimated and passed through an inhomogeneous magnetic field ("separation magnet", usually a six-pole magnet). In this way only atoms of one electron spin projection ($m_j = 1/2$) are retained in the beam. Nuclear vector or tensor polarization of either sign is obtained by passing the beam of atoms through a set of RF transition units. Typically, about 10^{16} polarized atoms/sec can be produced.

Polarized beams of protons or deuterons are produced by electron bombardment of the atomic beam. A magnetic field of some 1-2 kG is required to decouple nuclear and electronic spin. All current ionizers are based on the electron-bombardment ionizer designed by Glavish *et al.*[2] but through the years the ionization efficiency has seen continuous improvement (see ref. 3). The present best ionizer is a new type of Glavish-ionizer, developed three years ago jointly between ANAC and CERN. It uses a plasma-discharge, supported by electrons from a filament, inside a solenoid of ~ 40 cm length. The solenoid is wound as a number of separate pancakes so that the field can be contoured. Experiences by Grüebler *et al.*[4] with a similar home-built ionizer indicates that a DC beam of 100 μA $\vec{H}^+$ or $\vec{D}^+$ can be produced. Similarly, at the ANAC plant, 80 μA $\vec{H}^+$ was readily obtained[5] in a recent routine test of an ionizer prior to shipment.

There is promise that further increases in beam can be obtained from improvements of the atomic-beam apparatus. The ionization efficiency of the ionizer increases with decreasing velocity of the atomic beam, because slower atoms spend more time in the ionizer and thus have a greater chance of being ionized. Recent studies[6,7] suggest that the ion-beam intensity can be approximately doubled by cooling the exit nozzle of the dissociator. Additional gains may be possible by careful shaping of the magnetic field gradients in the separation magnet along the atomic beam trajectory. Initially, the field shaping consisted simply of using a taper on the aperture of the six-pole magnet. Later, the use of a separate second sixpole magnet was proposed[8] to reduce chromatic aberrations ("compressor magnet"). Recently, Mathews[7] described a new atomic-beam apparatus in which the magnet location and taper is specifically adjusted to fit the velocity spectrum of their cooled dissociator. The other novelty of this atomic-beam source is that it is pumped entirely by clean pumps [one turbo pump, two cryopumps]. It is highly probable that about 200 μA $\vec{H}^+$ or $\vec{D}^+$ can be obtained if one applies the best currently known design criteria.

While the best ionizers for $\vec{H}^o$ and $\vec{D}^o$ have an ionization efficiency of a few percent, polarized alkali atoms ($\vec{Li}$, $\vec{Na}$) can be almost completely ionized by surface ionization on oxydized tungsten, heated to ~ 1800 K. If the atoms stick to the surface for more than a millisecond or so, they tend to depolarize. This can largely be avoided by operating the ionizer at sufficiently high temperature.[9] Polarized alkali sources based on this principle have been developed at Hamburg and Heidelberg for use on the Heidelberg tandem.[10]

Once a beam of polarized *positive* hydrogen- or alkali-ions is obtained, it can be converted to negative ions in the usual way, i.e. charge exchange in an alkali vapor. Again, a magnetic field needs to be applied, in this case to avoid depolarization during the short time when the positive ion has turned into a neutral atom, but not yet into a negative ion. For hydrogen ions the most successful charge exchange vapor is Na, for which one expects a negative ion yield of about 10% at energies of a few keV. In practice the yield is reduced by the finite aperture of the vapor cell and by scattering in the cell. At ETH, Zürich, a $\vec{H}^-$ beam intensity of 3 μA has been achieved by this method.[4] The development of polarized Li^- and Na^- at Heidelberg has resulted in a beam intensity of 0.1-0.15 μA.

3. Production of Negative Ions by Colliding Beams

In 1968 I proposed a new way to transform $\vec{H}^o$ into $\vec{H}^-$ without going through $\vec{H}^+$ as an intermediate step.[11] The idea is to bombard the $\vec{H}^o$ atomic beam with a beam of Cs^o atoms or H^- (or D^-) ions: $\vec{H}^o + Cs^o \rightarrow \vec{H}^- + Cs^+$, or $\vec{H}^o + H^- \rightarrow \vec{H}^- + H^o$. Typically, for the first process, the Cs^o energy should be ~ 20-100 keV, while for the second process the increase of the cross section to low energies favors as low a H^- beam energy as possible, say 1 keV or so. The resulting $\vec{H}^-$ can readily be distinguished from the incident H^- by the difference in energy, but of course D^- can be used as the electron donor just as well.

This type of ion source is still in its infancy. A $\vec{H}^-$, $\vec{D}^-$ source based

on charge transfer from a 40 keV Cs° beam is in operation on the Wisconsin tandem. An $\vec{H}^-$ and $\vec{D}^-$ beam current of 3 μA has been obtained[12] using a Cs° beam of 2-3 mA. The polarization of the beam is very high, due to the fortunate circumstance that bombardment of background gas (including H_2) by Cs° produces very few H^- ions. For ionization of $\vec{H}^{\circ}$ in a magnetic field of 1 kG, the measured proton polarization after acceleration is (91±1)%, compared to a theoretically expected maximum polarization for this particular magnetic field of 94.6%.

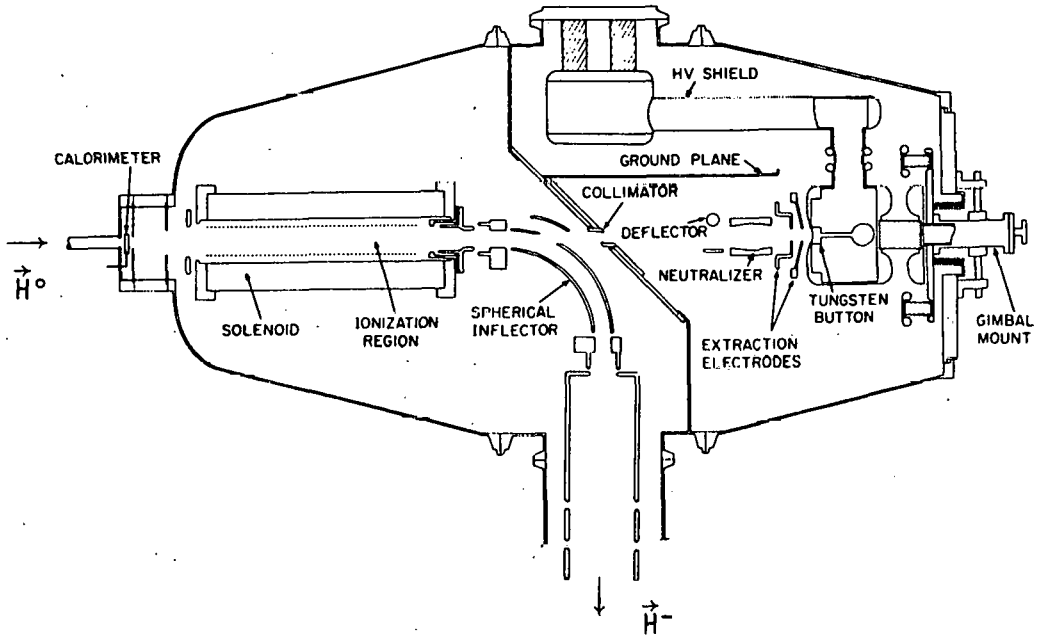

Fig. 1. Colliding-beam source for polarized negative ions. The atomic-beam apparatus is not shown.

Part of the Wisconsin source is shown in Fig. 1. The atomic-beam source and RF transitions are not shown. The calorimeter can be inserted to measure the intensity of the Cs° beam. The right-hand compartment contains the Cs° source, where Cs vapor is ionized by a hot porous tungsten disk. The Cs^+ beam is accelerated to 40 keV and is then neutralized to 95% in a Cs vapor cell. The remaining Cs^+ beam is deflected out of the way. The output of the source is limited at the moment by the relatively poor focus of the Cs beam, which leads to excessive loading of the electrodes in the ionizer if the Cs beam is increased too far. Since we have neither remote controls for the source nor tandem operators, the experimenters tend not to push the source to high intensity. All we know for certain is that 3.0 μA H^- and D^- can be obtained and that the source runs essentially unattended for 3-5 day runs at the 1 μA level. Construction of a cesium beam test bench is planned to further improve this source. A commercial source[13] based on this principle has a design aim of 10 μA. To reach the design aim will require substantial improvement in the focussing of the Cs gun or the addition of a magnetic focussing element.

The colliding-beam principle is applicable also to the production of alkali heavy ions and ions of other atoms which can be polarized by Stern-Gerlach separation. No experimental work has been done and only a few of the relevant cross sections are known. The cross section for $\vec{Li} + Cs^o \rightarrow \vec{Li}^- + Cs^+$ is largest for a Cs^o energy of about 100 keV where $\sigma \sim 3\times10^{-16}$ cm^2. A colliding-beam source using a polarized Li atomic beam[14] of 1×10^{16} atoms/sec ($v \gtrsim 1700$ m/sec) and 3 mA/cm^2 Cs^o beam would be expected to yield about 1 μA $\vec{Li}^-$ for a 30 cm long ionization region. In view of the fact that the Wisconsin colliding beam source produces the predicted H^- intensity[2], it is likely that this would be the case for operation with Li as well. Nevertheless, it is certainly possible that as much or even more beam could be obtained with the Heidelberg-type source. Even if it should turn out that the colliding beam method for heavy ions does not surpass other schemes for beam intensity, this would seem an attractive method for laboratories requiring both H^- and heavy ion beams since one would only need to replace the dissociator (for H_2) with an oven (for the alkali atoms) to switch from one ion species to the other.

4. Lamb Shift Sources

The Lamb-shift source is not only the most common source of polarized hydrogen ions on tandem accelerators, but is used also to provide polarized proton beams at two medium-energy facilities (TRIUMF,LAMPF). It appears that this type of source is approaching the end of its development as far as beam intensity is concerned. The figure of merit ($P^2 \times$ intensity) has seen no substantial improvement for some time, in spite of several high-quality development programs. While the Lamb-shift source does not provide the highest available beam intensity, it has important advantages. It is at present the only practical source for polarized tritium ions. Another advantage is that deuterium ions can be prepared in single hyperfine states, yielding a tensor polarization $P_{zz} = -2$. It should be noted, however, that this is possible also with atomic-beam sources, if one induces transitions *between* separate sections of the six-pole magnet.

5. Sources Based on Optically Pumped Alkali Vapor

In this section I want to comment briefly on the present state of sources which use optically pumped alkali vapors. We must distinguish clearly between two different types of applications: (a) for sources of polarized Li, Na etc. ions, optical pumping can replace the six-pole separation magnet employed in the conventional atomic-beam source, (b) for sources of polarized hydrogen ions, a beam of unpolarized protons (e.g. 5 keV energy) can be passed through an optically pumped alkali vapor cell. The protons pick up polarized electrons to form a beam of fast (5 keV) $\vec{H}^o$ atoms. These are then ionized to H^+ or H^- in a second charge exchange cell.

(a) The production of a polarized alkali beam by optical pumping has been discussed by Anderson[15]. In its simplest form, the source would consist of an oven containing, for instance, Li or Na. The atomic beam emerging from a small aperture is illuminated by circularly polarized laser light to excite the ground state atoms, e.g. to various $P_{3/2}$ states. As the atoms

travel through the laser beam they decay and get excited again several times. In each absorption process the atoms acquire additional angular momentum until they all are in the state of highest total angular momentum, i.e. the state of highest m_F. Another few cm downstream from the oven the atoms impinge on a surface ionizer, just as in the more conventional alkali polarized-ion source. No complete source based on this principle has been built yet, but experiments have been reported very recently[16,17] which show that ^{6}Li and ^{23}Na atomic beams can be polarized by this method.

The advantages of optical pumping over the more conventional atomic beam method arise primarily from the fact that in the conventional method the solid angle of atoms from the oven is much smaller than is possible with the new scheme. Thus one should obtain much more ion beam intensity, or instead, for the same intensity, one can reduce the flux out of the oven and correspondingly reduce build-up of alkali on apertures etc. Another advantage is the improvement in the degree of polarization resulting from pumping all atoms into a single hyperfine component. It should be pointed out, however, that a similar improvement can be gained in the conventional atomic beam source by placing RF transition units between separate six-pole magnets. There is virtually no question that the optically-pumped polarized alkali source can be made to work. However, one should not assume that this new source is necessarily so cheap and simple that the conventional atomic-beam source is obsolete. It is neither cheap nor simple to set up and operate the required tuneable dye lasers.

(b) The application of optically pumped alkali vapor to produce polarized hydrogen ions was suggested a long time ago. The general principle, illustrated very schematically in Fig. 2, goes back to a suggestion by Zavoiskii[18] to have a proton beam of a few keV energy pick up polarized electrons in a first target (he proposed a magnetized iron foil). In a

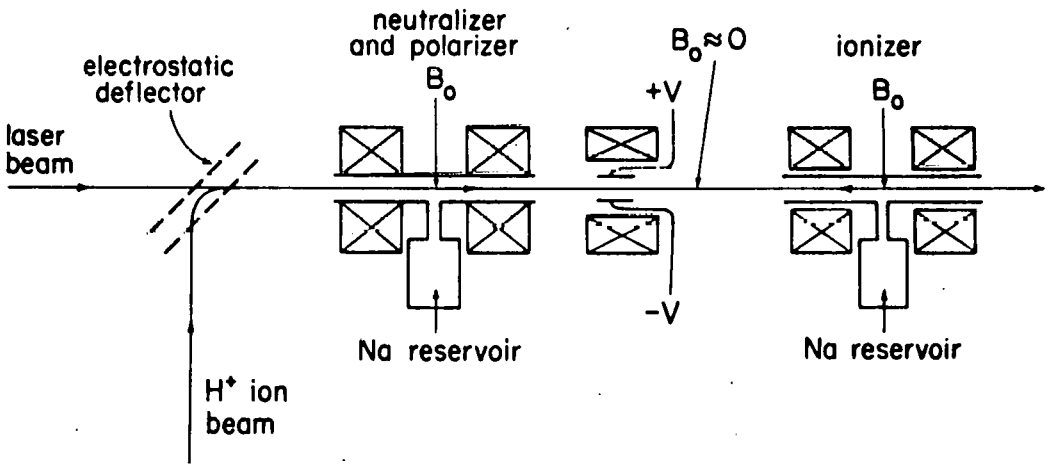

Fig. 2. Proposed production of $\vec{H}^-$ ions by pick-up of polarized electrons in an optically pumped alkali vapor cell [polarizer], followed by charge exchange of the H^0 in a second vapor cell [ionizer]. The figure is from ref. 20.

second target the fast polarized $\vec{H}^0$ are to be converted to $\vec{H}^+$ or $\vec{H}^-$. In 1965 I proposed[19] to use as the first target a polarized alkali atomic beam or an optically pumped alkali vapor. Anderson[20] recently discussed this possibility in some detail and showed that considerable $\vec{H}^-$ or $\vec{D}^-$ intensities might be achieved by this method.

There does not yet exist an operating ion source of this type, but development work is progressing in a number of laboratories. The questions are (i) can one maintain high polarization of the alkali vapor inside a charge exchange tube in spite of depolarization in collisions with the wall? Specifically, how does the polarization of the alkali vapor depend on the intensity of pumping radiation and on the thickness (atoms/cm^2) of the vapor target? (ii) how large is the resulting polarization of the beam, and how does the polarization depend on the magnetic field strength in the optically-pumped cell. Here the question is what fraction of the $\vec{H}^0$ is formed in excited states which then lose angular momentum in transitions to the ground state. (iii) what beam intensity can be achieved? This depends in part on the previous question, since depolarization can presumably be avoided by applying a sufficiently strong magnetic field to the vapor cell (~ 10 kG) but this may cause ion-optic problems. Of the above questions, the first has been answered very recently by a group in Japan.[21] They pumped a Na cell with a 1W dye laser and measured the polarization of the Na atoms by deflection in a six-pole magnet. The results show that 70% polarization of the Na vapor is achieved with a density of 3×10^{12} Na atoms/cm^2, in good agreement with Anderson's estimate. The most pressing next problem is to measure the beam polarization. It must be noted that a source of this type, even if all depolarization problems can be overcome, is not particularly suited for tensor-polarized deuterons, because the application of Sona-transitions to ground-state atoms yields a maximum tensor polarization $P_{zz} = 1/3$.

6. Conclusions

Not so many years ago, experimenters had to suffer a big loss in intensity if they wanted to use polarized beams. This has now changed dramatically. One can safely say that $\vec{H}^+$ beams of 100 μA and $\vec{H}^-$ (or $\vec{D}^-$) beams of 3 μA can be expected in routine operation if one designs a source that combines the best presently known technology for the various parts of the source. Until quite recently, a source output of 1.0 μA was a cause for celebration. Now there are available reliable and easy to operate sources which produce above 1 μA source output day after day.

The best current sources make use of the atomic-beam method. Further improvements are very likely, particularly for the colliding-beam method which is still in its infancy. Lamb-shift sources are not likely to compete as far as intensity is concerned, the aficionados claim that the new sources based on optically pumped alkali vapors will yield 100 μA $\vec{H}^-$ or more. Whatever the merit of such claims, it should be kept in mind that there is a substantial difference between estimates on paper and operating hardware. It would be a mistake to pursue only this one avenue because of expected large and easy rewards. Other schemes, like the colliding beam source using $\vec{H}^0 + D^-$ should be pursued as well, since the pos-

sible rewards in this case are probably even higher. It has been argued that no one can use more than a few μA of beam anyway, so that the interest in even more intense beams may seem misplaced. Nevertheless, it is much more comfortable to work with a source that has a margin of safety. Also, applications to meson factories, to synchrotrons and to "cyclograafs" [cyclotron injecting H^- into a tandem] would make 100 μA $\vec{H}^-$ and $\vec{D}^-$ highly useful.

The most natural application of optically pumped alkali vapor is to the production of polarized alkali ions. Rapid progress has been made in these developments during the last year. Interesting work is also being done in other areas, such as experiments to produce polarized $^3He^-$ ions. The extensive body of interesting new experiments presented at the recent International Symposium on Polarization Phenomena in Nuclear Physics attests to the important role that polarized ion sources now play in nuclear physics.

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A REMOVABLE HEAVY ION TERMINAL FOR THE LOS ALAMOS VERTICAL ACCELERATOR

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During the past year we have built a heavy ion terminal for the Los Alamos Vertical accelerator and have placed it in operation. This terminal allows for rapid change to the pulsed, light ion terminal[1] which was built by the General Ionex Corp.

The ion source used in the heavy ion terminal is an IONEX model 834 built by the General Ionex Corp. The source has been modified to use an offset cone, reflected cesium beam geometry. The beam from the source is focussed and then inflected into the accelerating tube with a 30° magnet. This magnet is capable of bending 20 keV uranium atoms.

The terminal has now operated for over 800 hours without a major failure. The source has been opened only for the insertion and removal of cones. The terminal has been used almost exclusively for the production of ^{14}C beams which are used in 3-stage acceleration. The ^{14}C beams are produced from a "pill" of ^{14}C graphite which is pressed in the downstream face of the source target cone.[1,2] Experiments have been run with up to 0.5 μA (electrical) of charge state +6 ^{14}C on target.

Several changes are being considered to improve the terminal's operation. One is to improve the source optics for more efficient transmission. Another is to go to an on-axis reflection geometry similar to that used on our tandem sputter source[2].

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Noé: I have two questions, Joe. I know these are not directly related to the source but to the procedures you use for the tank. I don't think you ever said why you backfill with nitrogen and Freon. And secondly, can you say a word about the history of the tubes.

J. Tesmer: First about pressurizing with nitrogen and Freon: we go into the machine more often than I'd like to admit. I think it's historical why we use nitrogen and Freon. I talked to Joe McKibbon about that and his comment was that well we needed to try something, so we put Freon in and it seemed to work a little better, so we left it. So we use one pound of Freon 12 to 10 psi of nitrogen. The tubes are Dowlish straight tubes with spiral magnetic suppression. They have not worked as well as we would like them to. I think we understand why they do not work and we don't see much hope of fixing these tubes. They suffer from the long tube effect. There are five sections of tube. If you take the first two sections near the terminal and run them up to the column current or the tube current that you think you need and then extrapolate, you would expect to get 12 MV on the full machine. But when the third tube is added you can't go quite as far as you should. And by the time you add the fifth tube you are actually going backwards. The machine will run just about as high with four sections of tube as it will with five sections.

A Versatile High Intensity Negative Ion Source*

Roy Middleton
University of Pennsylvania

About three years ago we undertook to develop a new type of negative ion source in which we hoped to incorporate some of the better features of the cesium beam sputter source, the reflected and inverted sources and the Aarhus source. Since then we have built and tested some seven different designs and I think finally have developed a practical and reliable new type of source which fulfills most of our design goals. Having reported on this work at previous SNEAP meetings only a summary of our most recent findings will be presented here.

Figure 1 shows not only a schematic representation of the source but also highlights many of the design features that we have found to be of extreme practical importance. For example, all of the temperatures indicated in the figure are necessary for satisfactory operation and the design of the cathode insulator, although shown schematically, is equally important.

The principles of operation are as follows. Cesium vapor flows into the source chamber and is adjusted (using a needle valve) such that the cesium pressure is about 10^{-5} Torr. Cesium atoms striking the inner surface of the ionizer are surface ionized and accelerated towards the tip of the sputter cathode and strike it with an energy of between 2 and 7 keV, depending upon the cathode potential. The cesium ion beam striking the cathode is remarkably well focussed, usually producing a spot of about 0.5-mm diameter, and in many respects is reminiscent of the reflected beam in a conventional sputter source. However, here the sputter cathode is contained in a cesium vapor environment and the presence of neutral cesium on the sputter surface greatly enhances the negative ion formation. In short, two of the most important advantages of the source are 1) the negative ion emitting area is very small, and 2) the presence of neutral cesium on the sputter surface leads to high efficiency. Other advantages are 3) the positive cesium energy can be varied independently from the negative ion extraction energy, and 4) the sputter target is in the form of a simple pill (usually 3 mm diam. by 3 mm long) and requires a minimum of machining.

Figure 2 shows a sectional drawing of the source that is presently in use in our laboratory. An enlarged and more detailed drawing is shown in Fig. 3 of the areas that have proved most troublesome, namely, the ionizer and cathode insulator.

Source Performance

Table I lists some of the negative ion currents that have been obtained with the source shown in Fig. 2. Negative ion currents are tabulated under two headings -- 'typical' and 'best'. The typical currents were usually obtained with cathode voltages between 2 and 5 kV (depending upon the element) and cathode currents ranging between 1.5 and 2.0 mA. The best currents were obtained by what is locally termed 'pushing' - cathode voltages and currents ranging up to 7 kV and 6 mA respectively. Also listed in

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* Work supported by the National Science Foundation.

the tables for comparison purposes are some typical currents that were obtained in this laboratory with a conventional sputter source. With a few exceptions, the latter are frequently an order of magnitude less than those from the new source. The exceptions are interesting and these are discussed in some more detail below.

Lithium

Most of our early attempts were frustrated by melting of the sputter target and only after unusual care had been taken to ensure excellent thermal contact between the pressed lithium pellet and the freon cooled cathode stem did we have some success. However, the success proved short-lived and on several occasions we were frustrated to observe a Li^- current that rapidly grew to between 1 and 2 μA only to slowly fall and eventually stabilize at about 0.1 μA. Although this behavior is not fully understood it is thought to be a result of the extremely low partial pressure of oxygen in the source chamber. Previous experience with lithium in a conventional sputter source strongly supports the belief that an oxygen layer on a lithium sputter surface greatly enhances negative ion formation.

Beryllium (Metal)

Several attempts with a beryllium metal cathode followed the same pattern -- initially the $^9Be^-$ current steadily increased to between 0.1 to 0.2 μA only to gradually fall to the point where it could no longer be measured. The decline in the negative ion current was accompanied by a corresponding fall in the cathode current strongly suggesting that the ionizer was not properly functioning. It is surmised that sputtered beryllium metal, because of its high melting point, condensed on the ionizer surface and owing to its low work function (~ 3.2 eV) reduced the work function of the ionizer surface until it ceased to function. If this explanation is correct then it appears likely that the source will not function efficiently with other elements having unusually low work functions and elevated melting points. Fortunately such elements are very much in a minority.

Lead

Several attempts were made to generate negative ions from a lead cathode. In all cases the yield was disappointingly low -- rarely exceeding 50 nA for all isotopes. No reason is known for this failure but it is interesting to note that the electron affinity of lead has recently been revised from 1.1 eV, as previously reported, down to 0.365 eV. (Private communication, W.C. Lineberger)

Table I

Ion	i^-(μA)	Cathode	i^-(μA) (typical)	i^-(μA) UNIS
H^-	40-80	3 mm diam. × 3 mm long Ti + 28 cc of H_2 (H/Ti ~ 1.2)	45	4
D^-	30-60	As above (D/Ti ~ 1.0)	35	4
Li^-	0.1-2.0	Li metal in copper	0.1	2.5
$^9Be^-$	0.01-0.15	Metal	0.01	0.02
$^9BeO^-$	3	BeO pressed into 1 mm × 1.6 mm hole in Mo (~3.5 mg of BeO)	3	N.M.
$^{11}B^-$	1-1.8	Sintered natural boron (~70% theoretical density)	1.8	2.0
	1.5-2.0	LaB_6	2.0	N.M.
	7-9	60-mesh natural boron + Al powder pressed into Cu	8.0	N.M.
$^{12}C^-$	50-150	3 mm diam. × 3 mm long graphite pressed into Cu	60	20
$^{27}Al^-$	1-1.5	Aluminum metal	1.2	0.05
$^{27}Al_2^-$	10-15	Aluminum metal	12	1.0
$^{28}Si^-$	20-40	3 mm diam. × 3 mm long Si pressed into Cu	30	15
$^{48}Ti^-$	N.M.	Pure titanium	N.M.	N.M.
$^{48}TiH^-$	4-5.5	Ti + H_2 (see Hydrogen)	4	0.5
$^{52}Cr^-$	0.25-0.50	Chromium chip pressed into Cu	0.35	0.018
$^{56}Fe^-$	1.5-3.0	Pure iron	2.0	0.050
$^{59}Co^-$	10-23	Pure cobalt	15	0.9
$^{58}Ni^-$	30-60	Pure nickel	40	5
$^{63}Cu^-$	30-60	Pure copper	40	3
$^{74}Ge^-$	6-10	Germanium crystal 5 mm × 2 mm pressed into aluminum	7	1.1
$^{107}Ag^-$	10-20	Pure silver	15	0.08
W^-(all isotopes)	3-5	Sintered tungsten (70% theoretical density), also 0.5 mm thick sheet soldered to copper	4	0.1
Pt^-(all isotopes)	10-40	Natural Pt nugget pressed into Al	30	3.5
$^{197}Au^-$	30-60	Pure gold bead (~3 mm diam.) pressed into aluminum	50	10
Pb^-(all isotopes)	0.005-0.080	Pure lead bead (~3 mm diam.) pressed into aluminum	0.05	0.11

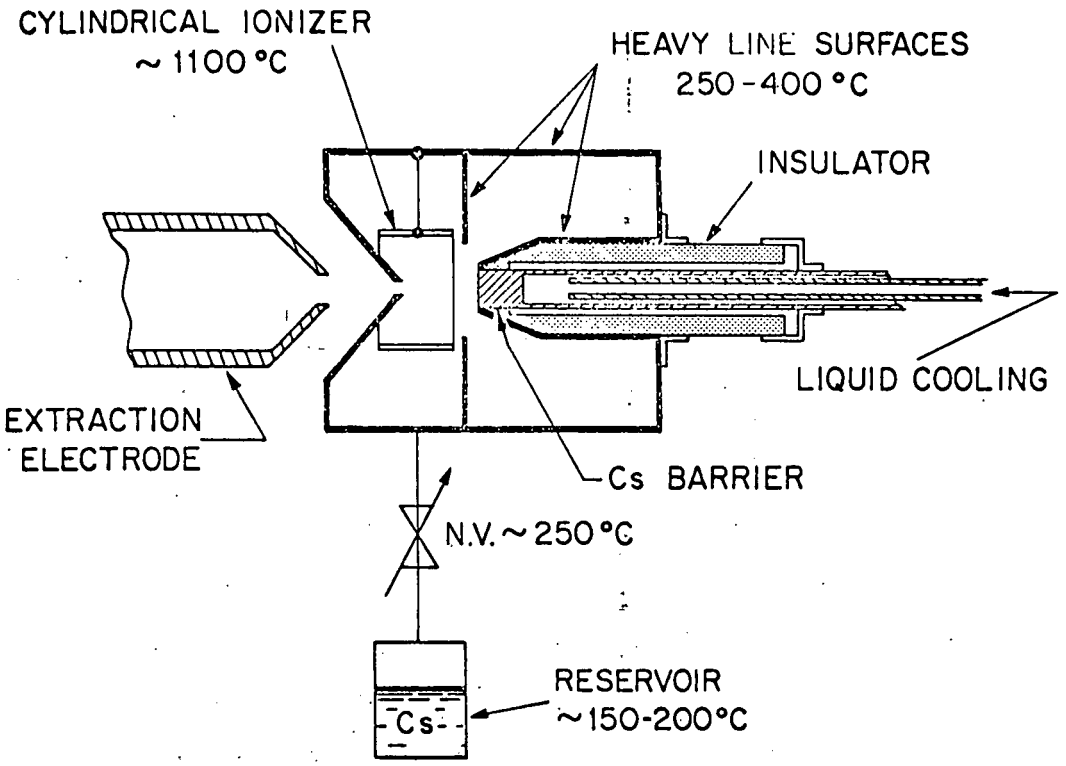
CYLINDRICAL IONIZER
~ 1100 °C
HEAVY LINE SURFACES
250 - 400 °C
INSULATOR
LIQUID COOLING
EXTRACTION
ELECTRODE
Cs BARRIER
N.V. ~ 250 °C
RESERVOIR
~ 150-200 °C
Cs

Fig. 1.

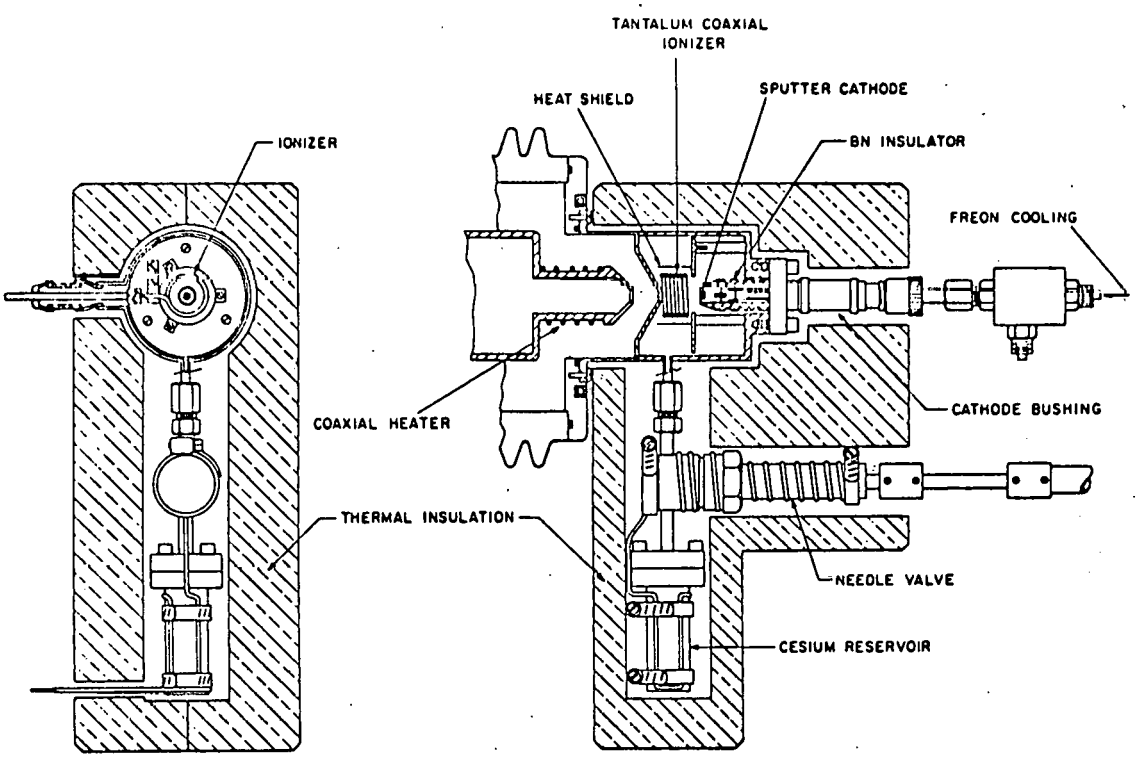
TANTALUM COAXIAL
IONIZER
HEAT SHIELD
SPUTTER CATHODE
IONIZER
BN INSULATOR
FREON COOLING
COAXIAL HEATER
CATHODE BUSHING
THERMAL INSULATION
NEEDLE VALVE
CESIUM RESERVOIR

Fig. 2.

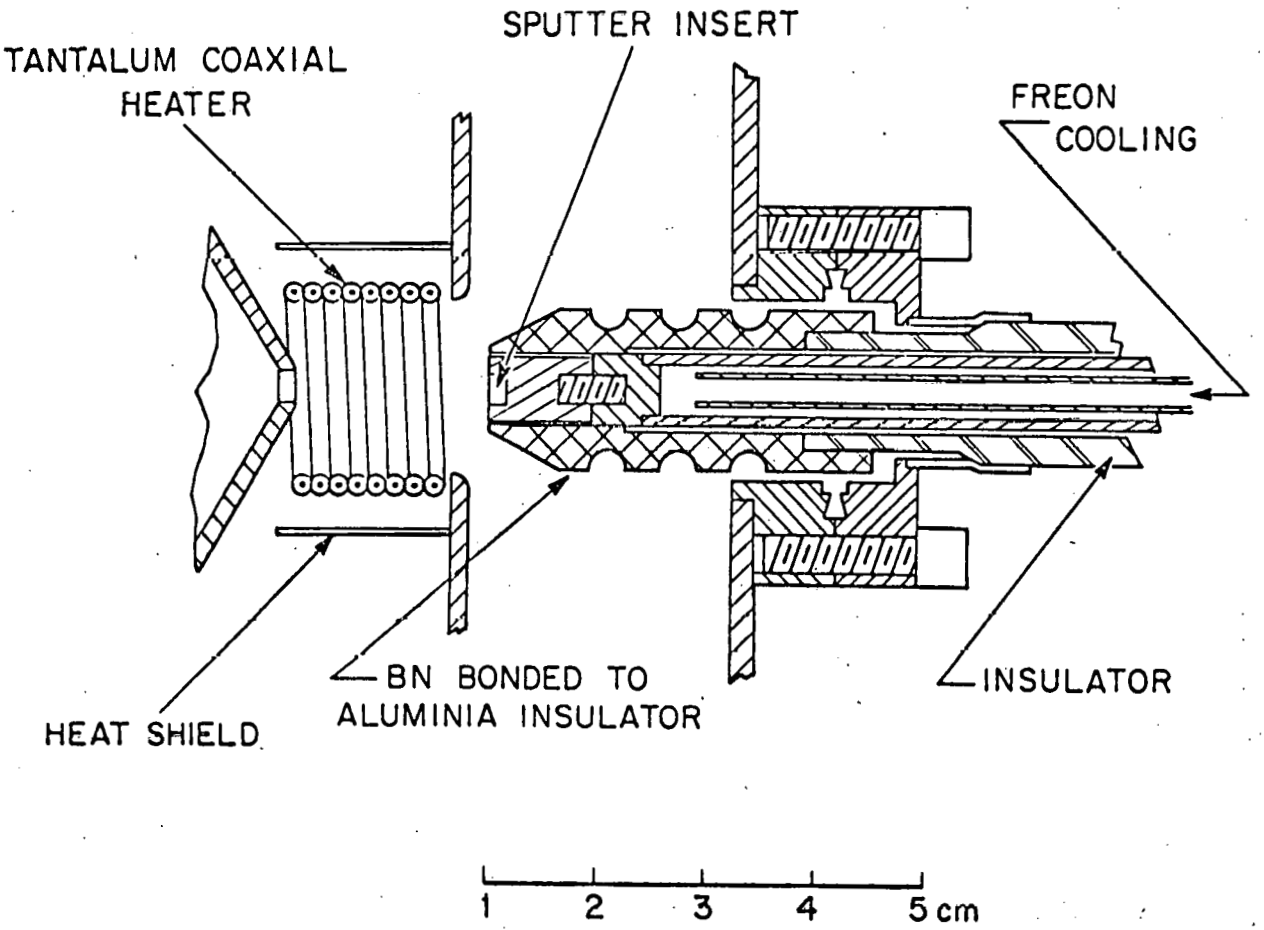
SPUTTER INSERT
TANTALUM COAXIAL HEATER
FREON COOLING
BN BONDED TO ALUMINIA INSULATOR
INSULATOR
HEAT SHIELD
1
2
3
4
5 cm

Fig. 3.

Larson: Do you attribute the poisoning of the ionizer from substances such as beryllium to the neutrals coming back from the cathode surface?

Middleton: Well, Be has a work function that is about 3.6 eV if I remember correctly...

Larson: Is the source of the material a neutral source or a charged source?

Middleton: You just can't ionize cesium with an ionization potential of 3.8 eV with a surface that has a work function less than 3.8 eV.

Larson: That's not the point of my question. Let me go on with a comment then.

Middleton: Maybe I misunderstood you.

Larson: It seems possible since the ions leaving the ionizer are electrostatically controlled in their paths to put a barrier between the cathode and the ionizer to mask off neutrals and yet pass the ions from the ionizer to the cathode.

Middleton: No, I think you missed a point, Dan. You cease to get ions altogether once the surface of the ionizer has been poisoned.

Larson: My point is to keep the poison off the surface of the ionizer.

Middleton: There's only one way that I know of doing that. And that is, in fact like the very first source that we built which had an annular ionizer in back of the front surface of the sputter cathode. Indeed, a number of electron evaporators use a very similar principal where one has a source of electrons in the form of an annulus. One mounts some sort of crucible with the evaporant in it, and one can, with proper geometric shapes, really get a very good focus of the electron beam. To my mind this is about the only way that one can begin to solve this problem. But even then you're only going to get a partial solution because evaporated particles don't condense with 100% certainty. They often bounce around and you're bound to get some secondaries back to the ionizer.

McKay: Roy, what is that coaxial heater made of?

Middleton: Well, it's exactly what is sounds like. It's like a heating element for a stove. It is just a tube of tantalum a little over 1/16 inch diameter with a 0.010-inch wall thickness. You can buy it with either magnesium oxide or aluminum oxide as insulation, and there is a 0.020-inch diameter tantalum wire in the middle. The whole thing is swaged down. It took me three years to find a supplier of it. If you're interested the supplier is a company called Temptron in California. I think they specialize in making high-temperature thermocouples for jet engines. Charlie may have the address.* It is very much like the Amperex heaters that are made out of I think an Inconel material. But this one has a tantalum outer and a tantalum inner piece.

Storm: How stable is the Cs ion current?

Middleton: Well, we now have zero problems with stability, but there are lots of tricks. That simple diagram I showed you really contains an awful amount of information. It is imperative that temperature gradients be correct, that one should use a needle valve. Then really we have just no problems whatsoever. In fact it seems as if one can introduce a certain amount of cesium into the source, and once that cesium is present, one can close the needle valve and the source will run for hours and hours without any additional cesium. The Cs plays the role of catalyst. Other than the Cs that escapes through the extraction aperture, there is no reason why the rest can't be used over and over again. And that leakage at these low pressures is really quite small. I think Jim will bear me out; the SNICS source shows similar characteristics.

Storm: The current then depends on the cesium density?

Middleton: The current depends on a combination of Cs vapor pressure in the source and, of course, on the temperature of the ionizer. At last year's SNEAP meeting I talked a little bit about a theory we had that I still think is partially correct even if it isn't totally correct. We're running in a very saturated mode. Whereas in a normal surface ionization source one is very careful to keep the Cs level below the 1% monolayer on the ionizer, we are woking in a totally different part of the curve. In our case a very large fraction of the Cs that leaves the ionizer probably leaves as neutral cesium. We don't really care about efficient operation. It's very different from the normal surface ionization source where if you go a little bit high on the Cs, you get Cs everywhere and all hell breaks loose.

* Charlie Adams has kindly supplied the following information regarding the coaxial heaters. The Ta heaters with dimensions given above are available for approximately $20./foot from Temptron Engineering, 7823 Deering, Canoga Park, CA 91304, Telephone: (213) 346-4900. Mr. Ed Skei is the person to contact.

ADDITIONS TO THE MP NEGATIVE ION INJECTOR

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University of Rochester

INTRODUCTION

An addition to the negative ion injector for the MP tandem Van de Graaff accelerator at the University of Rochester is presently under construction. This injector addition has been designed to extend the work done at Rochester in using the tandem accelerator system as an ultra-sensitive mass spectrometer. Also, the new features of this ion source injector will provide improvements in the heavy ion capability of the machine for all of the other aspects of the nuclear physics research program at the laboratory. The main features of this addition include the following:

1. New support table
2. High resolution 90° inflection magnet
3. Redesigned sputter ion source
4. Computer control of various parameters
5. Sodium charge exchange canal capability
6. Faster isotope switching
7. Improved vacuum

This injector addition will be operated from the existing 150-kV pre-acceleration power supply and will require modifying the protective cage around the existing injector, but will not require any modification or addition to the building. The overall layout of this addition is shown in Fig. 1. The various features of the new injector will be described in some detail in the following sections.

BEAM OPTICS

The major element in the beam optics is a 90° inflection magnet which has been designed to have a mass resolution of about 1 part in 200 while accepting all of the beam from the ion source. The beam from the source is focussed with a 5-inch diameter gridded einzel lens to the object point of the inflection magnet. At the object focal point, an aperture is installed. This aperture can be easily changed, in order to alter the magnet resolution for a particular experiment, and also, enough space has been reserved at this location so that insulated and/or adjustable slits can be installed in the future. This same comment is true for the image point, following the magnet. Initial operation will be apertures based on the size of the beam calculated with a beam optics program. A set of electrostatic steerers follows the image aperture, and then a biased Faraday cup.

Following the Faraday cup is another set of steerers and an einzel lens to focus the beam at the end of the preacceleration tube, which is the location of the image focus of the standard MP inflection magnet. The overall optics has been designed to match the beam phase space at this point to that of the beam from the original source locations.

The main motivation for constructing this addition is the program which uses the overall tandem accelerator as an ultrasensitive mass spectrometer. In order to use the machine in this way, it is necessary to cycle the mass setting of the injector to the various masses of interest. For carbon-14 measurements, the system cycles between masses 12, 13 and 14. For the highest accuracy, these cycles need to be repeated on a short, perhaps 10's of milliseconds, time scale. In order to achieve this switching time, the inflection magnet chamber has been insulated from ground. A variable voltage can be applied to the chamber in order to modulate the energy of the beam so that each of the desired masses comes through the inflection magnet at the same magnetic field setting. Only a few kilovolts are needed to achieve this switching.

ION SOURCE

A modified design of the Hiconex-type sputter ion source will be available for the new injector. The important changes in this new source are: (1) direct cesium beam on the "back" side of the sample holder, and (2) target positioner with vacuum lock. As shown in Fig. 1, the cesium beam is directed at the back side of the cone or the sample holder, rather than reflected through a hole in the sample holder as is the case with the standard Hiconex Model 834 source. The sample changer is a combination x-y manipulator and vacuum lock. With this geometry, the cesium beam can be adjusted with a lens/steerer combination built into the cesium gun, and then the target manipulator can be adjusted to maximize the beam, and/or consume the entire sample. Typical sample masses can be less than 10 mg, and especially if expensive separated isotopes are used, it is advantageous to utilize the entire sample. The vacuum lock that is part of the sample manipulator allows the samples to be changed without shutting down the cesium beam, and this will reduce the turn-around time when samples need to be changed. Usually, six sample holders will be installed at one time, but the design can accommodate other combinations or different sample holders.

CONTROL SYSTEM

One of the main features of this new injector is the microcomputer system for controlling the various power supplies. Primarily because of cost restrictions, an 8-bit Z-80 microprocessor is the processor being used. The computer system uses S-100 plug-in cords in order to utilize the wide variety of memory and interface cards that are readily available. A block diagram of the overall system is shown in Fig. 2. The power supplies used on the new injector are intended to be used with voltage input programming of typically 0 to 10 volts, and this feature simplifies the interfacing requirements. These power supplies also provide outputs for monitoring current and voltage which makes it easier to monitor the performance of the supplies with the microcomputer. Some of the existing

injector supplies, such as the inflection magnet 300-A power supply will have to be modified to be compatible with the computer control.

For the actual control of each power supply, we have chosen to use one fiber optic light pipe per channel. This system will minimize the amount of electronics which has to be operated at the high voltage potential, and will hopefully provide a reliable system.

A somewhat detailed diagram of an individual control channel is given in Fig. 3. The up/down counter circuitry is used to provide smooth continuous control of the digital-to-analog converter (DAC) and also to provide a certain degree of immunity from computer crashes. The data is latched in the up/down counters, and can only be changed a bit at a time. If a direct-parallel connection were made from the computer to the DAC, some additional complexity comes from handling 12 bits for the DAC with 8 bits at a time from the processor. The parallel connection could be taken care of with some appropriate software or hardware, but we've opted for the up/down counter interface. This decision was also tied in to the scheme for actually tuning the beam, as described below. The 12-bit DAC was chosen to provide adequate resolution of the controlled variable. For example, for the inflection magnet, if it is capable of a mass resolution of 1 in 200, then we need 5 to 10 times more resolution in order to have smooth control over beam tuning. The 12-bit DAC has a resolution of 1 in 4096 of full scale, but only 1 in 400 at 10% output. By having a programmable reference as indicated in Fig. 3, we can achieve a mass resolution better than 1 in 1000 over the power supply range. The 12-bit resolution is not really necessary on some parameters, but it is an advantage to have identical components in every channel. The DAC drives a voltage-to-frequency converter (VFC) to transmit the data to the power supply at the high voltage potential. Then only a frequency-to-voltage converter is necessary at the high voltage potential to operate the power supply.

To read out the power supply voltage and or current, the VFC circuits operate the other way around, and at the computer, the frequency is monitored by any one of several methods. A hardware counter can hold the data, or the computer can count pulses for a certain time, or the computer can measure the time between pulses. Each of these possibilities involves some consideration of hardware vs. software, and a combination of all three may actually be used, as appropriate.

A serial link, patterned after the CAMAC serial highway standard, connects the local microcomputer with the console microcomputer as shown in Fig. 2. At the control console, the Z-80 system has more RAM memory (32K to 48K bits) and also two 8-inch floppy disc drives for program storage, table storage, and program development. The compucolor display provides status information about the system performance. The control knobs operate 2 or 3 digital shaft encoders with serial output. Since these controls can have an infinite number of revolutions, the response of the computer can be programmed depending on the power supply of interest. Also, the serial pulses from the encoder lend themselves to the up/down counter interface as described above.

VACUUM

The ion source itself is pumped with a Welch Model 3133 turbo molecular pump which is connected on the bottom of the gridded einzel lens. At the Faraday cup after the inflection magnet an Air Products cryopump provides isolation of the new ion source vacuum from the existing injector vacuum system. The pumping system is designed to prevent ^{14}C produced in the accelerator from working its way back to the ion source and to provide a cleaner environment for the sample to minimize unwanted contaminants such as sulphur in ^{36}Cl and ^{32}Si experiments.

SUMMARY

All of the beam handling components have been delivered from General Ionex Corp. The hardware is being assembled in a corner of the accelerator room for preliminary tests. The schedule calls for some test beams from the source in another 6 to 8 weeks, and then the system will be installed inside the modified cage area around the beginning of next year.

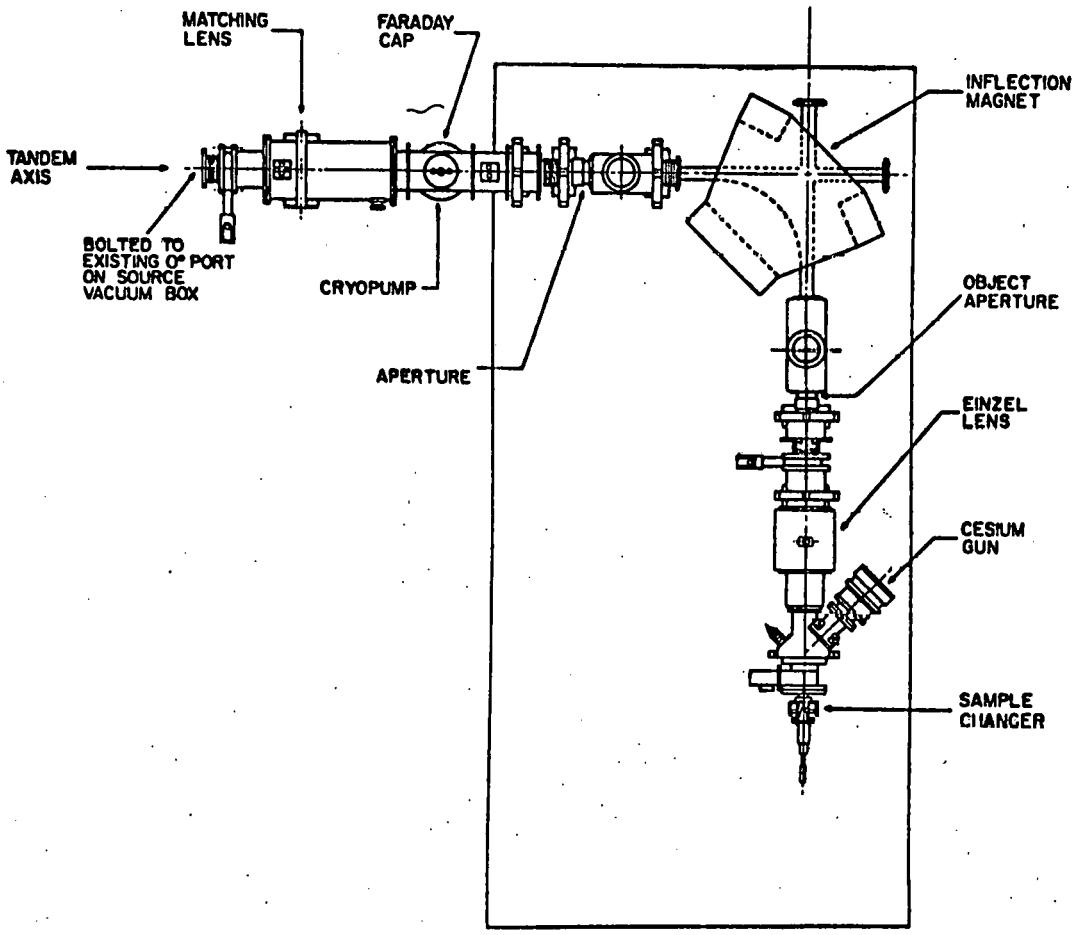

Fig. 1.

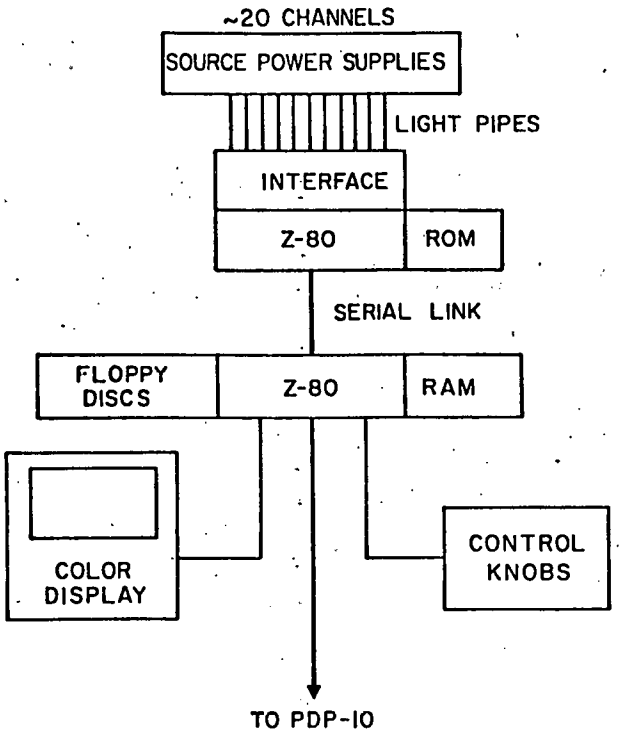
~20 CHANNELS
SOURCE POWER SUPPLIES
LIGHT PIPES
INTERFACE
Z-80
ROM
SERIAL LINK
FLOPPY DISCS
Z-80
RAM
COLOR DISPLAY
CONTROL KNOBS
TO PDP-10

Fig. 2.

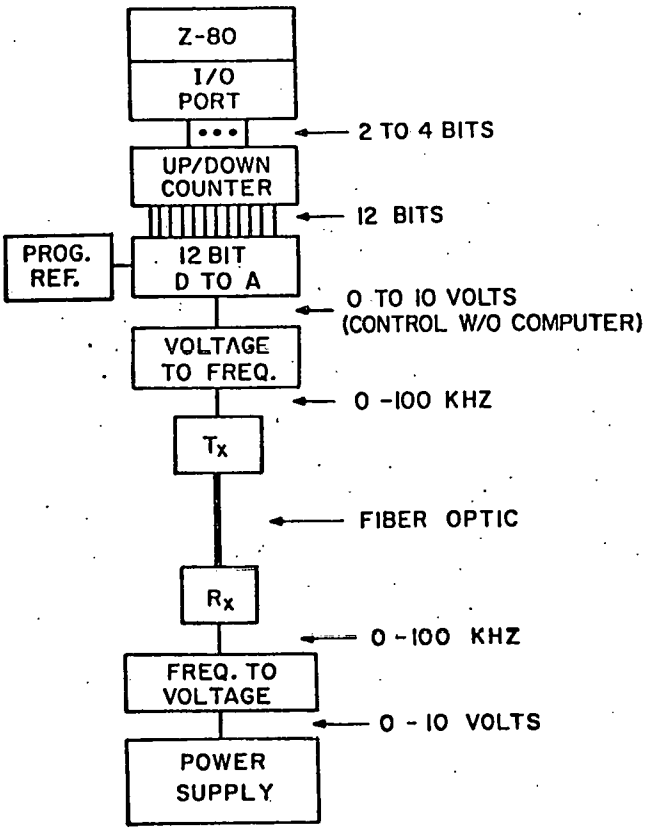
Z-80
I/O PORT
2 TO 4 BITS
UP/DOWN COUNTER
12 BITS
PROG. REF.
12 BIT D TO A
0 TO 10 VOLTS (CONTROL W/O COMPUTER)
VOLTAGE TO FREQ.
0 -100 KHZ
Tx
FIBER OPTIC
Rx
0 -100 KHZ
FREQ. TO VOLTAGE
0 - 10 VOLTS
POWER SUPPLY

Fig. 3.

Middleton: Could you say a little bit about the design of the 90° magnet? I understand this is a rather novel design with a third-order correction and with a very high mass resolution.

Lund: Yes, that's basically what I can say about the magnet. There's a special correction gap that's been machined into the pole piece in the middle of the 90-degree radius. So it does have higher-order corrections making it possible to achieve resolutions like 1 in 200.

Middleton: With reasonable solid angle?

Lund: Yes.

Middleton: Has this been tested?

Lund: We should ask Arnold Peterson that question. The answer is yes, they have run it at the factory, but we have not run a beam through this one yet. This is not the first one of these magnets that they have produced.

Storm: Who made the magnet?

Lund: General Ionex manufactured it themselves.

Noé: Our injector will have a 1 in 100 resolution magnet with about a 1.5-inch gap. I don't think it has the special features that deal with higher-order aberrations.

Allen: What spot size can you have if you want 1 in 200 resolution?

Lund: I can't quote a number off hand.

Allen: It looks like a rather small radius magnet, so I would guess that the spot size should be rather small to get that resolution.

Jones: The magnet doesn't have a homogeneous field. Is that correct?

Lund: That's right.

Jones: Can you tell us how it is shaped?

Lund: I really don't know the details of it, Charles, other than the fact that they've made this extra cut in the middle of the magnet to provide higher-order corrections on the fringing fields.

Middleton: I think it has essentially a uniform field.

Noé: General Ionex is making some magnets now that have alternating gradients which provides strong focussing effects.

SURVEY OF GAS MIXTURES USED IN ELECTROSTATIC ACCELERATORS

by

N. Burn

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Chalk River, Ontario

CANADA, K0J 1J0

Introduction

Early in July 1980 I received the SNEAP "First Announcement" together with the deceptively innocuous comment "We welcome your suggestions for topics to discuss". Being a long time aficionado of sulphur hexafluoride (SF_6) I was naturally prompted to suggest as a topic for discussion "Gas Mixtures - where it's at, 1980". Two months later when the official program arrived, I saw that I was being hailed as the keynote speaker! But seriously, knowing how much work is involved in organizing a meeting of this type, I was only too happy to assist Jim Billen.

I dutifully sent out survey forms to 30 laboratories using a list taken from the 1978 SNEAP Proceedings, then sat back waiting for the information to pour in. And pour in it did - there was a gratifying 86% return rate. At this point I would like to thank most sincerely all of those who participated in this survey. I'm sure you all noticed there were some relevant questions on the form, for example question 2 "Institution", and I'm pleased to report that almost everyone managed to get this one correct. I also learned some very interesting facts not even connected with insulating gas. I learned that one of our former SNEAP members had disappeared without trace. I also had a reply in French from Strasbourg with the cryptic comment "VIVE LA COURROIE" written across it!

Perhaps the most significant thing that I learned was something which I had long hypothesized on; this survey finally gave me the chance to prove it. In the bottom left hand corner of the survey form I had provided a space for the date. When I plotted these dates against the location of the laboratories I came up with the curve shown in Figure 1. Orsay, one of the laboratories furthest away from Chalk River returned the survey first and, as I realized when I started to compile data, I had not filled in my own survey, thus putting Chalk River last! The dashed vertical line at two weeks represents the minimum turn-around time for mailing. The dotted break in the curve is the delay that was caused by yet another in a continuing series of Great Canadian Mail Strikes.

Historical Background

In 1965, based on several years operating experience at Chalk River, Ashbaugh[1] wrote what we thought was a definitive paper on insulating gas - 100% SF_6. Eight years later in 1973, at the First International Conference on the Technology of Electrostatic Accelerators held in Daresbury, Ashbaugh[2] re-affirmed his belief in pure SF_6 and elaborated on the most effective ways of storing and handling the gas, along with appropriate safety precautions. At the same conference, Letournel[3] described voltage tests on the Strasbourg MP Tandem using pure SF_6.

In 1976, with the continuing accumulation of experience from many MP Tandem installations, Skorka[4] at the International Conference on Tandems held in Trieste wrote "Pure SF_6 will be standard in future for all larger installations". This statement came as a result of direct comparisons at Munich between pure SF_6 and a 30% mixture and where a dramatic reduction of resistor failures was observed with the pure gas even at the higher terminal voltages attainable. Such was the state of affairs on insulating gas, that the topic received no mention whatsoever at the Second International

Conference on Electrostatic Accelerator Technology held in Strasbourg in 1977.

Later that same year, however, at the 1977 SNEAP Meeting held in Los Alamos, Sato[5] presented a paper indicating that a four component mixture was superior to pure SF_6. At the 1978 SNEAP Meeting in Oak Ridge there were two more papers also questioning the superiority of pure SF_6. Broadhurst[6] had made small scale tests on a series of gases including Minnesota and Yale mixtures; Wegner[7] had carried out full-scale tests at Brookhaven in MP-7. Both reports indicated that pure SF_6 was perhaps not the optimum gas to use. Which prompted me to ask the question "Gas Mixtures - where it's at, 1980".

Survey Results

Survey forms were sent out to 30 laboratories; there were 26 returns listing a total of 31 accelerators. A comprehensive listing by accelerator type giving gas mixture details, pressure, moisture and losses is given in Table 1. The accelerators were arbitrarily listed in order of ascending terminal voltage; this method of listing also provides a convenient grouping of accelerators such as FN's and MP's. From the table we can make the following generalizations about the group of accelerators surveyed:-

1. 55% use pure (or nominally pure) SF_6.
2. 78% use SF_6 as one component of the insulating gas.
3. 50% keep the moisture level below 20 ppm (only one laboratory does not use an activated alumina recirculator/drying system).
4. No one has a purification system to remove air.
5. Reported annual losses vary from 1% up to 100%.

Discussion

It is interesting that eight out of the 17 users of "pure" SF_6 indicate an air contamination anywhere from 1% up to as much as 20%. What is more interesting is that not one single laboratory has a purification system to remove air. It is obviously felt that the deleterious effect of air is not significant enough to warrant the capital cost of building a purification system - or perhaps it is just that a large scale, efficient, economical purification system has not been developed to date although several attempts have been made.[8) 9) 10)] Perhaps, since in most facilities clean-up need only be done once every few years, it might be feasible for one of the manufacturers of SF_6 to build a mobile purification system that could be leased or rented to interested laboratories.

Only one laboratory reports using SF_6 in a binary mixture (SF_6/N_2: 60/40). In tertiary mixtures excluding "pure" SF_6 with air contamination, the concentration of SF_6 is generally between 40% and 46% although one laboratory uses 20% and another only 10%. Quaternary mixtures appear to be tertiaries with air contamination providing the fourth component, oxygen.

Reported losses vary tremendously, but in general the larger the accelerator, the lower the losses. Losses are usually, but not invariably lower when using SF_6 either pure or as a component of the gas mixture. Obviously the loss figure should not be considered without taking into consideration the total gas inventory.

I would like to conclude by again thanking all of those who took part in the survey. I hope that this presentation and in particular Table 1, will provide a meaningful background for the remainder of this session.

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4. "Tandem Accelerators". S.J. Skorka, International Conference on the Physics of Tandems, Trieste (1976)

5. "Microscopic Accelerator Physics". K. Sato, Proceedings of the Symposium of North Eastern Accelerator Personnel, Los Alamos (1977) 139

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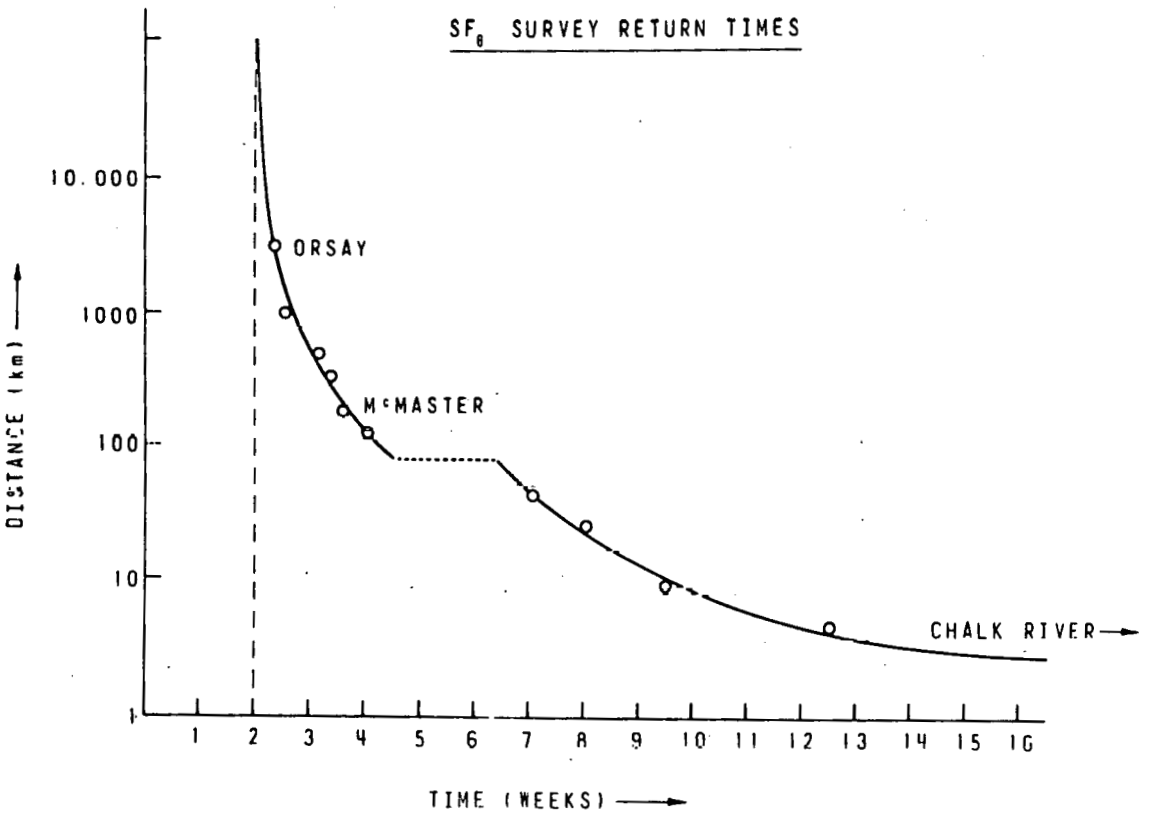

Figure 1.

TABLE 1

Accelerator Type	Maximum Voltage (MV)	Gas Mixture (%) SF_6	N_2	O_2	CO_2	Operating Pressure (psig)	Moisture (ppm)	Losses %
JN	1.2		80		20	200	200	12
2 UH	2.0	* 100				100	--	100
2 UDH	2.0	* 100				90	--	10
AK	2.0		75		25	380	100	200
K	3.0	* 87 + Air				80	<20	11
K	4.0	* 100				70	75	5
K	4.0	20	60		20	260	30	14
Dynamitron	4.0	* 100				90	50	1
Dynamitron	4.2	* 80 + Air				90	--	25
Notre Dame 3	4.8		80		20	100	30	100
EN	6.2	* 100				60	50	1
EN	7.0	* 100				70	20	10
CN	6.2	* 99 + Air				85	<20	16
CN	7.5	10	72		18	180	<20	20
Los Alamos	8.0		100			200	200	--
FN	9.0	* 80 + Air				60	<20	17
FN	9.0	* 87 + Air				80	<20	11
FN	9.0		80		20	185	<20	12
FN	9.0		80		20	225	<20	100
FN	9.2	* 98 + Air				90	500	43
FN	9.6		80		20	200	30	100
FN	9.7	60	40			90	<20	9
MP	-9.5	46	43	5	6	140	<20	1
MP	12.0	38	51		11	160	<20	5
MP	12.5	* 96 + Air				85	<20	5
MP	12.7	40	55	3	2	135	<20	3
MP	13.0	* 100				90	<20	5
MP	13.0	* 85 + Air				80	<20	5
MP	14.1	46	43	5	6	140	<20	1
MP	16.0	* 91 + Air				115	60	5
25 URC	25.0	* 100				85	<20	1

* "Pure" SF_6 - 8 out of 17 users indicate air contamination of 1% to 20%

Saylor: Please tell us what the loss percentage is again?

Burn: Yes, the loss percentage which is generally reported either in cylinders or pounds is listed here as a percentage of the total inventory. It is the loss per year.

Noé: One figure in the moisture content column stands out, and that is an FN with pure SF_6 reporting 500 ppm which is exceptionally wet. Do you have a comment on that? Is that deliberate or inadvertent?

Burn: Perhaps the FN user will stand up and identify himself. I can't comment on that. It was simply reported on the survey form.

Den Hartog: I think that might be us but I'm not sure because I did not fill out the form. We do not measure the water content and we are the one lab listed that does not recirculate SF_6. The 98% plus air notation is interesting because according to my last gas analysis that is not correct. Our air contamination is very small, probably much less than 1%. But that does not agree with what we know to be our experience with inadvertent contamination, so something in the system must be gettering the oxygen and nitrogen.

Burn: Maybe we can have some comments on that. The observed ratio of oxygen to nitrogen in machines with air contamination isn't always the same as it is in atmospheric air. We did some tests at Chalk River on air contamination. I think we have a very high oxygen contamination. One would expect that the oxygen would be used up, but in fact the ratio was richer in oxygen.

Fauska: I noticed that even though you mentioned that somebody used Freon in the machine, I don't see that on the survey form. Also, why has no one tried pure Freon 12, for example?

Burn: Yes, that is the Los Alamos machine that uses two components: nitrogen and Freon 12.

J. Tesmer: The numbers listed here are for the outer region of the machine which we never go into. The inner region is the one that has the Freon 12.

Letournel: About the moisture content, the last MP listed I think is the Strasbourg machine. It says 60 but it is actually 40 ppm. We run with SF_6 contaminated with close to 10% of air. We use an empirical formula to determine the partial pressure of SF_6 needed in MP-10. The formula is

$$P = \frac{2}{3}(V - 6),$$

where P is the pressure in kg/cm^2 and V is the terminal voltage in MV. This comes from an examination of the curve for our past operating experience.

Pohl: Does anyone actually know whether this air contamination harms your maximum voltage? Does a 5% air contamination degrade your performance 5%?

Burn: It degrades the performance somewhat, but air does have dielectric strength. I think that was the comment that Michel was making. Were you subtracting the air percentage completely, Michel?

Letournel: We calculate first the amount of SF_6 we need. We know that we have this 10% contamination, so we add more gas to get the right amount of SF_6.

Burn: So you assume that the air contamination does nothing as far as insulating properties are concerned.

Letournel: Yes.

Rowton: We had an opportunity to change all of our tank gas. We collected enough sulfer hex to make a total change so we were certain that the gas in the tank was pure. The gas we took out according to spectrographic analysis was approximately 50% air. I was really expecting a gross change, but there was very little if any change noticeable in the operating potential of the machine.

Larson: I'd like to make two comments. Even 5% of oxygen at 10 atmospheres ought to support combustion reasonably well. A 50% air mix in the accelerator sounds fairly dangerous. Perhaps some of you know that air was used in acclerators until some accidents and fires convinced people that it was not a good thing to do. I'd be a little bit cautious about combustible materials in accelerators that have that much air. Secondly, with regard to Pat's comment on gettering, it seems entirely possible to burn up the air including the nitrogen since nitrogen is reasonably active chemically, by putting some material (probably a solid) into the circulating loops or into the tank itself and then pull it out. You would need, perhaps, some kind of mechanism for replacing cartridges.

Burn: I think at Canberra they did this by allowing their storage tanks to rust.

Larson: There are perhaps more controlled ways to do this.

Burn: I think that once you start getting a high proportion of oxygen in the mixture people should start worrying about combustible materials and the danger of fire. That's a very valid comment, Dan.

Chapman: If you look at the FN machines and you ignore the two with 100% losses, the leak rate for all of them is something like 10% per year. That represents an expenditure of about $4000. For all of the MP machines the losses are much nearer 4%. I wonder if there is some characteristic of the FN tank that makes it particularly vulnerable to losses. We've had some problems with door gasket seals. We are in fact bothered by that 10% loss a year.

Burn: Does anyone else have any comment on the losses?

Noé: We've been troubled with losses too, and they seem to be almost entirely when pumping the machine out, especially with a compressor. There are measurable losses from door seals and the like, but we don't believe that they amount to a significant part of the loss.

Burn: With the MP at Chalk River each time we pressurize we go over all the door seals that have been open with a SF_6 detector looking for leaks. Presumably other people do the same thing.

Weitkamp: At Washington we have a leak on the gasket on the compression shaft that puts compression on the column. The only way I can figure out to fix that is to take the whole machine apart.

Chapman: At Florida State we have a leak in the same place.

Burn: It sounds like a leak you're going to have to live with for awhile.

Jones: I'm not sure that Michael's question ever got answered. I'm not aware of any evidence that small concentrations of air have a deleterious effect on voltage holding capability.

Burn: Our indication at Chalk River is that we have an air contamination of about 15% and we still run up to about 85 psi at 13 MV. If you take into account the partial pressure of the SF_6 then you would expect that you would need more gas than that.

Jones: The problem with air contamination is in storing it.

Burn: I think the main problem, as Charles just commented, is storage. If one uses liquid storage and the air contamination builds up, then one is unable to get all of the gas back into the storage volume.

Mastroianni: To answer the question about air affecting the insulating quality of the gas: one of the possible uses of SF_6 is for gas-filled transmission lines. These are high-voltage transmission lines in the form of a coaxial cable that run in the range 178 to 1200 kV. The gas in the bus sees the same kind of electric fields that you have in your application. Your voltages are higher but your gaps are higher so the electric fields are comparable. Even with 50-60% air with SF_6 in a transmission line there is no fall off in the 60-Hz breakdown voltage. There's really little effect. I have another comment on the subject of burning. A major use of SF_6 is in high-voltage circuit breakers. A high-voltage circuit breaker is basically a fuse in the transmission system that absorbs energy in a switching surge, rather than have the surge hit the transformers and blow them up. The SF_6 circuit breaker is initially in the closed position and as the surge comes in the contacts break apart. Some of these breakers are rated at 50 kA and the interuption can occur in 2 or 3 cycles. Ostensibly the SF_6 is pure, but sometimes they get as much as 30 or 40% air in them. In that kind of arc you get decomposition but you don't get runaway burning.

Larson: I may have said something wrong. I hope I said it could support combustion. Do people put combustible materials in those circuit breakers? I doubt it.

Mastroianni: Some of the epoxy insulators are combustible. If there is a flashover involving any solid support material containing carbon, you can see some involvement, but it is not a runaway reaction. The SF_6 that's left will ultimately extinguish the arc.

Den Hartog: Everybody else seems to be very concerned about the moisture content. Is there evidence that this is important?

Burn: I think the cause for concern there is perhaps that the SF_6 breakdown products react with moisture to cause deterioration of materials inside the accelerator. One cause for concern is the nylon links in the Pelletron chains. There's some evidence that breakdown products may affect the strength of the nylon.

Den Hartog: Our Pelletron has been in operation for six years now with no breaks.

Mastroianni: There has been a lot of work on the effects of moisture content on breakdown voltage. The breakdown voltage will be affected very adversely if there is moisture present in the form of water in the condensed phase. There was a recent paper this summer in Boston by some people from Switzerland that showed that the dielectric strength of SF_6 both for 60-Hz systems and for switching systems started to deteriorate when the moisture content approached 50% relative humidity.

Burn: I think another major concern for low moisture content is that systems that run with a belt give poor performance with high moisture content.

Insulating Gas Fast Transfer System

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1- Introduction

At the Université de Montréal, we have been working for the past five years on various problems related to SF_6. We succeeded in purifying to better than 99.9% the SF_6 of our EN tandem, which contained 35% air, and of our Dynamitron, which was contaminated with 10% air. This separation was accomplished at the rate of 1 cylinder per day, using a simple device [1,2] which is inexpensive to build and to operate. In 1980, we have also investigated the feasibility of storing [3] SF_6 adsorbed in activated charcoal, instead of as liquid or a pressurized gas.

The SF_6 transfer system which we are reporting herein has been in successful operation for more than two years in its present form. Taking into consideration a possible risk of damage to the accelerators from the unusually fast rate of transfer or from an oil contamination, we have delayed the publication of the method. After two years, we feel that the method is safe, and we hope that the experience of designing and of operating the fast transfer system can be of use to other laboratories.

The layout of the transfer system is straightforward, as can be seen in Fig. I; a similar design for a single machine would be even simpler. To achieve a rapid transfer system at low cost, two special features have been introduced: the use of refrigeration compressors and the operation of Roots blowers at high pressure. In addition, we shall discuss the control system, which is unusual.

2- The use of refrigeration compressors

Most of the insulating gas transfer systems for accelerators are equipped with unlubricated compressors which use teflon sealed pistons, and our previous transfer system was no exception. Because of substantial leakage around the piston seal, this compressor was changed initially for a Corblin diaphragm compressor. Diaphragm compressors are ideal in this application, being unlubricated and perfectly sealed; unfortunately, ours was much too slow, with a 6-hour transfer time. Since we could not afford the two larger diaphragm compressors ($75,000. in 1975) which were required to bring the transfer time to a reasonable 1 1/2 hour, we searched for alternatives.

We eventually installed a pair of standard Tecumseh Model "M" (previously "SA") compressors, in parallel. These are standard (i.e. outside motor), three cylinder, oil lubricated, refrigeration compressors. They were available off-the shelf in Montréal ($2500. each in 1976). The

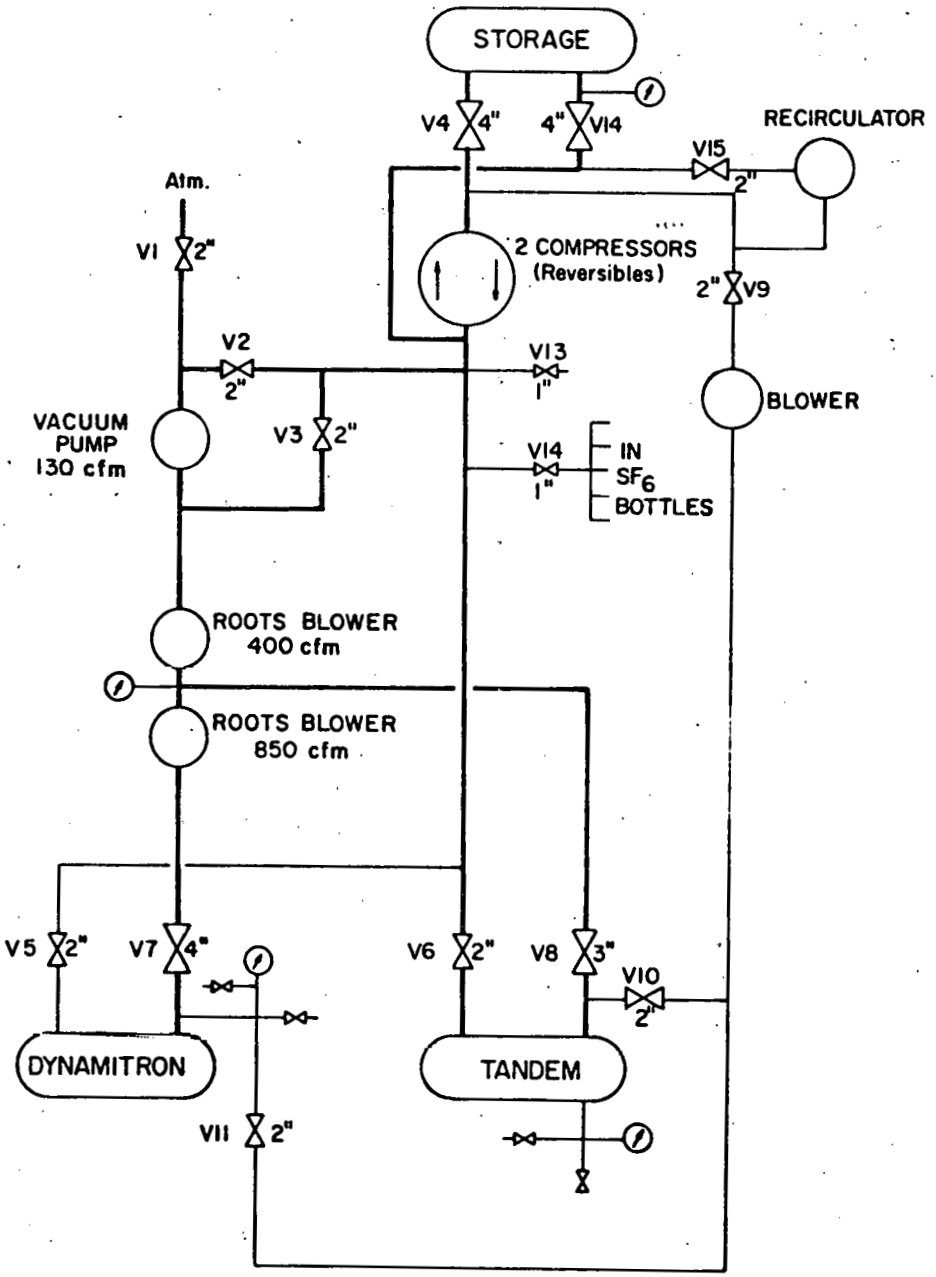


Fig. 1. Fast SF_6 transfer system.

modifications to the compressors were the following: 1) we use vacuum pump oil instead of the recommended refrigeration oil 2) an oil filter has been installed on the line which feeds the bearings 3) a water-cooled copper plate has been bolted on top of the compressor head.

In addition, it is necessary to make sure that the large quantity of oil which flows with the gas at the compressor exhaust returns to the crankcase. In the last three years, this has been achieved by ordinary refrigeration oil separators. It remains the weak point in our system, since we have to add oil in the compressors after 10 transfer cycles; we are presently investigating more efficient coalescing filters. The oil which is lost from the compressors does not reach the accelerators; it is trapped very efficiently in a home-made activated charcoal filter (volume: 17 liter), which is changed periodically. As a final element in the system, we installed a dust filter.

The only serious problem which we encountered was a very strong vibration which developped only when we ran both compressors simultaneously. We solved the problem by using pulleys of slightly different diameters on the 15 HP motors, to insure that the compressors could not synchronize.

3- Operating Roots blowers at high pressure

Roots blowers are the pump with the fastest throughput at a given price. Unfortunately, their use is normally limited to low pressures. This limitation comes from two different effects: firstly, from the heat load on the rotors, and secondly, from the increasing torque at high pressure.

The heat load effect is easily understood, since the rotors have very poor thermal contact with the case of the blower. The heat load is proportional to the throughput (in mass per unit of time) and increases with the compression ratio. At very low intake pressures (i.e. 10^{-5} torr), the throughput is negligible and the heat load remains reasonable in spite of the very high compression ratio. Cooling is achieved through the driving gears, and also from the gas which flows back between the rotors and the case; at these low pressures, the mean-free-path of the gas molecules is larger than the clearance between the rotors and the case, so that heat can be transferred from the rotors to the case by successive collisions. As long as we remain in the molecular regime, this gas cooling increases proportionally with throughput and the compression ratio can be maintained.

When the pressure increases to the transition region and we eventually reach turbulent flow, the heat load continues to increase with throughput while the cooling levels off. Therefore, in principle, the compression ratio must be reduced.

We realized, however, that this limitation strictly applies to

continuous operation; it was calculated from the compression energy involved and from the thermal mass of the rotors that we could not obtain a dangerous temperature rise during a single transfer. To guard against the possibility of overheating in situations of rapid successive transfers, we installed a temperature sensor in the gas flow at the exhaust of the Roots blower. This sensor interlocks the control system as described below.

The second limitation of increasing torque cannot be completely overcome unless a special Roots blower, with stronger shafts, bearings and gears is built for this purpose. Since we had a spare 10 HP DC motor in the laboratory, we used it to drive the blower in a torque-limited configuration. This motor is simply fed from a 6-diode 3-phase bridge rectifier connected directly to the 550 volt power mains, with a limiting resistor in series. For this resistor, we used water-cooled heating elements. A variable speed AC motor control would perform better in this application, but it was not available.

4- The Control System

It was felt rather strongly that the operation of the new transfer system should be made as practical and as safe as possible. Therefore, we wanted the possibility of introducing various interlocks in the system, and of retaining full control in case of power failure.

These considerations led to the construction of a rather sophisticated control system which has performed very reliably. The system consists of three control consoles; the main panel is located near the compressors and two remote stations permit local operation at the tandem and at the Dynamitron. Pneumatic logic was used to implement the system. Information is transferred between panels and to the SF_6 valves through pressurized copper tubes. Air pressure also runs a Haskel booster which supplies 750 psi of hydraulic pressure to the hydraulic cylinders which actuate the SF_6 valves.

The pneumatic logic was preferred over the much simpler electronic circuit because of the requirement of maintaining control of the system's valves during power failures. We believe that a local pressurized air reservoir constitutes a much more reliable form of energy storage than lead-acid or NiCd batteries.

5- Overall system performance

With the new system described above, the SF_6 transfer time, from the 1200 cu. ft. Dynamitron to the reservoir has been reduced to a total of 50 to 70 minutes depending on the initial pressure (60 to 90 psig). The system has been performing very reliably over the last two years, without any damage to the accelerators.

The SF_6 loss from normal operation is totally negligible, because of the low SF_6 pressures attained at pump down and because the

system is completely sealed, including the compressors. Finally, the cost of building this system was a small fraction of the cost of building a conventional system with a comparable transfer speed.

1) C. BRASSARD, "The Separation of Gas Mixtures Containing Sulfur Hexafluoride", SNEAP, Tallahassee, FLA (1975)
2) C. BRASSARD, "The Purification of SF_6 in a Constant Temperature Adsorption Process" Revue de Physique Appliquée 12 (1977) 1423
3) C. BRASSARD, to be published in Nuclear Instruments and Methods

Burn: What kind of oil separator do you use?

Brassard: At this time we use normal refrigeration-type oil separators. They cost a couple hundred dollars. But they put too much of a burden on the oil filter which is activated charcoal, so we will probably replace it with a larger unit. We can do about ten transfers without damaging the charcoal at present. We would like of course to do perhaps 20 or 100 transfers.

McKay: How long do you run the roots blower in this overloaded mode?

Brassard: Not more than about five minutes.

Pohl: I have a question about something that you really didn't go into. You briefly mentioned your absorption of SF_6 on charcoal. Was that designed to clean up the SF_6 or to reduce the volume occupied by the SF_6?

Brassard: In the storage system?

Pohl: Right.

Brassard: Yes, this reduces the pressure very much so the vessel costs less. Of course, it reduces also the size of the vessel so you can use a vessel which will run to 15 psig for instance. It will be a little bigger than several times the size of a liquid storage reservoir, but it will be cheaper.

The Trace Analysis of SF_6 Arc By-products

Michael Pohl

Air Products and Chemicals, Inc.

We of Air Products and Chemicals have been attending SNEAP Conferences for several years and have heard of concerns about the decomposition products of SF_6. In addition, people have wondered about the effect of CF_4, moisture and air. It was our idea to study this question by developing a procedure for analyzing SF_6 samples for these compounds. The analytical procedure which we have spent the past year developing will form the basis of my talk today.

On the first transparency you will see a list of the compounds which we are presently capable of analyzing. In addition the procedure is briefly described and the lower limits of detection are indicated. The analysis indicated is performed by use of gas-liquid partition chromatography. As can be seen, there are two entirely different sets of conditions used to perform the analysis. The dotted line in the middle of the transparency serves to divide the two procedures. Above this line is the low temperature conditions used for the more volatile compounds while below are the higher temperature conditions for the less volatile components. Thus each analysis requires two injections into the GC to perform a complete analysis.

This method may appear to be time consuming, but its advantages far outweigh the inconvenience. With this procedure we can achieve resolution of at least one minute between peaks. This allows us to achieve detection limits in the range of 25 ppm. In addition we can prepare our standards containing only a few compounds instead of having to incorporate them all into one sample. This greatly simplifies the procedures for preparing the external standard.

While this procedure seems adequate for most samples, we felt a need to take the analysis to even lower levels of detection. In order to do this we had to resort to GC-MS type of analysis. The GC part of the work obviously utilized the conditions and procedures already indicated on the transparency. The results which we obtained by this procedure were in very good agreement with those which we obtained by the GC procedure. This procedure allowed us to achieve detection limits which were 5 ppm or less. By utilizing single ion monitoring we are able to quickly identify all of the decomposition products. Then by comparing these peak areas with those of our prepared standards we can quickly quantify the results. The result is a good reliable method for detecting these products.

One other advantage of the GC-MS procedure which I failed to mention is its ability to positively identify the peaks. The spectra of each of the compounds serves as a fingerprint of that compound. In our particular case we compare the peaks

to a library of mass spectra which our computer has stored in its memory banks. So this in addition to our prepared standards have proven the structures of each of our compounds. This capability has also greatly helped in identifying unknown peaks. Obviously we cannot guarantee that the twelve or fourteen compounds which we have identified are the only ones which will be present. So if we encounter unexpected peaks, we still have a very strong chance of proving the identity of the unknown compounds. Thus we feel we can handle a wide variety of samples and still give very good results.

Obviously the analytical procedure is only one half of the program. One must have some way of assessing the data after it has been accumulated. The obvious answer is to establish a set of typical parameters to compare any set of results against. While we currently lack such a data base in the area of accelerators, we have been working on one for gas-filled circuit breakers. The next transparency shows typical results for a utility customer of ours. As you can easily see, we analyzed for all of the decomposition products, but we only found a few. These results were not surprising since the sample was drawn from a normal functioning breaker. We would have been very surprised if we had seen many of the decomposition products.

As you can see, we have added the capability to analyze for SOF_4. This thus completes the set of sulfur, oxygen, fluorine compounds. By detecting all of these, one should be able to draw conclusions about the history and maintenance of equipment. Has air been accidentally introduced? If this is the case, maybe a modified filling procedure is required or the equipment is leaking and so some leak detection may help reduce SF_6 consumption. Maybe excess SOF_2 is showing up in the equipment indicating the presence of excess moisture. In this case, the driers need regeneration or replacement. These are typical questions which this type of data may answer. Thus we have high hopes that such a program will lead to increased reliability of SF_6 filled equipment.

What does all this mean for accelerator people like yourselves? Neil Burn has recently discussed a survey of accelerators which he recently completed. This could serve as a springboard for a data base on accelerators using SF_6. If we can establish normal operating parameters for a particular type of accelerator, it should prove much easier to isolate gas related problems. With an even increasing body of data we should be able to help you solve problems relating to accelerator operation. We are looking forward to working with as many of you as possible.

Table I: Analysis Capability

Components	Column Conditions	Rt Minutes	Low Det. Limit
1. Air O_2 + N_2		2.2 - 2.3	<5 ppm
2. Carbon Tetrafluoride, CF_4		3.1	<5 ppm
3. Silicon Tetrafluoride, SiF_4	2 cc 55°C 25' x 1/8" Porapak	5.6	<25 ppm
4. Carbon Dioxide CO_2	QS 42 cc/min.	6.0	<10 ppm
5. C_2F_6	He +2'x1/8" Porapak T in front of QS	7.2	~<10
6. Sulfur Hexafluoride, SF_6	Ni tube	7.7	--
7. Sulfuryl Fluoride SO_2F_2		15.5 8.1	<20 <10
8. Hydrogen Sulfide H_2S		11.8	<35
9. Thionyl Fluoride SOF_2		12.7	<10 mins.
10. Sulfur Tetrafluoride, SF_4		14.4	~<35
11. Sulfur Dioxide SO_2	2cc 115°C 18"x¼" Porapak T	31+	<10
12. Sulfur Dioxide SO_2	30 cc/min. He teflon tube	2-3	<10

Table II: Typical Sample Results

Component	Concentration
Air	0.12%
CF_4	150 ppm
SiF_4	ND <25 ppm
CO_2	~5 ppm
C_2F_6	ND
SF_6	---
SO_2F_2	ND <10 ppm
H_2S	ND <35 ppm
SOF_2	~10 ppm
SF_4	ND <35 ppm
SO_2	22 ppm
SOF_4	ND

Billen: How are these samples obtained and shipped?

Pohl: Normally samples are handled in specially prepared stainless steel cylinders. It is sent out containing a few psig of dry nitrogen. When it is received it is evacuated and then filled to several atmospheres of sample gas and finally returned for analysis.

Walker: How much do you charge for this analysis and how long does it take?

Pohl: We generally handle this on a case by case basis. For our customers we will perform the analysis free of charge. Otherwise we do have a nominal charge to cover shipping and handling. If we only use the gas chromotography (GC) part of the procedure we can perform the analysis in one day. If we must use both GC and mass spectroscopy (MS) it will take significantly longer. This is because we share the GC-MS equipment with other groups in the corporation. Usually we can return results within a week.

Mastroianni: The presence of SF_6 decomposition products in the gas will not necessarily ruin the dielectric strength of the gas, but it may result in reactive compounds. Also, the decomposition products will probably have a different distribution than that found, for example, in circuit breakers, but again the dielectric strength should not be affected.

Jones: There may be some contraindications. When there was a malfunction in the drier system at Canberra, David Weisser reported that the electrical performance of the machine was degraded by breakdown products. The machine was more unstable and so he was suspicious that the electrical properties are affected.

Mastroianni: Might it have been water rather than decomposition products?

Jones: They were convinced that water was being removed but that decomposition products were not being removed.

Mastroianni: It may be that the dielectric strength of the gas is not decreased but that there has been damage from HF.

Jones: In a project at Oak Ridge, there is evidence for chemical damage.

Ashbaugh: We find that if we're operating near the maximum voltage and we have a spark, we can go back up if we let the gas rest or if we recirculate the gas. So the effect is not just degradation of the insulating properties.

Mastroianni: In the design of transmission cables only about 48% of the dielectric strength of the SF_6 is used. After a spark there could be levitated submicron metal particles that temporarily degrade the dielectric strength.

Electrical Breakdown Studies of SF_6/CO_2/Fluorocarbon Mixtures and Experimental Fluorocarbons

Martin Mastroianni

[Editor's note: At this point in the conference Martin Mastroianni presented a short talk titled "Electrical Breakdown Studies of SF_6/CO_2/Fluorocarbon Mixtures and Experimental Fluorocarbons". Unfortunately, he was unable to supply us with a prepared text and as luck would have it, his talk occurred during the unexplained 118-minute gap on the tapes. We do, however, have the following view-graph presentation and a reconstruction from notes of the discussion that followed Mastroianni's talk.]

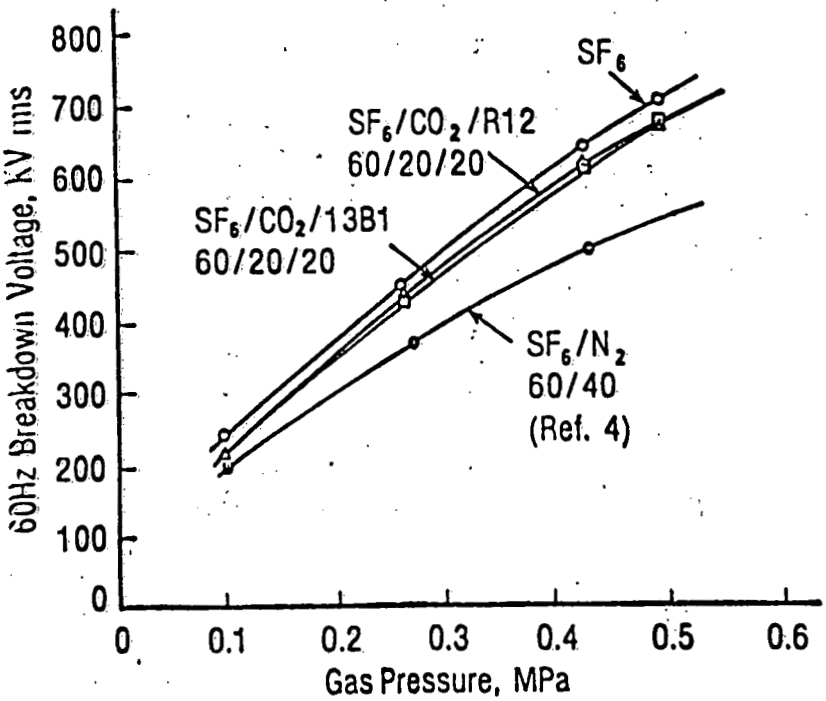
TABLE I **CHARACTERISTICS OF TEST GASES**

Gas	Structure	Molecular Weight	Boiling Point °C	Cost by Volume Relative To SF_6	Breakdown Voltage Relative To SF_6
R115	CF_3CF_2Cl	154.5	-38.7	0.61	0.95
R12	CF_2Cl_2	121	-29.8	0.14	0.92
R13B1	CF_3Br	148.9	-57.8	1.36	0.75
Carbon Dioxide	CO_2	44	-78.5 (sublimes)	0.006	0.36
Sulfur Hexafluoride	SF_6	146	-63.9 (sublimes)	1.00	1.00
Air	-	28.8	-190	0.003	0.4
Nitrogen	N_2	28	-196	0.003	0.4

60 HZ BREAKDOWN DATA

GAS	VOLUME PERCENT	COST RATIO C_r	BREAKDOWN VOLT. RATIO V_r	V_r/C_r
SF_6	100	1.00	1.00	1.00
SF_6/CO_2/R115	60/20/20	0.72	0.98	1.36
SF_6/CO_2/R12	60/20/20	0.63	0.96	1.52
SF_6/CO_2/R13B1	60/20/20	0.87	0.95	1.09
SF_6/N_2	60/40	0.60	0.92	1.53
SF_6/Air	60/40	0.60	0.92	1.53
SF_6/CO_2	60/40	0.60	0.92	1.53
SF_6/N_2	40/60	0.40	0.87	2.18
SF_6/Air	40/60	0.40	0.87	2.18
SF_6/CO_2	40/60	0.40	0.87	2.18

Figure 1. 60Hz Breakdown Voltage for SF_6/CO_2/Fluorocarbon Mixtures



STABILITY OBSERVATIONS

1. WITH SF_6, A FAINT "BAD EGG" SMELL.
2. WITH R12, CARBON WAS NOT OBSERVED.
   WHEN R12 CONCENTRATION EXCEEDED 30%,
   A LINGERING ACRID CHLORINE SMELL WAS APPARENT.
3. WITH R115, SIGNS OF CARBON.
   A THIN LINE OF FINE BROWN POWDER ALONG ENTIRE LENGTH OF CONDUCTOR.
   NO CHANGE IN BREAKDOWN VOLTAGE WITH NUMBER OF BREAKDOWNS.

AMOUNT OF X NEEDED TO DOUBLE INTRINSIC SF_6 DECOMPOSITION RATE

X	PPM_V
N_2	>10,000
H_2	42
O_2	360
CO	250
CO_2	3,000
H_2O	140
CH_4	41
C_2H_4	25

J. CASTONGUAY
GAS. INSUL. CONF.
BOSTON, MASS.
1980

DIELECTRIC STRENGTH OF SOME GASES IN UNIFORM FIELDS (@ 0.15 MPa)

NAME	STRUCTURE	V_r
Sulfur Hexafluoride	SF_6	1.00
Hexafluoro-2-Butyne	CF_3CCCF_3	2.20
Trifluoromethyl Sulfur Pentafluoride	CF_3SF_5	1.55
Thionyl Fluoride	SOF_2	1.42
Trifluoromethane Sulfonyl Fluoride	CF_3SO_2F	1.49
Trifluoronitromethane	CF_3NO_2	1.34
Octafluorocyclobutane	$CF_2CF_2CF_3CF_2$	1.26
Hexafluoropropylene	CF_3CFCF_2	1.06
Sulfur Dioxide	SO_2	1.00
Perfluorodimethyl Ether	CF_3OCF_3	0.84
Hexafluoroethane (R116)	CF_3CF_3	0.79
Sulfuryl Fluoride	SO_2F_2	0.73
Chlorotrifluoroethylene	CF_2CClF	0.69
Nitrous Oxide	N_2O	0.50

SUMMARY

1. GOOD V_R WITH COMMERCIALLY AVAILABLE PERFLUOROCARBONS.

2. SIGNIFICANT IMPROVEMENT IN V_R WITH EXOTIC PERFLUOROCARBONS, HOWEVER, THIS DETERIORATES AT HIGHER PRESSURES.

3. SERIOUS SPARKING INSTABILITY WITH PERFLUOROCARBONS AND MIXTURES WITH SF_6.

4. SPARKING DECOMPOSITION PRODUCTS FROM PERFLUOROCARBONS MAY PRESENT A PROBLEM.

Den Hartog: What is the intrinsic decomposition rate of SF_6?

Mastroianni: I don't think I can give a quantitative answer. Sparking decomposes SF_6 molecules, but one of the particular advantages of sulfur hexafluoride is its ability to self-heal after such an event. However, there is some irreversible loss of SF_6 from sparking.

Noé: I want to comment on the Canberra problem. One of the observations during the period when the drier was not functioning properly was of a large amount of lost charge. That lost charge probably was along solid surfaces. I also have a question. Can you comment on the toxicity of SF_6?

Mastroianni: SF_6 itself is nontoxic although it is asphyxiating, of course. But some of the breakdown products are toxic. Probably one of the worst is SO_2. One should definitely avoid long exposures to SO_2.

ANALYSIS OF THE PELLETRON CHARGING CHAIN BREAK IN THE CHALK RIVER MP TANDEM ACCELERATOR

by

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ABSTRACT

On February 7, 1980 one of the three Pelletron charging chains in the Low Energy end of the Chalk River MP Tandem Accelerator broke during normal operation. The chains had been in use for 38 000 h at the time of the break. Tensile tests were carried out on pieces of the broken chain as well as unused pieces of chain. Several possible reasons for the chain break are suggested; ways of improving performance and reliability are proposed.

1. INTRODUCTION

A charging chain[1)2)] consisting of alternate insulating and conducting segments (Fig. 1) is inherently a more uniform structure than a conventional woven fabric charging belt. In 1974, for reasons of stability, efficiency and cost, the original belt-charging system of the High Voltage Engineering Corp. model MP Tandem Accelerator was replaced by a Pelletron chain-charging system.[3)4)]

To transport sufficient electrical charge to the high-voltage terminal of the MP Tandem, six chains are required; three in the Low Energy (L.E.) end of the accelerator and three in the High Energy (H.E.) end. Each chain consists of approximately 600 steel pellets and an equal number of nylon connecting links. Each set of three chains is driven by sheaves mounted on a common shaft and powered by an 11 kW electric motor.

To reduce stresses on the chains caused by sudden speed changes during start-up, the original motor-starters were replaced in 1978 by four-stage starters which provide a smooth, continuous acceleration from rest up to operating speed. The H.E. chains drive a 3 kW, 400 Hz generator located inside the high voltage terminal.

2. CHAIN BREAK

Shortly after the initial installation of the Pelletron in 1974, one of the screws holding a small idler pulley in place became loose. Eventually the idler fell from its mount and became lodged between two pellets on the centre H.E. chain causing the chain to break. It was necessary to replace several pellets but the only damage to the rest of the accelerator structure was in the form of small dents on the outside protrusion of about eight accelerator tube electrodes.

On February 7, 1980, one of the three chains in the L.E. end of the accelerator broke during routine operation, the terminal voltage being only 4 MV at the time. Damage to the accelerator structure was much like that of the earlier chain break; unlike the previous incident, in which the break occurred at only one point, the chain shattered into more than 30 pieces varying in length from one or two pellets up to about 30. Subsequent tensile tests showed that the nylon links in the failed chain had all become brittle. However, these specimens failed at loads in excess of 700 kg, whereas normal operating chain tension is less than 30 kg.

3. TENSILE TESTS

Tensile tests were carried out at room temperature on one three-link section of chain and on 18 nylon links; a cross-head velocity of 0.02 mm/s was used. The three-link specimen started to yield at 155 kg and further elongation caused the metal connecting pins to deform while the nylon remained unaffected (Fig. 2). After the test, the ends of adjacent nylon links were touching and consequently, the chain would not bend freely.

In the first three tests of nylon links, they were loaded using drill rod through the connecting pin holes but the specimens all failed by tearing through from the pin hole to the top of the specimen. In the remaining tests, the specimens were gripped so that the reduced cross section sustained all the load. The results are summarized in Table 1. Two distinct load-elongation behaviours were observed (Fig. 3).

3.1 Nylon links which had never been used or used only briefly. The load rose to a maximum (mean = 950 kg ± 33 kg) then gradually decreased to zero as the specimen yielded.

The fracture appearance was white and propagated between the root radium at the reduced cross section and the pin hole (Fig. 4A). No surface cracks were observed in these specimens.

3.2 Nylon links from the broken chain or in use for several years. The load rose to a maximum (mean = 935 kg ± 100 kg) then suddenly dropped to zero as the specimen broke.

The fracture appearance was grey and across the reduced cross section at the root radius (Fig. 4B).

There were transverse surface cracks in the gauge length close to the fracture surface in most cases (Fig. 4C).

Although the fracture toughness of the nylon is affected by use, it appears statistically, that the strength of the nylon is unaffected. However, only a small number of samples was tested and specimens from used chains showed a much larger standard deviation than new or unused specimens; with a larger number of samples, a significant reduction in strength may occasionally be observed.

Examination of pieces of the broken chain revealed several bent pins which prevented easy bending of the chains as described above, but whether the pins were bent before or as a result of the chain break is unclear. Broken nylon links from this chain showed a grey fracture similar in appearance to the fractures described in 3.2 above.

4. DISCUSSION

It is disturbing that at the time of the latest chain break no undue stress was being placed on the chain and the break occurred in a nylon link; all the metal connecting pins remained in place. Although the primary failure point has not been determined, several possible explanations for the break can be made from the tensile test data and examination of the chain:

4.1 Part of the chain was defective. This may be expected to cause an early failure and should have been detected during inspection.

4.2 The nylon links failed by stress corrosion or corrosion fatigue in the sulphur hexafluoride (SF_6) breakdown products. The results of the mechanical tests show that the fracture properties of the nylon links can be degraded by use but further testing would be required to fully determine the cause of property loss.

4.3 The metal connecting pins deformed sufficiently that the ends of adjacent nylon links could occasionally interact and become loaded in bending during operation. Loads in the chains may be higher than expected during operation because of mechanical misalignment or impact during start-up, for example. The loss in fracture toughness of the nylon would increase the possibility of failure.

The mechanism described in 4.3 above seems the most likely cause of failure because it fits in with the observations of the failure itself and with the results of mechanical testing. The metal connecting pins start deforming at loads far below the lowest maximum loads of the nylon. This is partly because of the properties of the pin material and partly because of the large bending moment that can be applied. Possible ways of improving performance would be to increase the strength of the connecting pins and to reduce the pin length. Periodic inspection of the chains to test for flexibility should give warning of gross pin deformation. It would also seem a wise precaution to remove possible harmful breakdown products from the SF_6 by continuous recirculation through activated alumina for example.

Since the chains have operated for about 40 000 h, the cause of the chain break is uncertain and future breaks may seriously damage the accelerator structure, the decision was made to replace all six chains with new ones.

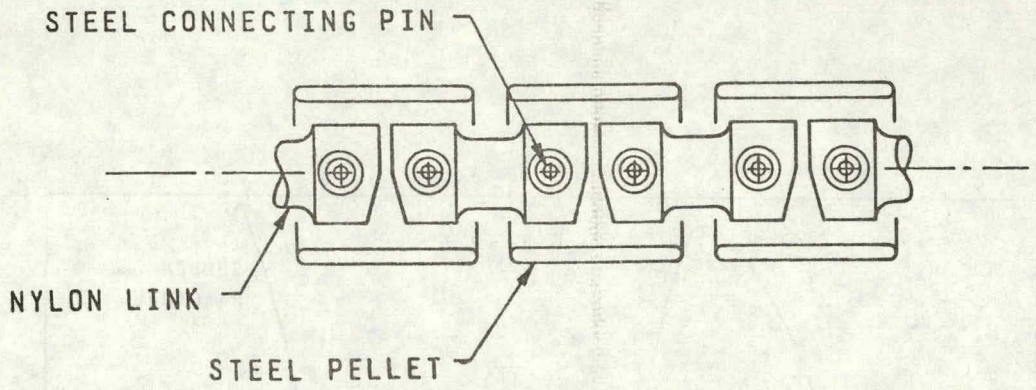
REFERENCES

1) Herb R.G., IEEE Trans. Nucl. Sci. NS-18, No. 3 (1971) 71

2) Herb R.G., Nucl. Instrum. Methods 122 (1974) 267

3) Burn N., et al., Proceedings of the International Conference on the Technology of Electrostatic Accelerators, Darcsbury (1973) 48

4) Burn. N., et al., 2^e Conférence Internationale sur la Technologie des Accélérateurs Electrostatiques, Strasbourg (1977) 1369 Revue de Physique Appliquée Tome 12 No. 10 1977 October



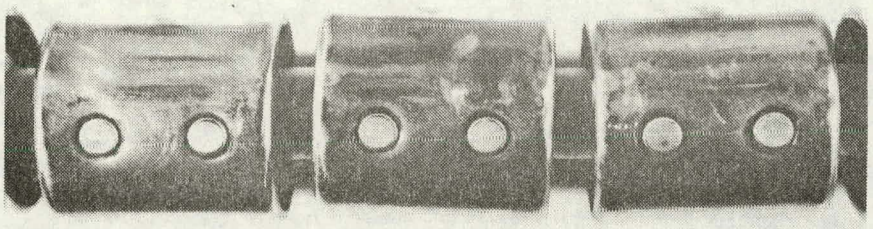


Fig. 1. Pelletron charging chain.

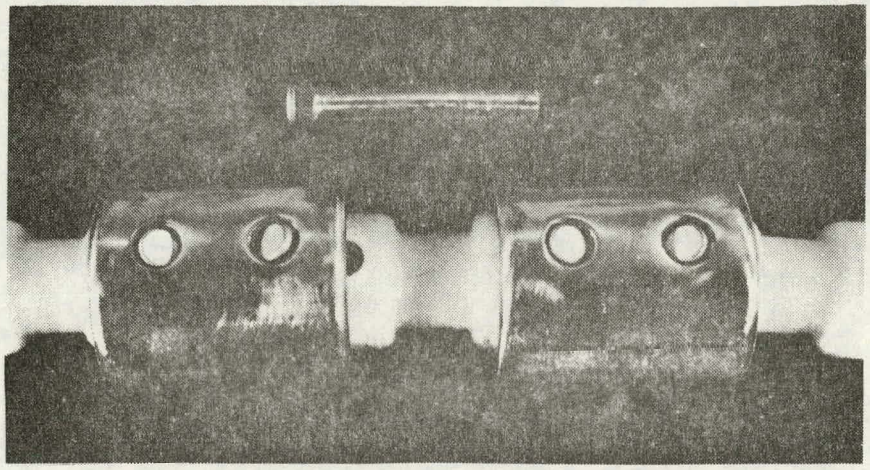


Fig. 2. Deformed chain after tensile test.

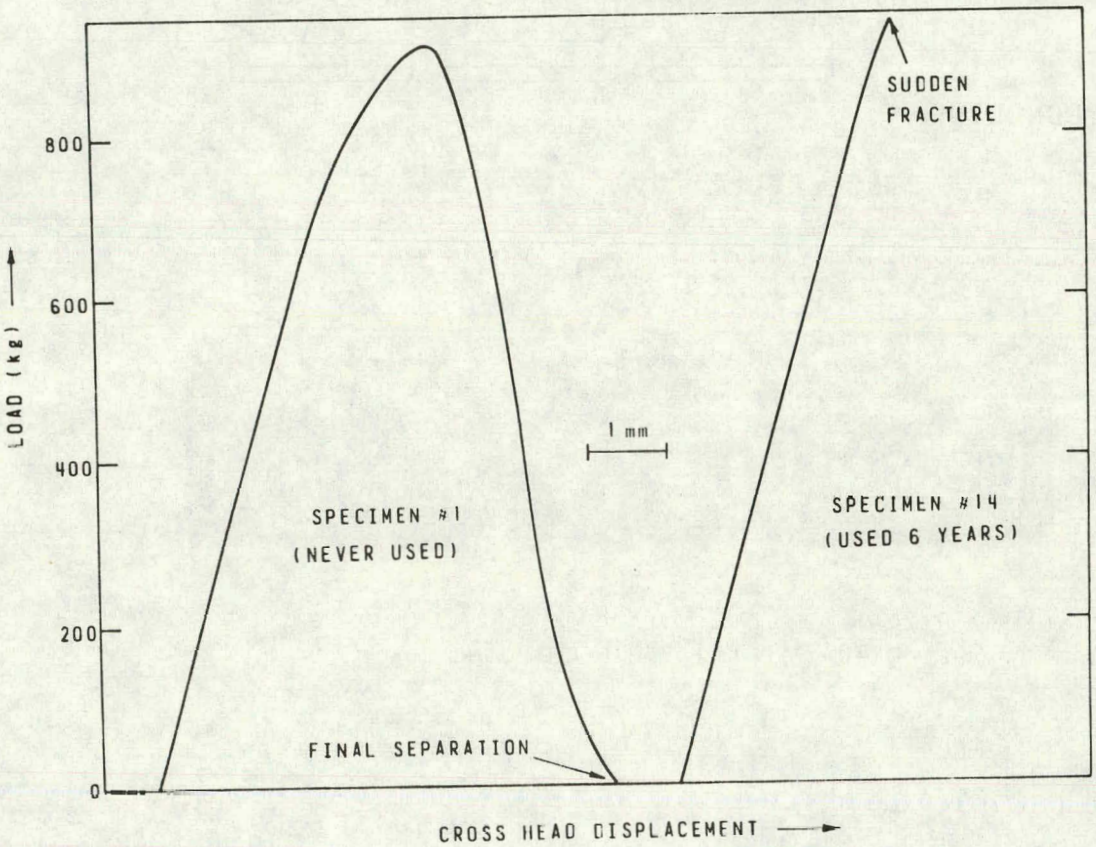


Fig. 3. Nylon link load-elongation curves.

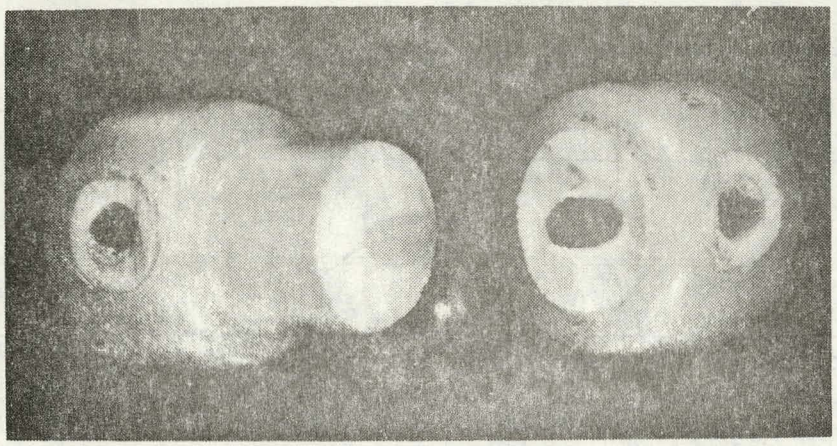


Fig. 4A. Fracture of new specimen.

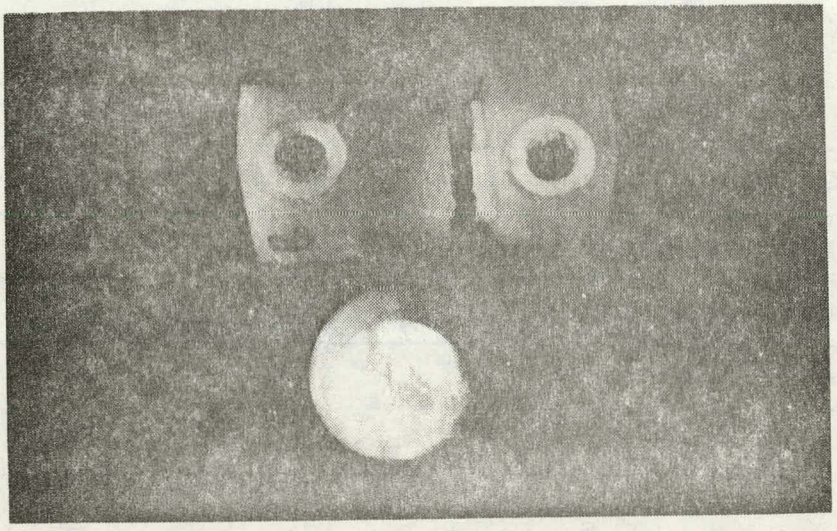


Fig. 4B. Fracture of used specimen.

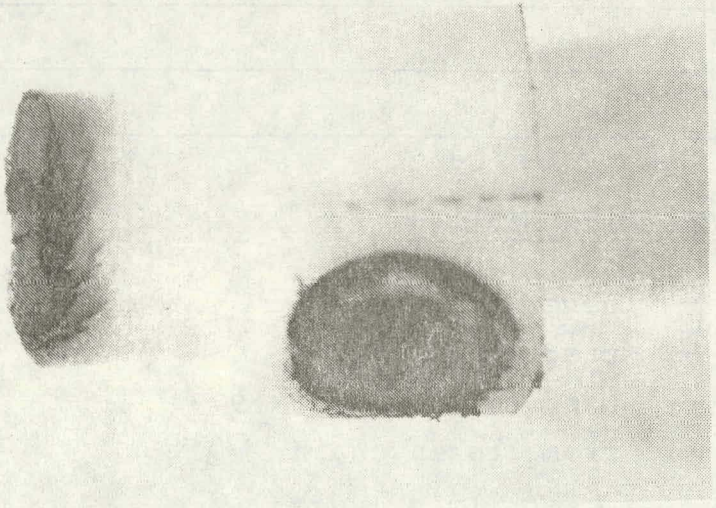


Fig. 4C. Dye penetrant test on fracture surface of used specimen.

TABLE 1

MECHANICAL PROPERTIES OF NYLON LINKS IN PELLETRON CHAIN

SPECIMEN #	MAXIMUM LOAD, kg	COMMENT
1	912	
2	950	Gradual loop drop to zero. Fracture
3	920	through to pin hole from root radius
4	987	Mean = 950 kg ± 33 kg
5	964	
6	970	
7	1068	Sudden break. Brittle fracture across
8	1066	reduced section starting at root radius
9	1061	as 1 to 6
10	998	as 7 and 8
11	735	
12	1048	
13	966	as 7 and 8. Mean = 935 kg ± 100 kg
14	946	
15	1014	
16	905	

Specimen History

1 to 3 — never used
4 to 6 — used briefly
7 to 10 — used until summer 1979
11 to 16 — from broken chain

Adams: What temperature do the pellets reach in operation?

Burn: I don't know.

Rathmell: The SF_6 cools the pellets very effectively during the transit between the pulleys. The nylon will not tolerate a temperature rise.

Lund: Have you considered testing some of Canberra's links?

Burn: No, we haven't.

Fauska: What if someone put a twist in the chain.

Burn: You can't put a twist in it because of the way they are constructed.

Wise: Have you considered sidewise forces and bending moments? It seems that all of the tests you did so far relate only to longitudinal strength.

Burn: That's right. We haven't tested them in the bending mode.

Connolly: What about radiation effects?

Burn: The radiation levels in the MP are not high enough to worry about.

Storm: Were the tests to 900 kg on the chain that broke?

Burn: Yes.

Janzen: Why did the chain break into so many pieces? You said there were about 30 pieces.

Burn: Presumably, only one link broke and then because of the high velocity of the chain the rest of the chain broke upon contact with other parts of the machine.

Chapman: Did any of the rivet pins fail?

Burn: No, and none bent.

Rathmell: I'm suspicious of the effects of drying of the nylon by SF_6 over long periods of time. This could account for the large number of pieces because of embrittlement of the nylon.

Sato: Our chain is much older than Chalk River's but with no damage yet. But our moisture levels are much higher.

Noé: Can the pellets be salvaged?

Rathmell: We'll see.

Brookhaven National Laboratory Pelletron Report

by

Robert Lindgren

The initial installation of Pelletrons was in April 1977 and we have to date about 25000 hours on it. One LE chain broke in July 1979, due to an inductor becoming loose. The chain was inspected, repaired and reinstalled. It broke again in July 1980 for no apparent reason. Not having a spare chain, we left it out, removed 1/3 of the counterbalance weights and continued running. In September 1980 a HE chain broke. By this time we had a spare chain, and we installed it in the empty LE position, and left one empty sheave in the HE end. Here we found out that the number of pellets in a chain do not mean as much as the tension on a chain run. The new chain measured 14 3/8" for 10 links. The old chains measured 14 9/16" for 10 links, so an old stretched chain would be about 11.5" longer than a new one, having about 620 links. We have made a tension measuring device that clips across three links, on an installed chain, that is tightened up until the middle link becomes loose, then measuring the length of a calibrated spring on this device gives the tension. When the tension of all 3 chains is approximately equal, you're in. We have about 50 lbs per chain run.

At this time we disassembled the remaining broken chains at the master link pins. We found that all of the master link pins were bent, and the rivet screw pins that hold each link to the next, were not bent. These pins along with a supply of spare unused pins, were brought to the Metallurgy department for a check on their hardness. It was found that all of the pins with threaded ends had a Rockwell A scale rating of 43, corresponding to about 50KPSI tensile strength. All of the pins which were not tapped, and meant to be used with rivet screws, had a Rockwell A scale rating of 61 corresponding to about 110KPSI tensile strength. Altech Corp., Dunkirk, N.Y. was consulted, and said that this type of SS stock, normally bought, would have a tensile strngth of 100K to 125KPSI. The only way it could have an Ra rating of 43 would be if it were annealed. We feel that someone in the process of making these pins annealed them to make the threading easier, and that this bending of the pins perhaps caused the breaking of the chain the last 2 times. Our program calls for removing the remaining chains from MP7 and

inspecting all of the screw-type links for bending, and to replace them with pins with the proper hardness. Robert Rathnell at NEC has checked the pins they have in stock at the present time and found they measure about 21 on the Rockwell C scale, corresponding to Ra 61. The chains we have were assembled in 1976, so who knows what someone did then, perhaps in an outside shop to make the threading of the pins easier. NEC will supply replacement pins for us.

Mike McKeown has been making some pickoff wheels for the terminal assembly, to replace the wheels using springs supplied by NEC, and modified by Chalk River. These pickoff wheels are used to put a charge on the inductors in the terminal for spark suppression and down charging. Mike has moulded urethane and carbon cloth to an aluminum hub to give a conducting surface to pick charge off the chain. These wheels have been installed in MP7 and have been operating for about a year. He is currently working on a drive sheave wheel 18" in diameter that we hope can be used to replace the present nylon rims and contact bands that are responsible for the self charging effect and do away with the need for oiling the chains.

We have also noticed a wearing of some of the nylon sheave rims. One side of the rim has worn down about 1/8 of an inch. It was thought that misalignment of the sheave and the first guide pulleys would do this but we've checked and the alignment is good. Now we're considering whether the bent pins might have caused this uneven wear.

One other problem that pelletron users might wish to check out on their own installations is on the width of the sheaves aluminum hub, as compared to the width of the nylon rim. We found on the MP6 installation that aluminum hub projected out beyond the nylon rim 10 to 30 mills. This would hold the pellet contact bands away from the pellets. On advice from NEC we machined the hubs back so they were at the same width dimension as the rims. Checking MP7 sheaves in place, the hubs are machined more carefully, but on some of the 12 sheaves we do find the hubs project a noticeable amount, maybe 5 to 10 mills, out beyond the rims. This we will correct when we have to take out a sheave assembly.

AN INVESTIGATION OF THE CHARGING SYSTEM USED IN THE CHALK RIVER 2 MV PELLETRON

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ABSTRACT

A simple device, which monitors the relative amount of charge on individual pellets, has been used to investigate the condition of the charging system of a 2 MV Pelletron. This report demonstrates the usefulness of this technique for diagnosing malfunctions in pellet charging systems.

INTRODUCTION

Investigation of "lost up charge" phenomena in the Chalk River 2 MV Pelletron has spanned several years and has revealed several possible problem areas. These have included tracking along the corona triode assembly, "wet" SF_6, and changeable corona conditions along the accelerator tube. As each area was uncovered, and subsequently analysed, steps were taken to maintain the original charging characteristics of the accelerator.

Recently a system of pellet charge monitoring was installed which enables the operator to supplement the basic metered charging current readings with observations of the relative charge per pellet along the chain, on its up and down runs.

The model 2UH Pelletron provides the acceleration potential for the CRNL High Voltage Mass Separator. The charging system of the 2UH utilizes a single chain of 125 pellets, passing over two 30.5-cm diameter pulleys, one of which is driven by a 2 HP motor operating at 870 rpm. The resultant chain speed of 14 $m \cdot s^{-1}$ ($\sim$ 380 pellets$\cdot s^{-1}$) produces a single pellet repeat frequency (PRF) of $\sim$ 3 Hz. The transit time, for a charged pellet leaving the charging inductor to arrive at the terminal pulley, is $\sim$ 140 ms.

The Chalk River Pelletron uses the original "spring contact" type pulleys in the charging system. The springs on the base pulley, which is mounted directly on the shaft of the drivemotor, are subjected to the most wear due to chain slippage, especially during start-up. After some 5500 hours of operation many springs had worn to such a degree that replacement was required.

Thieberger[1] recently reported the installation of a chain monitoring system of the capacitive pickup type for one of the BNL (Pelletron converted) MP Tandem accelerators. Prior to the replacement of worn springs we installed two such devices. A report which describes the details of this installation and subsequent observations, has been prepared[2]. A summary of these observations will be presented here.

APPARATUS

Aluminum pickup rings, one each for measuring the up and down charge on the chain, surround the chain approximately 5 cm from the charging and suppression inductors. The up charge monitor assembly is shown in Fig. 1. Each ring is ∿ 12 mm long and is supported off ground by a bakelite plate which permits adjustment for a nominal chain-to-ring spacing of 6 mm. They are slotted to pass over the chain during installation.

Signals are fed via coaxial cable to the control console where they are observed by an oscilloscope. Transient suppressors are inserted in each signal line where they pass through the tank base. The pulse response was measured and an attenuation coefficient of 3×10^3 was observed. A 150 mV peak-to-peak signal was observed for a charging current of ∿ 45 μA.

RESULTS AND DISCUSSION

At the time the monitors were installed the majority of the springs on the base pulley were worn down to stubs, providing questionable up charge uniformity. The springs on the terminal pulley, however, were all intact.

Fig. 2 shows an oscilloscope display representative of the original conditions. All photographs were obtained with a total charging current of ∿ 45 μA and a terminal voltage of ∿ 1 MV. Each cycle represents the passage of a single pellet through the pickup ring. The lower trace indicates good uniformity of the charge carried per pellet on the down charging portion of the system. The up charge monitor trace, however, displays factors of 2-3 variations in amplitude, indicative of intermittent pellet charge neutralization by the base pulley. The slow component, which appears in some photographs, is likely due to chain oscillations.

The identity of individual pellets can be simply demonstrated by intentionally shorting together two adjacent pellets. The results are shown in Fig. 3. This not only verifies the previously assumed single pellet response time of ∿ 2.5 ms but also displays the coupling effects of adjacent charged pellets as they approach the pickup rings.

Attempts to correlate the up charge variations with terminal voltage fluctuations were inconclusive, as one might expect, since the amount of charge deposited per pellet is small compared to the stored charge in the terminal. Fig. 4 is a typical example. The time span is ∿ 200 ms in this case (i.e. sufficient to see these effects assuming a ∿ 140 ms pellet transit time). The capacitive pickoff (CPO) signal represents 15 volts peak-to-peak ripple. The ripple frequency is approximately twice the PRF.

The option existed of adding a set of side bands to the base pulley, which NEC had provided during the installation of the 2UH, or simply replacing the springs. We decided to install a new set of springs on a spare mounting strip. This work could be done outside the machine as time permitted with no interruptions to experiments and the new springs could then be mounted on the pulley during a scheduled shutdown. The removal of the original strip of springs and the subsequent installation of a new set took only a few hours. One hour later the results shown in Fig. 5(b) were obtained.

Once the new springs were installed, minimal sparking conditions were obtained at the charging (base) end when $E_{supp} \approx E_{chg}$, indicative of correct

pellet charge neutralization. The down charge is running at ∿ 90% of the up charge. There still exist ∿ 10% variations in pellet charge as observed on these monitors, however these are insignificant when compared to the factors of 2-3 observed initially. The terminal stability of the 2UH remained well within the original design limits of ± 25 V @ 1 MV in spite of these large variations.

Fig. 6 illustrates the condition of the springs after removal from the base pulley. Of the original 93, 39 were badly worn but still intact and the remaining 54 were either broken off or reduced to stubs.

Prior to replacing the springs, we observed and subsequently analysed a number of small yet significant effects. For example, the down charge monitor initially exhibited several positive-going transients on an otherwise smooth cyclic pattern. If the charging system were operating correctly one would expect to operate the charging and suppression power supplies, at the same potential with minimum sparking[3]. Fig. 7 shows a sequence of photographs obtained while E_{supp} was varied and E_{chg} was held constant. The (initially) positive-going transients went through a minimum with $E_{supp} \approx 2\ E_{chg}$ and then negative as E_{supp} was further increased indicating non-uniform and irregular pellet charge neutralization at the base pulley. This effect was verified by using a third "signal" provided by a spare length of coaxial cable hanging in the base of the tank. With the transient suppressor removed temporarily, this provided an excellent monitor as shown in Fig. 8. Sparking during this last sequence was also monitored, and the minimum verified, by observing the "snow" level on the closed circuit TV system used to monitor source parameters in the terminal. This latter method has routinely been used to verify that E_{supp} was in fact set $\approx E_{chg}$.

SUMMARY

A simple device, originally developed to monitor relative parameters in a tandem accelerator employing several charging chains, has been applied with very worthwhile results to a 2 MV accelerator incorporating a single chain.

ACKNOWLEDGEMENT

The author wishes to express his gratitude to G.A. Sims who assisted in the installation of the monitors and for his indulgence during the author's frequent requests for accelerator time for that one "final" photograph.

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(2) D.A.S. Walker, AECL-MISC-194 (1980).
(3) R.G. Herb, Nucl. Instr. Meth. 122, 267 (1974).

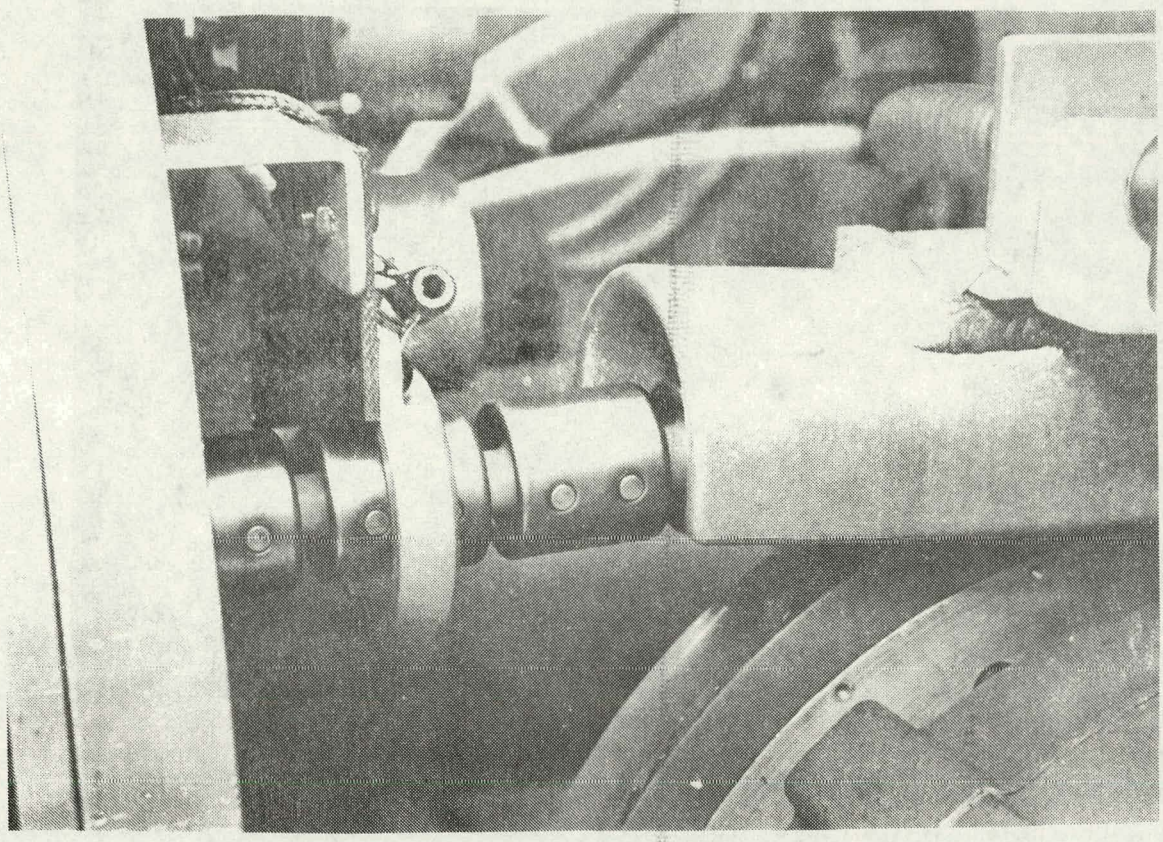

Fig. 1. The up charge monitor assembly. The pulley rotates in a ccw direction driving the chain through the inductor from right to left. Note the contact springs on the pulley. The drivemotor is in the foreground.

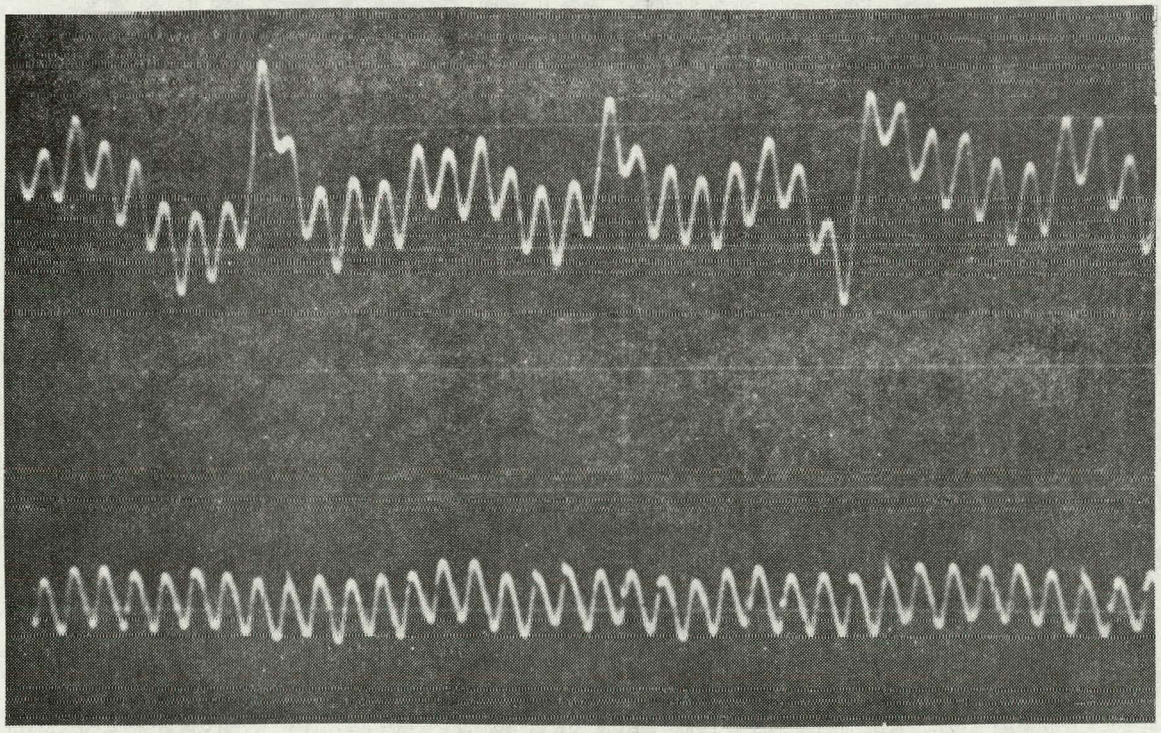

Fig. 2. Charge monitor signals with the original springs in place. Top trace is up charge, bottom trace is down charge. Time scale is 10 ms/div.

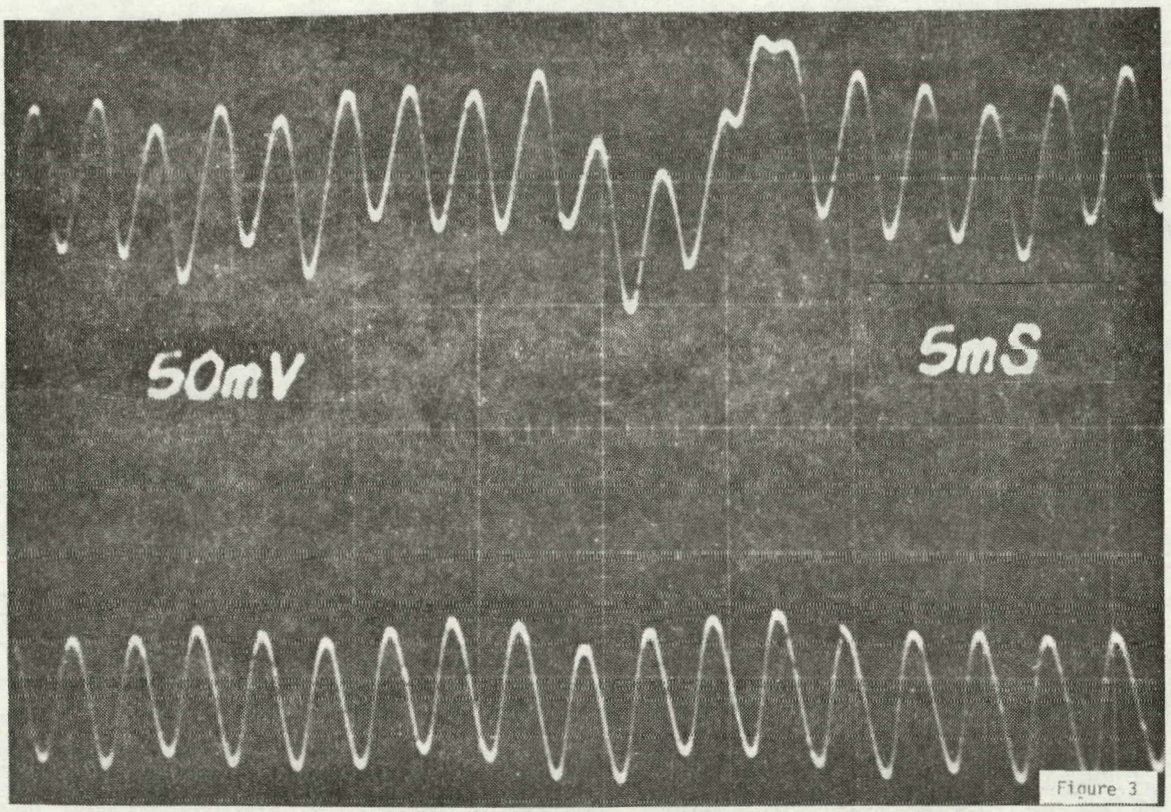

Fig. 3. Characteristic display for operation with two adjacent pellets intentionally shorted. Top is up charge, bottom is down charge. This example shows the shorted pellets passing the up charge monitor.

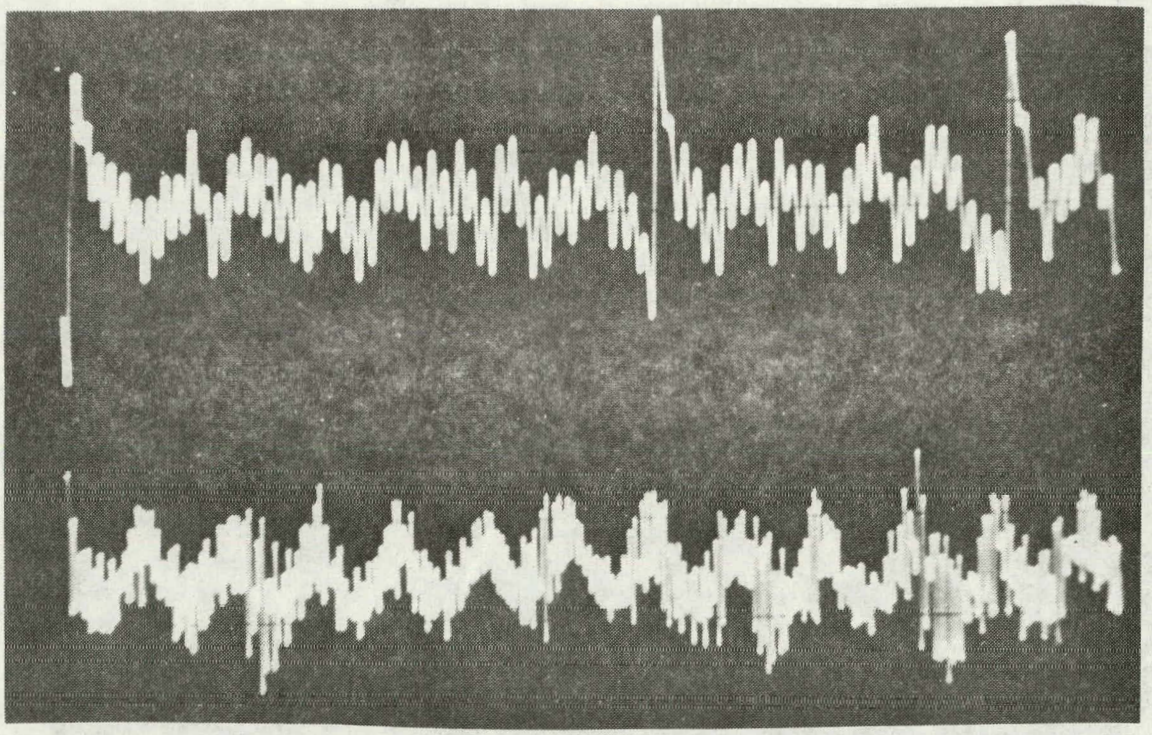

Fig. 4. Up charge (top) and CPO (bottom) signals. Time span ~200 ms.

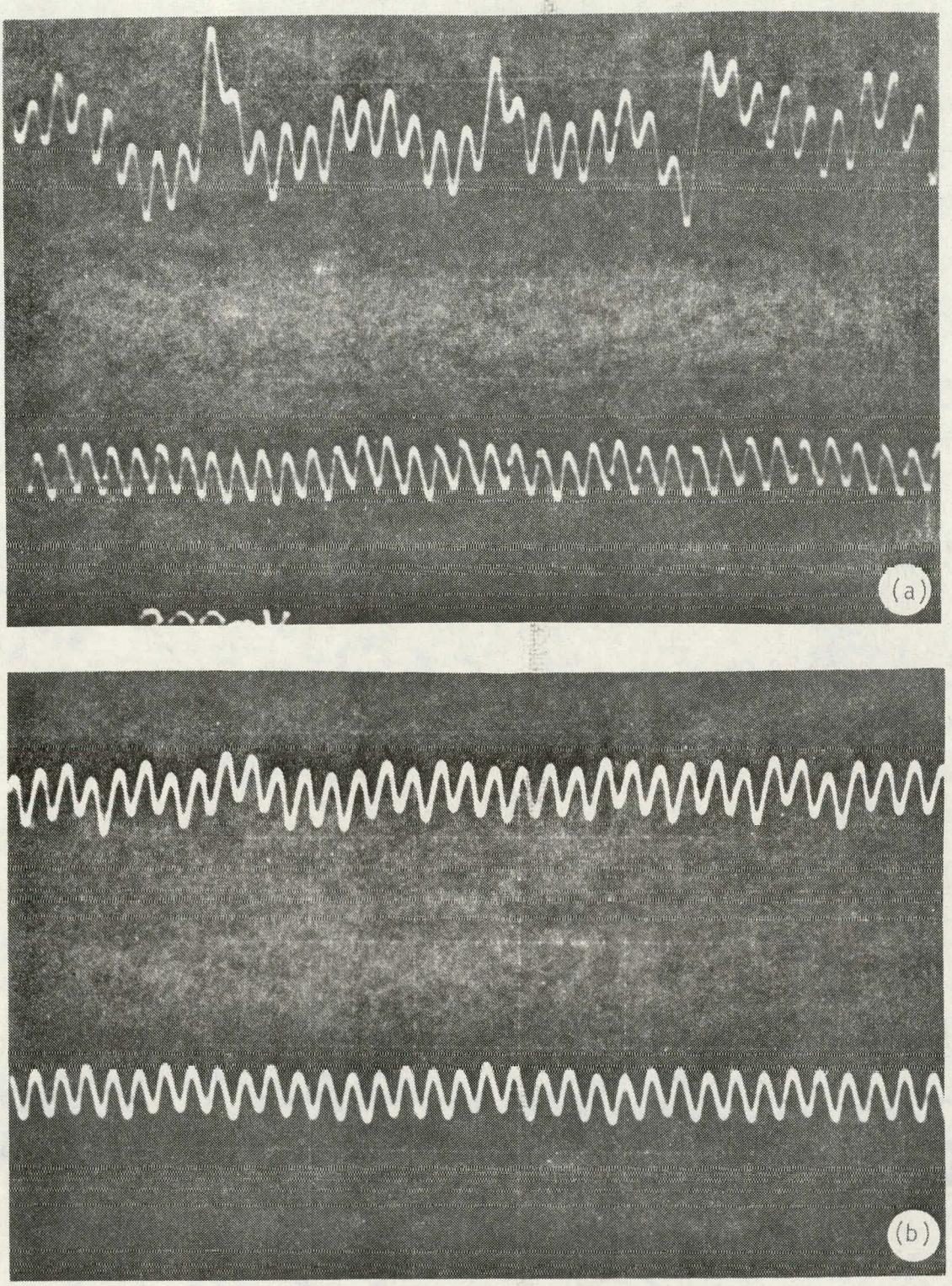

Fig. 5. Up charge (top trace) and down charge (bottom trace) monitor signals with (a) the original springs and (b) a new set of springs installed in the base pulley. For all traces the vertical scale is 200 mV/div and the time base is 10 ms/div.

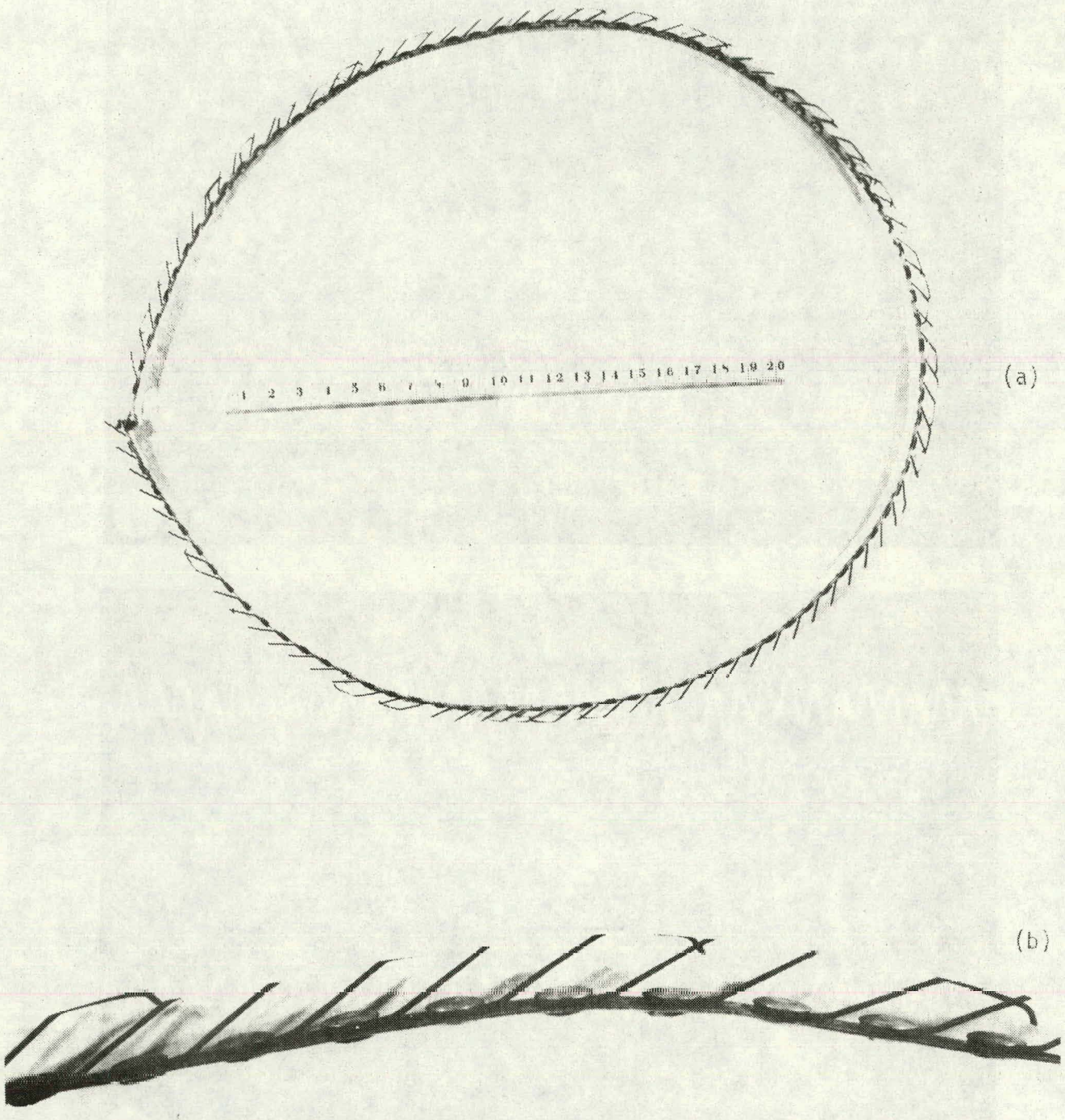

Fig. 6. Photographs of the worn spring assembly after removal from the base pulley. In (a) the entire strip is shown, and (b) shows details of a small section of the strip.

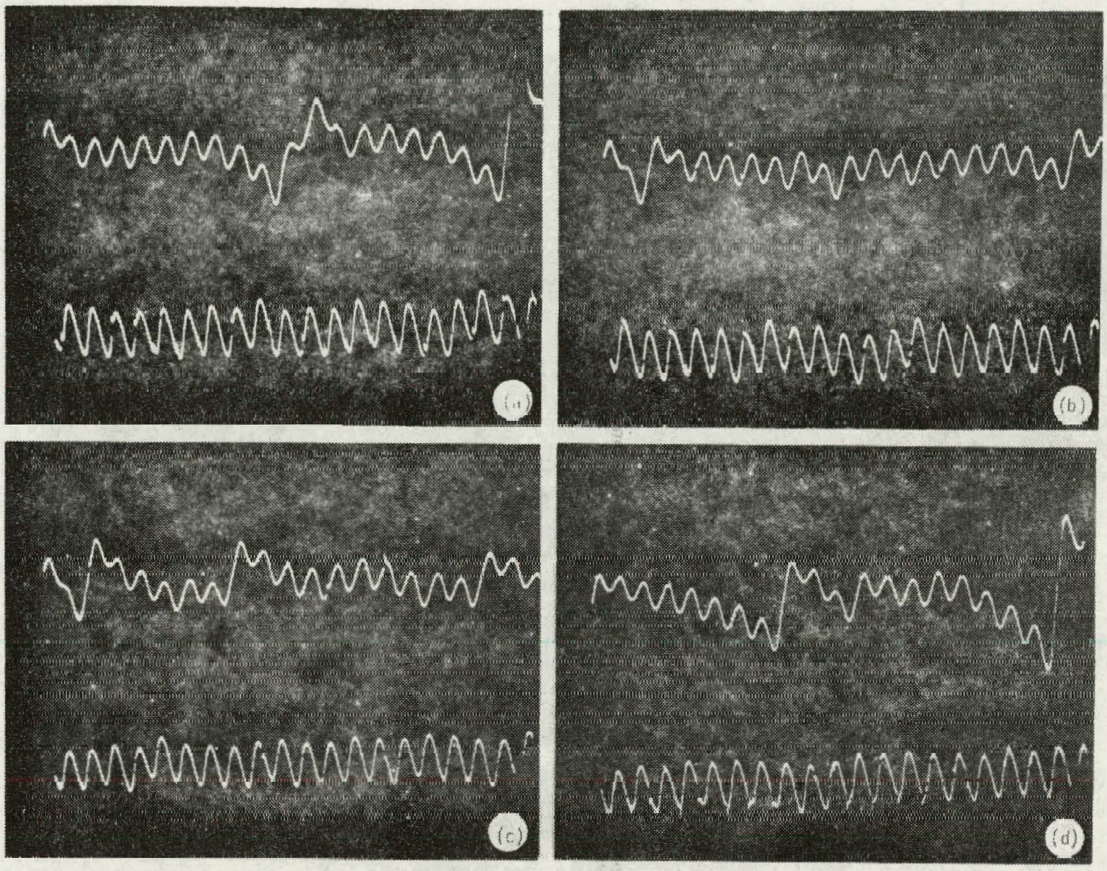

Fig. 7. Photographs showing the effects of varying the suppressor voltage relative to a fixed charging voltage of 8 kV with the original springs in place. In each photograph the top trace is the up charge and the bottom trace is the down charge. The time span is about 50 ms. The suppressor voltages are (a) 0 V, (b) 8 kV, (c) 16 kV, and (d) 24 kV.

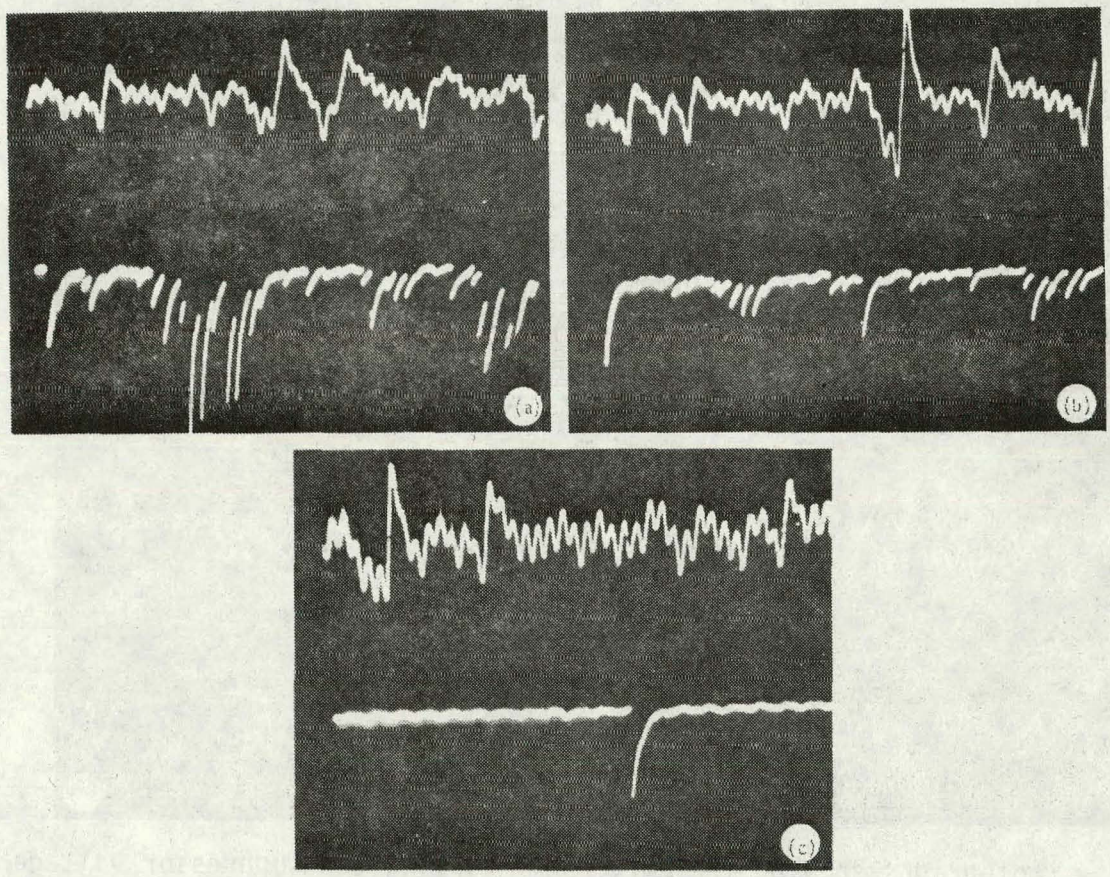

Fig. 8. Photographs showing "sparking" for three different values of the suppressor voltage, all with the original springs. The charging voltage was 10 kV and the suppressor voltage was (a) 0 V, (b) 10 kV, and (c) 15 kV. In each photograph the top trace is the up charge with a vertical scale of 100 mV/div, and the bottom trace is the "Spark Monitor" with a vertical scale of 20 mV/div. The time span was about 100 ms.

Walker: Just before I left Chalk River, Pete Hurley had installed similar devices in the Chalk River MP tandem. Figures 9 through 11 show monitor traces before and after new side bands were installed on the terminal pulleys. The before pictures correspond to a terminal voltage of 4.9 MV and the after pictures are at 8.1 MV. The time base is the same as before (about 20 mV/div). These are all for the high energy chains only. Neil, am I correct in assuming that all side bands were replaced in this system?

Burn: Yes.

Walker: Each figure shows both the up charge and the down charge for each chain and set of conditions. When Pete Hurley initially got this result he gave me a phone call and asked if I'd come over and have a look. Remember that they use side bands in the MP rather than springs. When we started counting pellets it looked as if something had gone amiss with one section. When they opened up and had a look one could see daylight between one particular side band and the chain as it passed through that section. So that device that Pedro described in 1978 has been a very useful one at Chalk River. The erratic "before" results for the right chain were directly related to worn side bands on the drive pulley. About 1/32-inch gaps were observed between the pellets and one set of side bands.

Den Hartog: I'd like to address this to Neil. Did you notice any change in performance when you changed the side bands other than what you see on these pictures?

Burn: Yes, we had more stable performance afterwords.

Noé: I missed the conclusion about the effects of varying the suppression voltage. Was your conclusion that the suppression voltage was not correct?

Walker: For the original bad spring condition, yes. Once we put in the new springs the minimums are obtained when the suppression voltage and the charging voltages are equal.

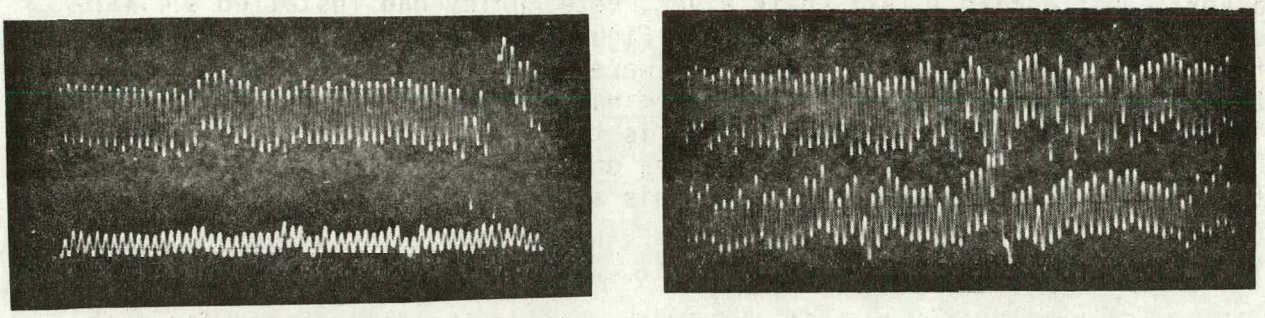

Fig. 9. Up charge (top) and down charge (bottom) monitor signals before (left) and after (right) new side bands were installed in the terminal after ~40,000 hours of running time. These traces are for the left chain as seen from the source end of the machine.

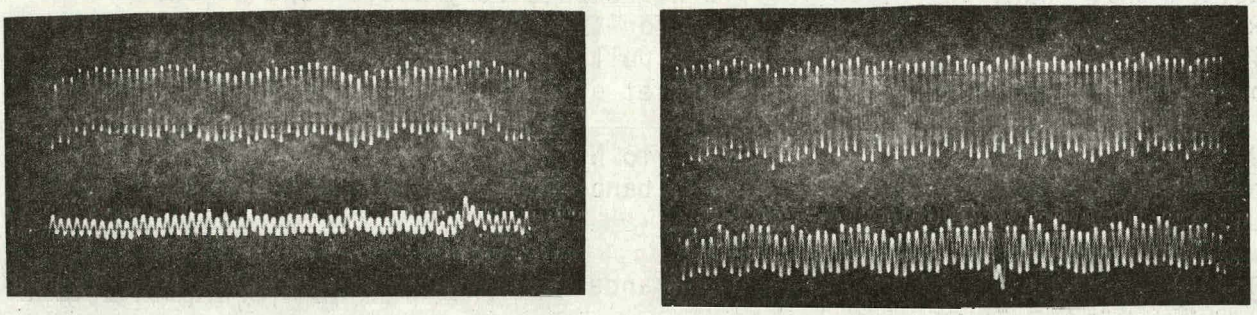

Fig. 10. Same as Fig. 9 but for the center chain.

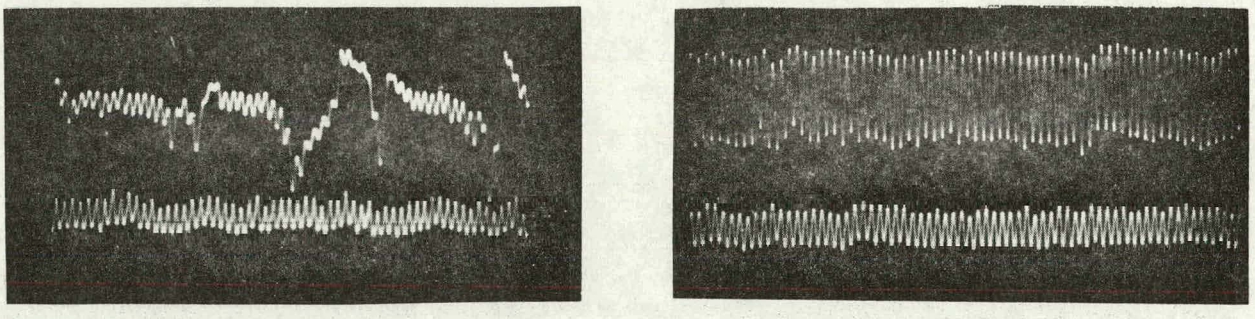

Fig. 11. Same as Fig. 9 but for the right chain as viewed from the source end of the machine.

An Improved Two-Loop Voltage Regulation System for an FN Tandem Accelerator

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The corona regulator loop is a standard feature of voltage stabilizer systems on almost all electrostatic accelerators. The corona loop has as its principle advantage a very large regulation range at low frequency. For a typical maximum current swing of 100uA and column resistance of ∿30GΩ (FN) the corresponding voltage swing is 3MV! The principle limitation of the corona loop is the sharp cutoff of its frequency response curve at a frequency near 10Hz. The frequency response and phase shift for an FN corona regulator are shown in Fig. 1.

The curves in Fig. 1 were obtained by driving the corona triode grid with an audio oscillator and observing the resulting signal on a capacitance pickup (mushroom) with a lockin amplifier. The response curves for the capacitance pickup alone have been used to correct the data. Use of the lockin permitted extension of these curves to relatively high frequencies (low signal/noise) for such a measurement.

The frequency response has two components. The first represents the simple parallel RC network of terminal-to-tank capacitance (∿150pf) and column resistance (∿30GΩ). This gives a corner frequency of about 0.06Hz followed by a smooth 20dB/decade rolloff out to about 5Hz. Beyond 5Hz the flight time of negative ions in the discharge from corona points to terminal (∿30mS) comes into play, producing the second major feature of the frequency response, the series of minima and maxima falling at about 40db/decade.

A more important consequence of the finite ion flight time is the rapid decrease in phase above about 3Hz. This has important consequences with regard to loop stability. A stability requirement for a servo loop is that at any frequency for which the loop gain is larger than one the accumulated phase shift around the loop must be less than 180°. In fact a "phase margin" of 20°-40° is usually required in a real loop. This requirement establishes the unity gain (0db) point of the corona loop at 10-15Hz. The exact point depends on ambient noise and the ion flight time, which in turn depends on terminal voltage, corona points position and insulating gas pressure.[1]

In order to see how well the corona loop alone does, one should consider the noise spectrum it must handle. Under normal circumstances this consists of low frequency (1/f) drifts of order 100 - 300kV and belt noise of order 10kV at the fundamental and harmonics of the belt frequency (2.5Hz). If unity gain is fixed at 10Hz this gives a noise reduction factor of 7 at the belt frequency and a factor of ∿200 at DC. Therefore the residual voltage noise with the corona loop in operation is expected to be 1-2kV, and this is typically observed.

For work involving narrow resources (width $\lesssim$ 1keV) or in instances for which some malfunction causes voltage excursions exceeding the values given above (beam loading, resistor breakdown, damaged belt, etc.) an additional regulation loop with improved frequency response is essential. This problem was realized and work began on fast regulation loops more than ten years ago.

One of the first fast loops was installed in the University of Washington FN tandem in 1972 after some years of development.[2] It is similar to a number of other systems installed at various laboratories and consists of a high voltage amplifier in the terminal which drives the foil or gas stripper. The amplifier output is determined by an axial light link using IR LED and photo transistor.

Installation of the fast loop or "terminal regulator" resulted in improved energy resolution and reduced beam intensity modulation. The resulting two-loop system was operated as installed until 1979 when it was decided to overhaul the system, eliminate ground loops, combine separate control panels and investigate the fundamental limitations of the two-loop system in detail.

The unified control system which evolved is shown as a block diagram in Fig. 2 and as a detailed schematic in Fig. 3. The three analog inputs are the image slit current difference and sum, and the GVM signal (1V/MV). The TVEC TTL signal is derived from a digital window set on the terminal voltage (GVM).

The two analog outputs drive the corona triode grid and the light emitting diode (LED) in the fast loop. The total image slit current (SUM) is compared to an adjustable threshold to determine whether the corona triode is driven by the slit difference or the GVM error signal. A TTL output reflecting this status is available to control data acquisition.

A rotary switch allows selection of four control modes: Auto, GVM, Slit, and Manual. In auto mode the triode is driven by the GVM error signal unless the total slit current is above the selected threshold and the terminal voltage is within the selected window. This latter feature is very useful for heavy ion operation as it determines a unique charge state for slit regulation during recovery from a tank spark or other disturbance. In manual mode, either error signal can be switch selected for either regulation device.

Separate corona loop gains are provided for slit and GVM difference signals. The diode loop has a common gain for both signals. In addition, the filter immediately following the diode loop gain stage is very important for best operation of this system, as discussed below.

Analysis of the fast loop as originally installed revealed that the frequency response was that of two successive low-pass RC stages with corners at 450Hz and 1.5kHz. Since the second stage accumulated a total phase shift of 180° above 2kHz this meant that the unity gain point was

about 2kHz and the maximum stable gain possible was about 4! In addition the response extended to DC, with the result that the fast loop with a voltage range of about 4kV, was competing with the corona loop to eliminate terminal fluctuations of 100kV or more, thus sending the fast loop into saturation periodically.

The new fast-loop filter, shown in detail in Fig. 3, produces an overall loop frequency response which peaks at 2.5Hz and rolls off at 20dB/decade at lower and higher frequencies. It consists of a high pass stage with corner at 1.2Hz followed by a lag filter which serves to move the old 450Hz corner down to 3Hz. The unity gain point is still at about 2kHz, but the maximum possible loop gain is now 500 (at 2.5Hz).

The above system requires logarithmic slit preamplifiers which contribute no phase shift to the loops out to at least 2kHz. At low currents (~1nA) this is not possible unless careful attention is paid to the stray capacitance in the diode feedback element. The circuit shown in Fig. 4 is adapted from a system used to determine semiconductor parameters.[3] It is temperature compensated (also absolutely necessary), has a 1V/decade gain curve (within 3%) from 10pA to 100 μA and has a separate feedback loop for capacitance compensation.

This compensation action can be seen in Fig. 5. In a) a 1nA square wave on a 10nA background is fed into the log preamp with no compensation. In b) the gain in the compensation loop is properly adjusted for best rise time (~100μs) without ringing.

In addition to the compensation above it is very important that ground loops be eliminated and capacitive coupling to AC sources be minimized. In this circuit opto isolators are used, a single point ground is made to the beam drift tube, and shrouds have been placed around the slit jaw adjustment micrometers to shield the slit cooling access holes. A closed circuit oil cooling system for the slits has been installed to eliminate current leakage and static charge buildup noise. The preamps themselves are placed in a double-shielded box also containing a double-shielded power supply transformer enclosed in μ-metal, and special low-noise coaxial cable is used between the slits and the preamp inputs. This combination of steps has resulted in a total line frequency and acoustical noise current equivalent to less than 10pA.

An additional source of "noise" is secondary electron cross talk between slits. We have staggered the image slits 20cm apart symmetrically about the image point of the analyzing magnet, and each slit has its own suppression cage within the drift tube. This system has eliminated all secondary electron problems.

The performance of the slit preamps is shown in Fig. 6. In a) the difference and sum signals are shown with beam present. The beam has been purposefully adjusted to produce a significant intensity modulation, illustrating the excellent common mode rejection capability of the slit preamps. This good common mode rejection is only possible if the amplifier gains are matched at all frequencies out to 2kHz. This means that the capacitance compensation, as well as the DC gains, must be matched. The difference signal

in a) is equivalent to a total equivalent voltage fluctuation of about 100 volts. The fast fluctuations in the signal are derived from 800Hz ripple in the terminal high voltage power supply. In b) there is no beam on the slits and the residual line and acoustical noise can be seen.

Figure 7 shows simultaneously the inputs and outputs of the voltage regulator system operating in slit control. One can see a 60Hz ripple on the beam current. The slit difference signal is again equivalent to a 100V RMS voltage fluctuation. The corona loop has received a gain factor of 20 within the control circuitry, but the total loop gain is over 1000 for this figure. This higher stable loop gain for the corona loop is permitted by the complimentary interaction of the fast loop, provided the fast loop response is as described here.[4] The diode current is shown at the bottom, with a loop gain of about 200. With this gain the diode signal gives a very accurate picture of the residual terminal voltage fluctuation.

We have been quite pleased with the operation of this new voltage regulation system. The essential new features have been the very good slit preamplifiers, proper shaping of various frequency responses to maximize stable gains and make the two control loops compatible, and careful attention to extraneous noise sources.

There is at least one other area of importance which we have not given attention to during this project: The position stability of the beam on the object slits. For a variety of reasons (inclined field tubes, column breakdowns, low energy optics instabilities) stability on the object slits can be poor. Because the energy analysis system is symmetric about the analyzing magnet the energy stability of the beam will then be degraded, even if the voltage regulator system has perfectly stabilized the beam on the image slits. Additional, independent control loops involving the object slits and steerers at the low-energy end of the accelerator are required in principle for the best, reproducible beam energy resolution.

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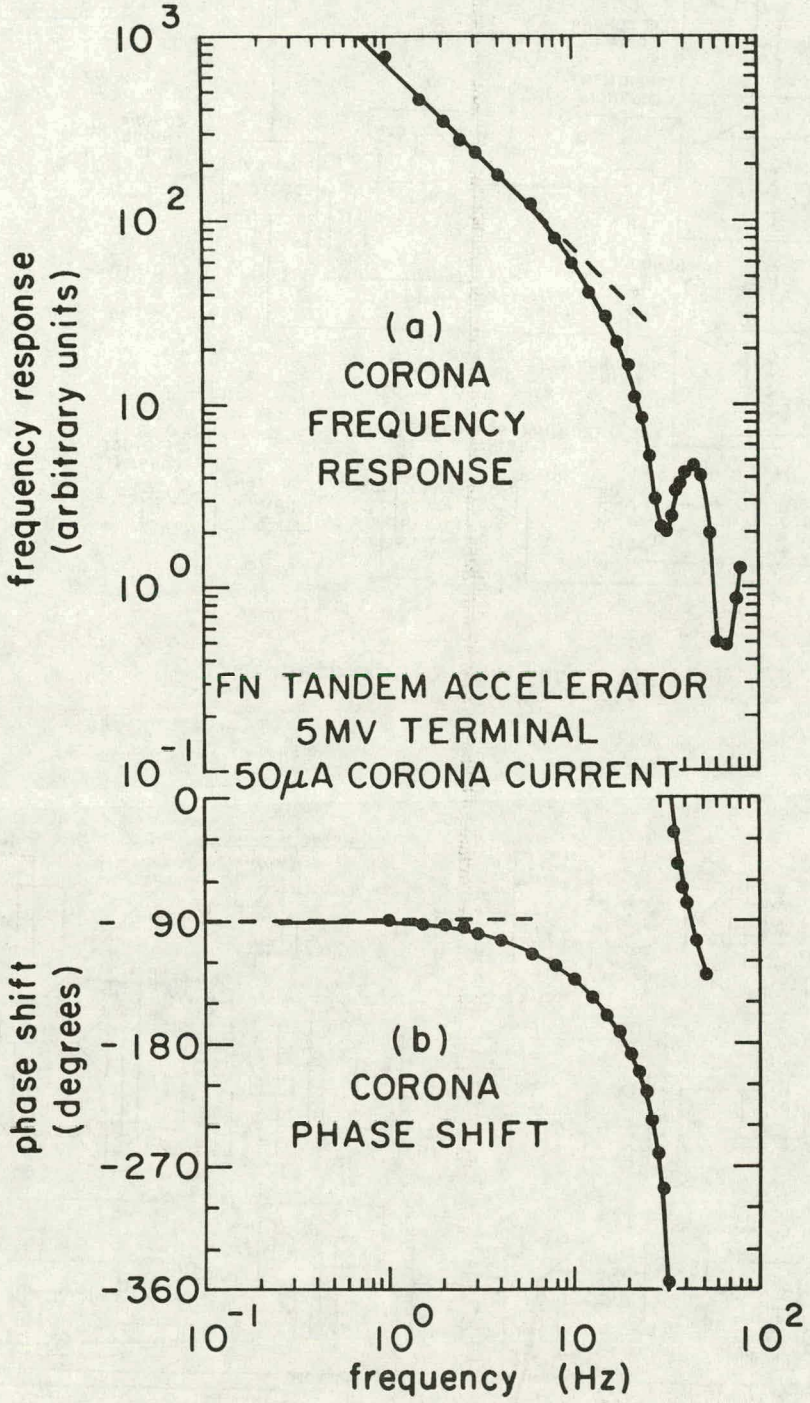

Fig.1. Corona regulator frequency response (a) and phase shift (b).

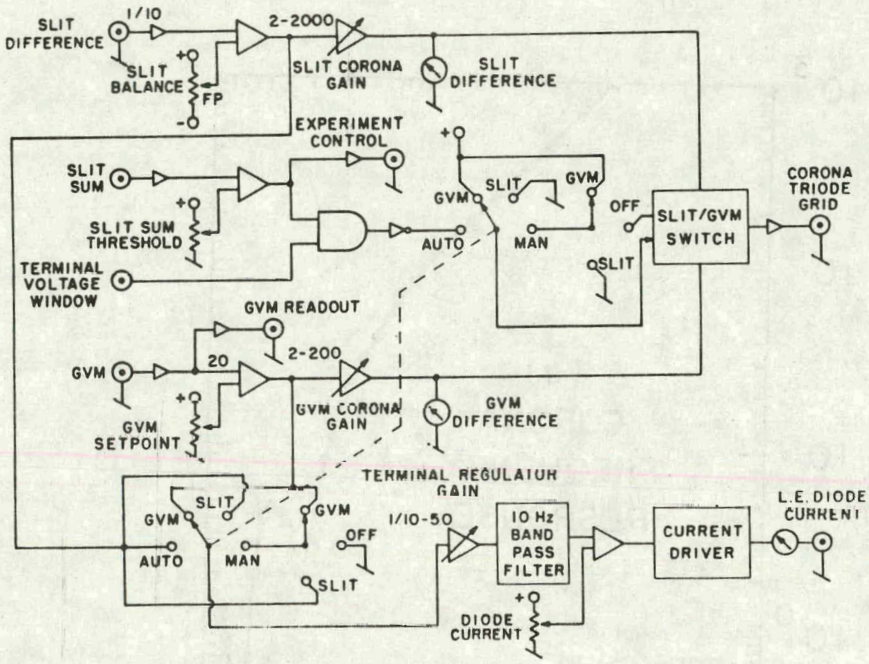

Fig. 2. Voltage regulator block diagram.

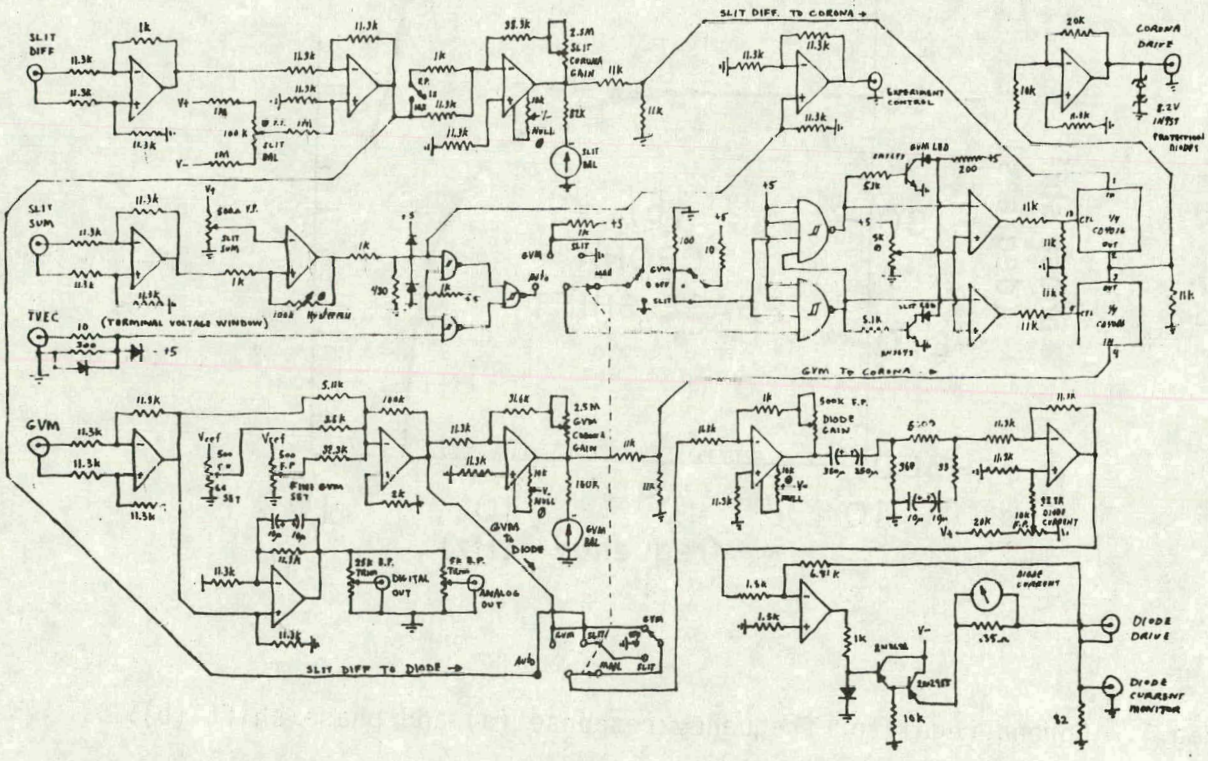

Fig. 3. Voltage regulator schematic.

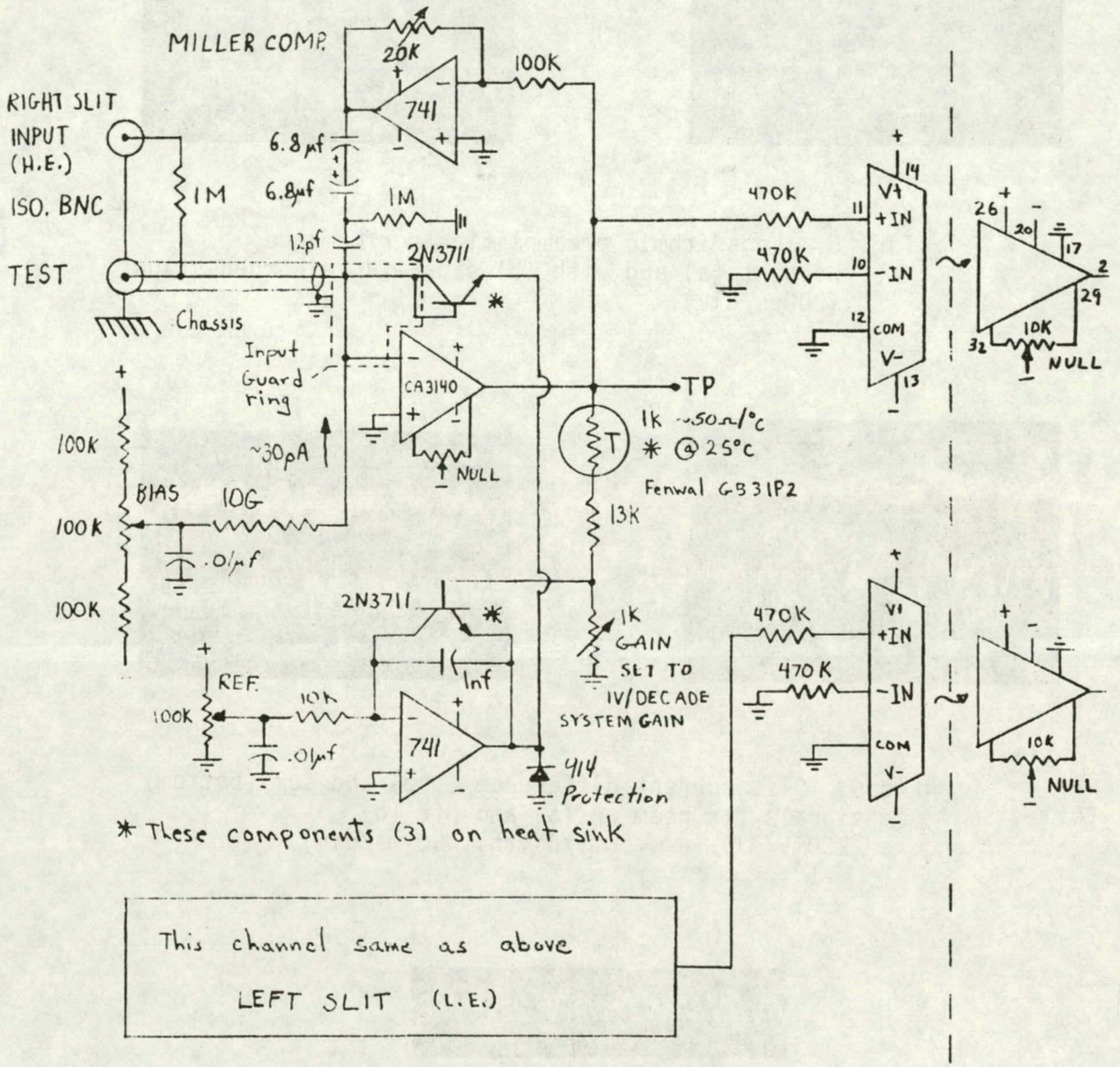

Fig.4. Logarithmic slit preamplifier.

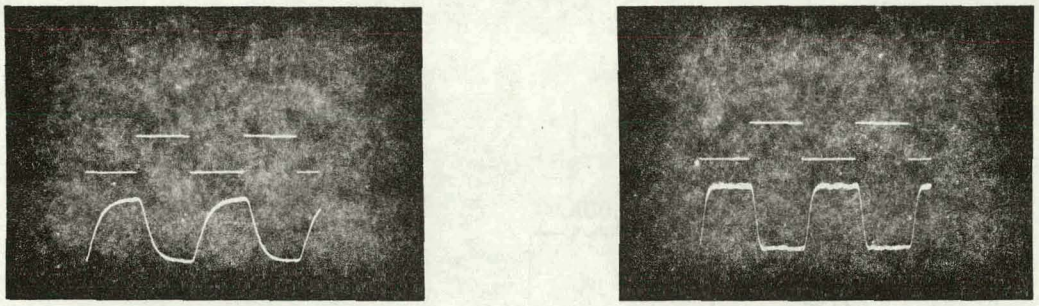

Fig. 5. Logarithmic preamplifier performance without (a) and with (b) capacitance compensation (200μs/div).

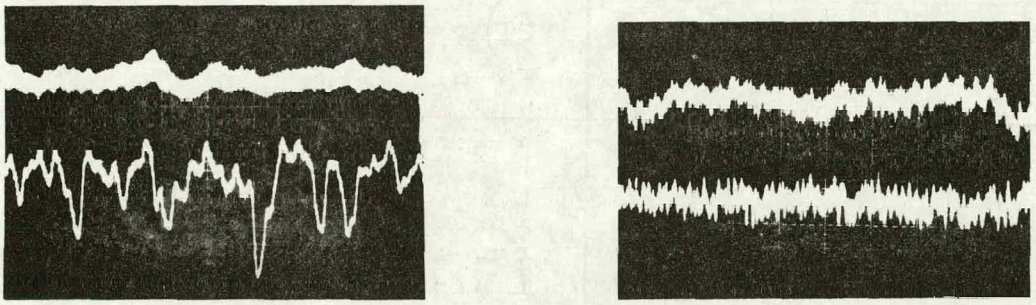

Fig.6. Slit current difference (top) and sum (bottom) signals for beam on (a) and off (b) (200mV/div.(a), 50mV/div(b), 50 ms/div).

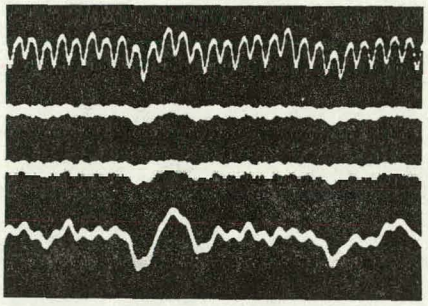

Fig.7. Voltage regulator inputs and outputs: Top-slit sum (500mv/div), second-slit difference (500mV/div), Third-corona grid (10V/div), bottom-LED current (all 50ms/div).

Recent Improvements on Generating Voltmeters

by

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University of Washington
Nuclear Physics Laboratory

The generating voltmeter used with electrostatic accelerators is basically a motor driven rotor positioned at the pressure vessel wall. The rotor shadows or interrupts the electric field induced on insulated segments of a disc called stators. Typically the unit appears as in Fig. 1. Typical rotor and stator shapes are shown in Fig. 2. Alternate stator segments are connected together producing two signals 180° out of phase. The rotor with half the number of segments will shadow and expose alternate stator segments as many times per revolution as there are rotor segments. The waveform is triangular when fed into a high-impedence circuit.

I had previously reported a method for adding the two signals after they were referenced to ground potential[1]. The process of adding retains more of the high-frequency information than the commonly used rectification filter method does. In our efforts to improve the high-frequency response, we found that the circuit which provided the reference potential introduced spikes. We also found problems with the triangular waveform caused by a finite stator spacing. The rotor would start uncovering one stator set before the next set was starting the shadowing process.

To improve the high-frequency signals one of my colleages suggested[2] we do two additions, one of them before referencing the signals to ground to use for the high-frequency response only. The second addition provides the same signal as before, a dc signal for low-frequency control of the corona system.

The problem of the stator spacing was solved by changing the rotor shape to nearly sinusoidal. The rotor was machined to the shape of the following equation:

$$R = A + B \cdot \sin^2(N\theta),$$

where R is the radius in inches, A is the constant inner radius in inches, B is the amplitude of the rotor segments in inches, and N is half the number of rotor segments. A typical rotor shape is shown in Fig. 3.

The machining of the rotor must be as precise as the required precision of regulation. To accomplish equality of the rotor shape, both faces of the rotor segments were machined with the same cutter. We used a single cutting edge ground to the desired shape of the segment. The tool resembles a fly cutter. The rotor was mounted on an arbor held horizontally by a dividing head on the table of a vertical milling machine. Thus the final equality of segment edges is limited only by machine limitations.

The spiking reflected back to the first adding circuit from the second adding circuit was isolated by a buffer amplifier. The final circuit is shown in Fig. 4. Test results of the high-frequency adding circuit are shown in Fig. 5.

Figure 5 was taken with a test plate one-half inch in front of the new generating voltmeter. The plate was at 90 V with an audio signal of 20 V amplitude superimposed on it. The top and bottom traces are the two stator signals, the motor's rotor frequency showing the direct current component. The higher-frequency audio signal is also present. The center trace is the result of adding the two stator signals and shows only the audio component.

We also developed and installed an energy control system to provide the following options: 1) Control on the image slits only, 2) Control on the GVM only, or 3) Control in an automatic mode that switches between image slit control and GVM control depending upon the total current falling on the slits. A digital voltmeter reads the terminal voltage. The digital output is compared to high and low digitally-selected energies. The energy controller can return to image slit control only if the terminal voltage is within the selected energy window. A signal is also provided to stop data collection if the image slits are not controlling or if the terminal voltage does not fall within the selected window.

Two types of control signals are used. Low-frequency control is via the corona needles and corona tube. Higher-frequency control is via an optically-coupled link to the stripper bias or terminal-housed regulator. The corona control is the portion that is multiplexed in the automatic mode. While the stripper bias regulator is switch selected, we do provide higher-frequency input from either the image slits or the GVM.

The unit has been in use for about ten months. After working out some initial problems, the experimenters find the system extremely useful in reducing unwanted background, and in assuring the heavy ion experimenters that the data is taken with the correct charge state and energy beam. A block diagram of the device is shown in Fig. 6.

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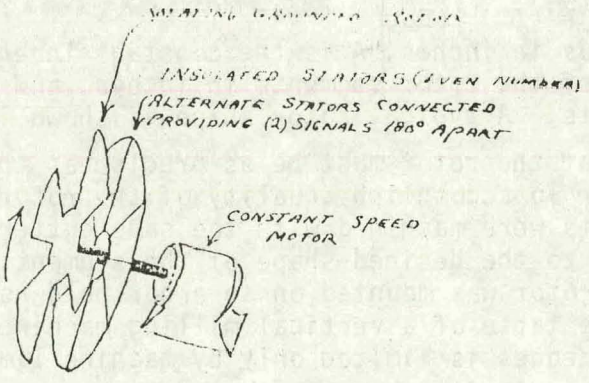

Fig. 1.

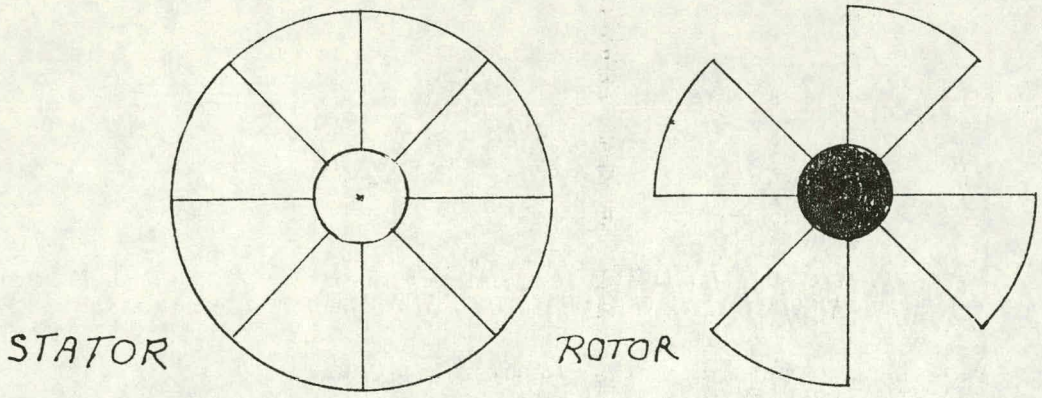

Fig. 2.

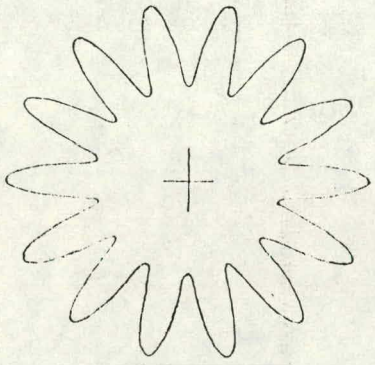

Fig. 3.

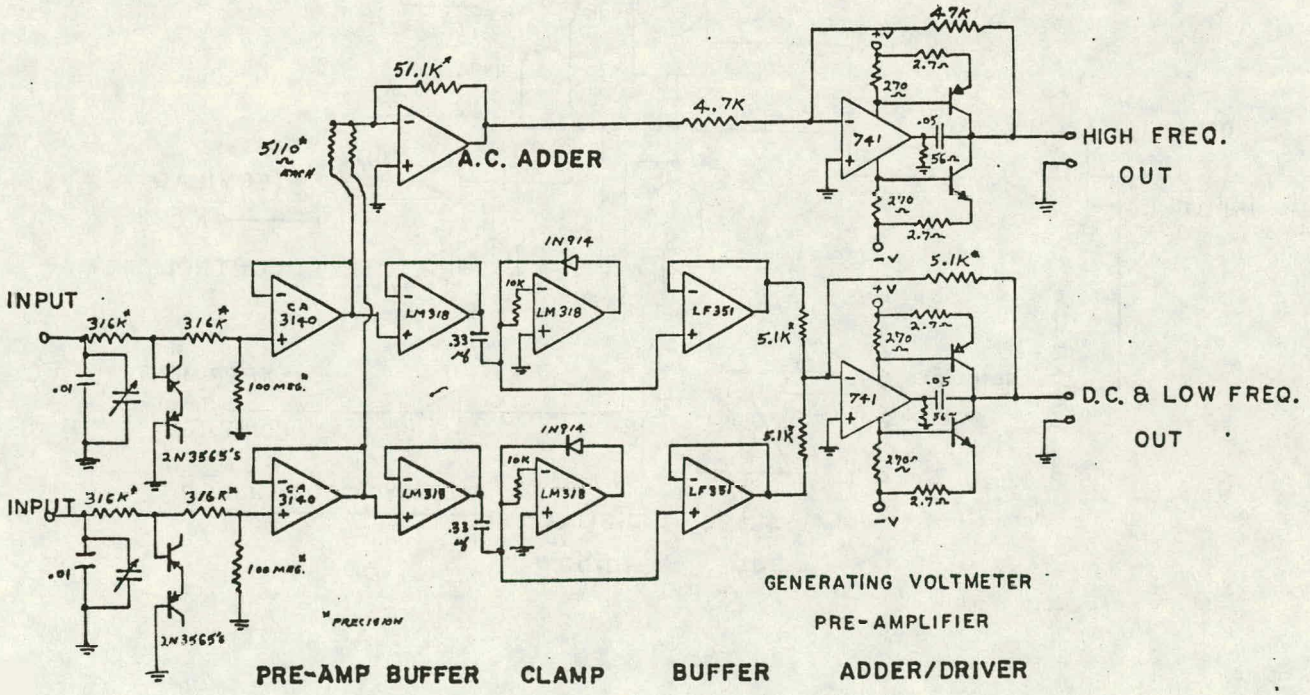

Fig. 4.

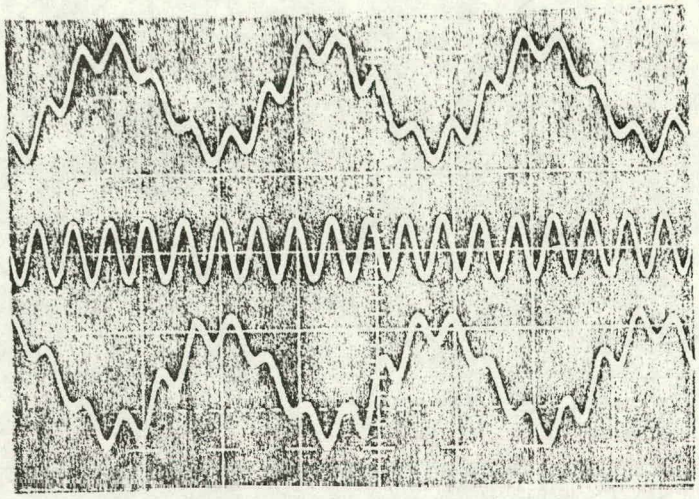

Fig. 5.

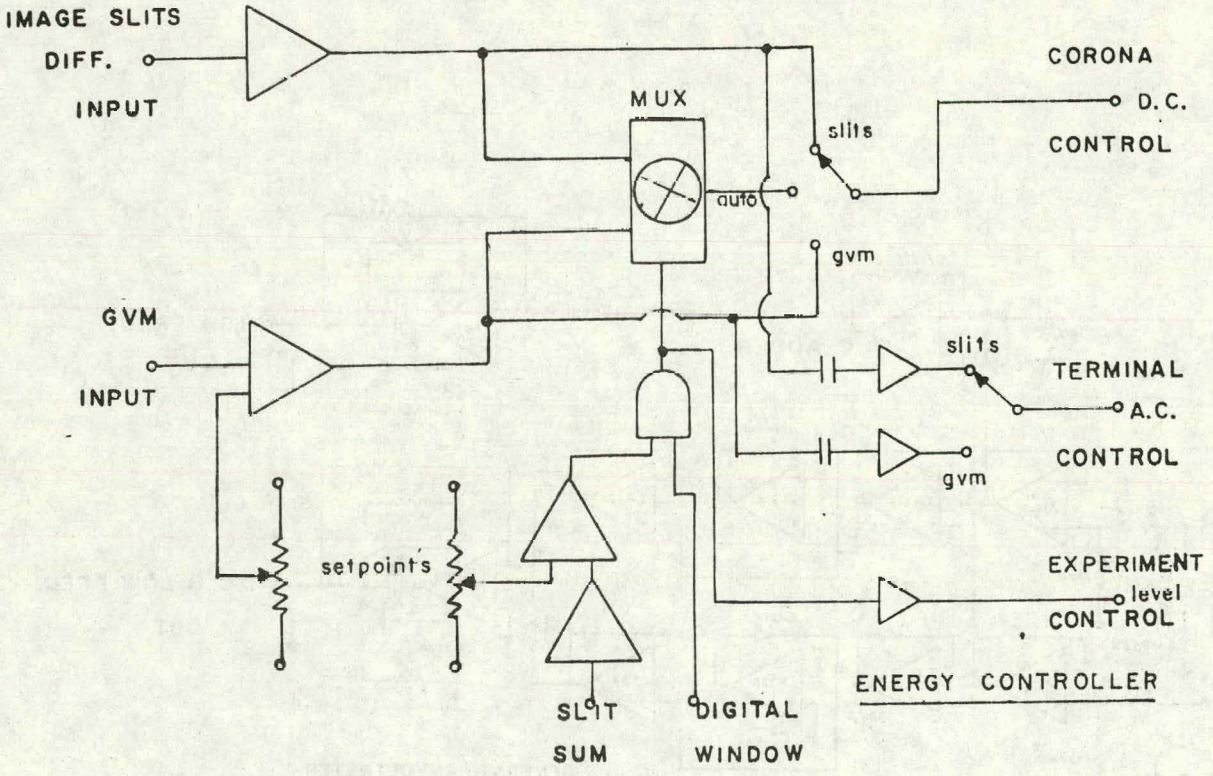
IMAGE SLITS
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CONTROL
GVM
INPUT
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A.C.
CONTROL
gvm
setpoints
EXPERIMENT
level
CONTROL
ENERGY CONTROLLER
SLIT
SUM
DIGITAL
WINDOW

Fig. 6.

Noé: This circuit detects an ac component, so is that similar to a corona mushroom?

Fauska: That's right. I had tried over the years to get a signal from the corona mushroom but I ran into problems with phase shifts and other things.

Noé: Do the circuits described by Tom Trainor use that information in the feedback loop?

Fauska: Yes, this is an input to Tom's circuit.

The Performance of the Carbon Stripping Foils in the Argonne FN Tandem

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Abstract

Carbon stripping foils produced by the glow discharge cracking of ethylene were produced and the foils were tested in the Argonne FN tandem accelerator. The results are presented and the characteristics of stripping media are discussed.

I. Introduction

Gaseous and solid electron stripping media are used in tandem electrostatic accelerators for ion polarity reversal. Additional effects of the stripping medium on the transmitted ion beam are not desired. The ideal stripping medium is characterized by the production of a positive ion beam with a high average charge state and with a small energy straggling. Also, additional divergence introduced into the beam by multiple scattering should be small and the stripper should have an infinite or very long lifetime.

The need for a high mean charge state to achieve maximum energy gain often eliminates gaseous media from consideration as strippers for heavy ions, $A \gtrsim 16$. Charge-state distributions for a number of heavy ions have been compiled by Betz[1] for O_2 and carbon strippers. A density effect strongly favors solid media[2,3].

The energy straggling introduced by a stripping media should be as small as possible so as to maintain the excellent beam quality available from electrostatic accelerators. In systems utilizing beam bunching, such as the Argonne superconducting linac, this requirement is especially crucial. Additional energy straggling introduced by the stripper is translated into a longer ion pulse at the nuclear target. For arc-evaporated carbon foils used as strippers, the energy straggling has been found to increase as $t^{\frac{1}{2}}$, where t is the thickness of the foil[4]. The technique can be used inversely to determine the thickness of carbon foils when the energy straggling is calibrated with standard foils of known thickness. Although the absolute energy straggling may differ for different carbon structures, the thickness variation is expected to remain.

The transmission of ions through the accelerator is dependent on the emittance of the ion beam and the electrostatic focusing of the accelerator tubes. Multiple scattering of the ion beam by the stripper increases the

*This research was performed under the auspices of the U.S. Department of Energy.

divergence of the beam. The net contribution to the transverse emittance is minimized by placing a waist at the stripper. Since the multiple scattering increases with projectile mass and stripper thickness, media only as thick as necessary to produce charge state equilibrium should be used. Self-supporting solid strippers, generally carbon films, are always thicker than necessary for charge state equilibrium at the terminal energies of existing electrostatic accelerators. A frequently observed characteristic of carbon strippers is a decrease in transmitted beam intensity as the foil irradiation time increases. Presumably, this effect is due to the thickening of the foil by ion beam cracking of residual hydrocarbons, although the effect has been observed in ultra-high vacuum systems with very low hydrocarbon partial pressures. The hydrocarbons may be liberated by beam heating of apertures in the vicinity of the foil[4,9].

Whereas a gaseous stripper can be continuously renewed, solid strippers are subject to radiation-induced damage and eventual destruction. The precise mechanism for the destruction is not known, but the macroscopic features of the process have been widely observed. The thin film of carbon becomes taut around the edges while the beam spot itself acquires a mirror-like appearance. The tension in the foil increases until rupture finally occurs. Several authors[5,6,7] have suggested that the time to rupture of carbon foils is dependent on the integrated radiation dose and varies inversely with the nuclear stopping power, S_N. Dobberstein et al.[5] have related this to the energy required to produce a Frenkel defect in crystalline solids. Livingston, et al.[8] have suggested the reduced lifetime, τ, which satisfies the empirical formula: $\tau(p\mu a\ min/mm^2) = A \cdot E^{1.15}(Mev/amu)$. A varies from $A \approx 60$ for ^{14}N to $A \approx 5$ for ^{58}Ni. They found that at low velocities the lifetimes of carbon strippers decreased faster than $1/S_N$. Because the lifetime of arc-evaporated carbon stripping foils can become very short for heavy ions, considerable effort has been devoted to the investigation of the properties of carbon strippers prepared by other methods.

II. Testing Carbon Foil Lifetimes

The comparison of the effectiveness of different stripper foils requires a uniform testing environment. The lifetime of carbon stripper foils is known to depend upon many variables including the ion species, energy, and the intensity, as well as the accelerator characteristics such as the beam position, stability and the size of the beam at the stripper. Foil-mounting techniques such as slackening the film[10], mounting a grid[12,10], and moving the film[11], have also been shown to effect the lifetime. Although stripper performance is only directly measurable in beam tests in the terminal of a tandem accelerator, many experiments have been done after acceleration using the positive exit beam. This type of test allows excellent control over parameters such as the beam spot size, foil position, and the residual pressure. Extensive instrumentation is possible, allowing the simultaneous measurement of many attributes, but the time available for such tests is usually very limited. In tandem terminal tests, however, time is not a critical factor if the beam time can be shared with other experiments. Although the foil position is remote and instrumentation is difficult, the dose-response curve of the foil is directly determined in terms of the quantities of primary importance: the useful lifetime, the beam transmission, and the energy straggling.

The lifetime of a carbon foil has been variously defined by different investigators. In tests performed in beamline scattering chambers, foil rupture has often been the criteria, since it is easily observed and marks a definite transition. In terminal tests, however, incomplete foil breakage will only cause a reduction in beam intensity or possibly a decrease in voltage stability or beam directional stability. We have chosen to measure the lifetime as the dose required to reduce the transmission of the foil and accelerator to 70% of its initial value. The transmission is measured as the ratio of the injected current to the analyzed current of the most probable charge state. Frequently, experiments cannot tolerate a larger fluctuation in intensity.

III. Foil Testing Facility at ANL

To enable foil testing while sharing beamtime with other experimenters, a microcomputer controlled stripper indexer was mounted in the terminal of the Argonne FN tandem. The system is shown in Figure 1. The indexer operates similarly to the one developed at Munich[14]. A punched mylar tape driven by an NEC[15] 230 position changer is read and the position is transmitted by the computer on a fiber-optic link. A base computer decodes the signal and displays the position. Figure 2 is a block diagram of the system. The indexer allows complex foil loadings with many foil types, yet particular foils can be located and tested or used as required. To simplify data acquisition another computer is used to monitor and control the Faraday cup positions and record the measured currents.

Energy straggling measurements can be made using the beam bunching system of the Argonne superconducting linac which is described elsewhere[16,4].

IV. Production of Cracked Ethylene Foils

During the past year the primary emphasis of our foil development program has been the reproduction of the encouraging results reported by Daresbury[17], Chalk River[18] and Oak Ridge[19] for foils produced by cracking ethylene in a glow discharge. Figure 3 shows the apparatus, which is modeled closely upon that of J. Gallant. Table 1 compares the techniques used by the several groups. The techniques appear equivalent in most respects, although the ORNL and Daresbury groups report significantly lower deposition rates than the CRNL and ANL groups. We have used thinner release agents than the other researchers. The use of a thinner release agent was prompted by the occurence of sparking in the discharge when thicker layers were used. The sparking was reduced by first cleaning the substrate with a preliminary pure Ar discharge. The Ar discharge was found to reduce the carbon deposition rate of the subsequent 90% C_2H_4 + 10% Ar discharge, presumably by changing the surface structure of the NaCl. The Ar cleaning was only done for a few seconds to reduce this effect. Figure 4 shows typical voltage and current characteristics of two successive 5 second glow discharges of the ethylene -Ar mixture following a brief pure Ar cleaning discharge. The small amount of sparking evident in the first discharge is completely absent in the second. These two discharges produced a foil with areal density of about 10 $\mu g/cm^2$ as estimated from transmission measurements.

V. Results and Discussion

Preliminary results of testing these foils in the Argonne FN tandem nave been disappointing. Table 2 shows a comparison of the results obtained by the different groups and the testing condition under which they were acquired. The wide disparity in testing technique makes the comparison difficult, but three of the groups report large increases in stripper lifetimes with glow-discharge foils. Our failure to reproduce these results suggests that factors important to the success of the process remain unknown. The presence or absence of trace contaminants is a possibility, or perhaps the substrate or release agent differ in some significant but subtle respect. We hope to be able to delineate these differences after additional work. Future plans also include a direct comparison of the performance of carbon strippers supplied by other laboratories by testing in the Argonne FN tandem.

VI. Acknowledgements

The authors wish to thank J. Gallant for his help in designing the glow discharge chamber, and Ludwig Roher for many helpful discussions and suggestions concerning the design of the terminal foil indexer. We also wish to thank Bruce Nardi for electronic assistance and the tandem operators for their continuing effort on this project.

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[Editor's note: Discussion of this presentation was postponed until after the two following reports by Neil Burn and Charles Jones.]

TABLE 1. Comparison of glow discharge techniques. The CRNL data is from Ref. 18, the ORNL data is from Ref. 19, and the Daresbury data is from Ref. 9 and 17.

	ANL	CRNL	ORNL	DARESBURY
Cathode Area/type (CM^2)	161 Metal	128 Metal	58 Metal	25 Glass
Cathode-Anode Spacing (CM)	8	7.3	9	7
Gas	90% C_2H_4 10% AR	90% C_2H_4 10% AR	90% C_2H_4 10% AR	90% C_2H_4 10% AR
Pressure (Torr)	0.08 Flowing	0.08 Static	0.10 Flowing	0.10 Flowing
Current Density ($ma \cdot cm^{-2}$)	1.5	1.3	1.7	1.0
Release Agent ($\mu g \cdot cm^{-2}$)	2-10 NACL	15 NCAL	15 NACL	- NACL
Deposition Rate ($\mu g \cdot cm^{-2} \cdot S^{1}$)	1.0	1.0	0.1	0.08
Discharge Voltage (KV)	3.0-3.5	2.5	1.9-2.8	1.5-3.5
Estimated Thickness ($\mu g \cdot cm^{-2}$)	5-10	5-10	2-10	5-15

TABLE 2. Comparison of glow discharge stripper foil test conditions and results. Data is from same references as Table 1.

	ANL	CRNL	ORNL	DARESBURY
Test Beam	$^{58}_{N}$	$^{127}_{I}$	$^{35}_{CL}$	$^{40}_{AR}$
Beam Intensity (μa)	0.5	1.0	0.35	0.7
Beam Energy (MEV)	8.0	4.9	10.0	4.8
Terminal Test	Yes	Yes	No	No
Lifetime Definition	$.7I_o$	$.5I_o$	Rupture	Rupture
Lifetime Increase Factor	1	10-20	5-20	25

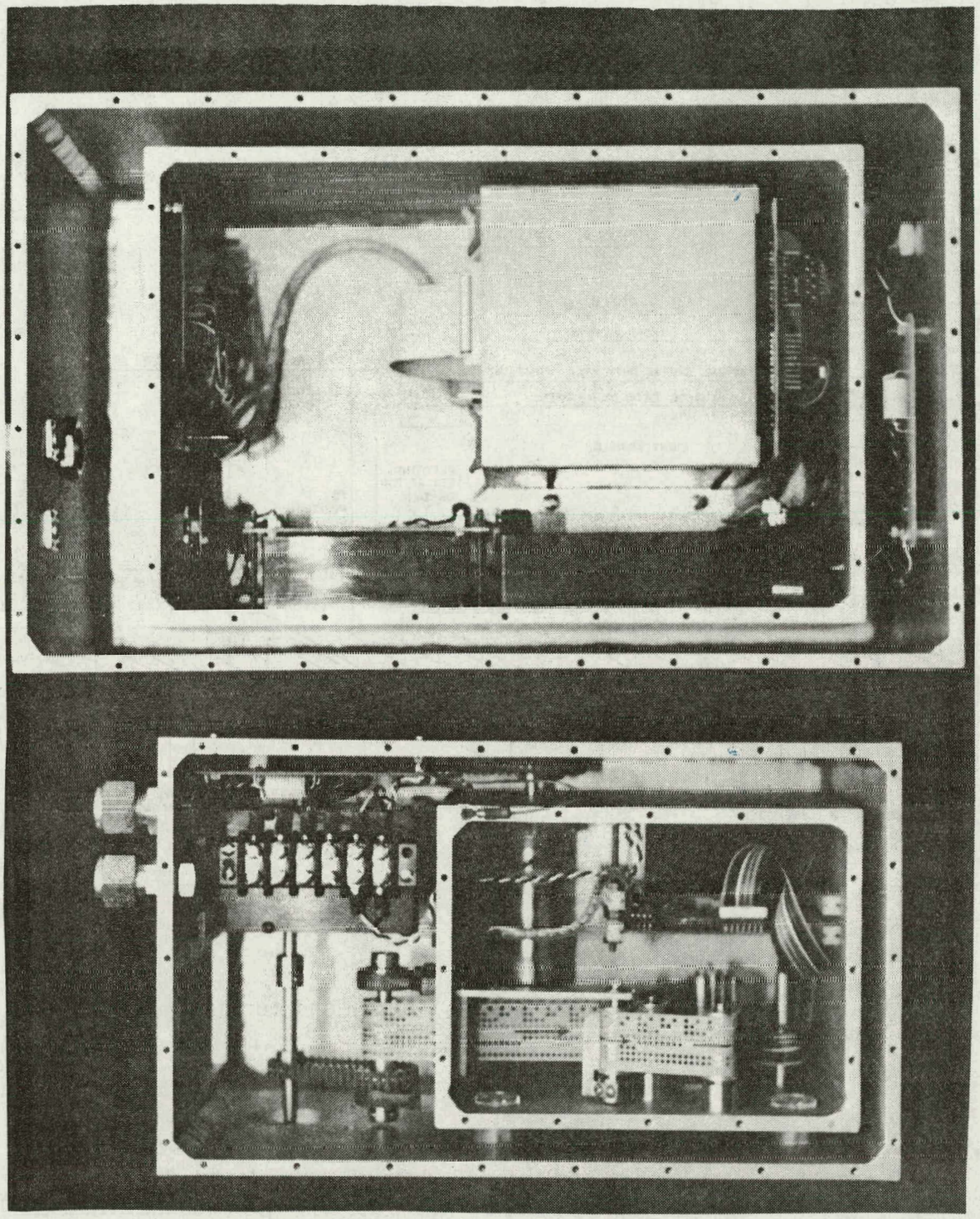

Fig. 1. The terminal foil indexer and the computer in their double shielded boxes showing the mylar punched tape.

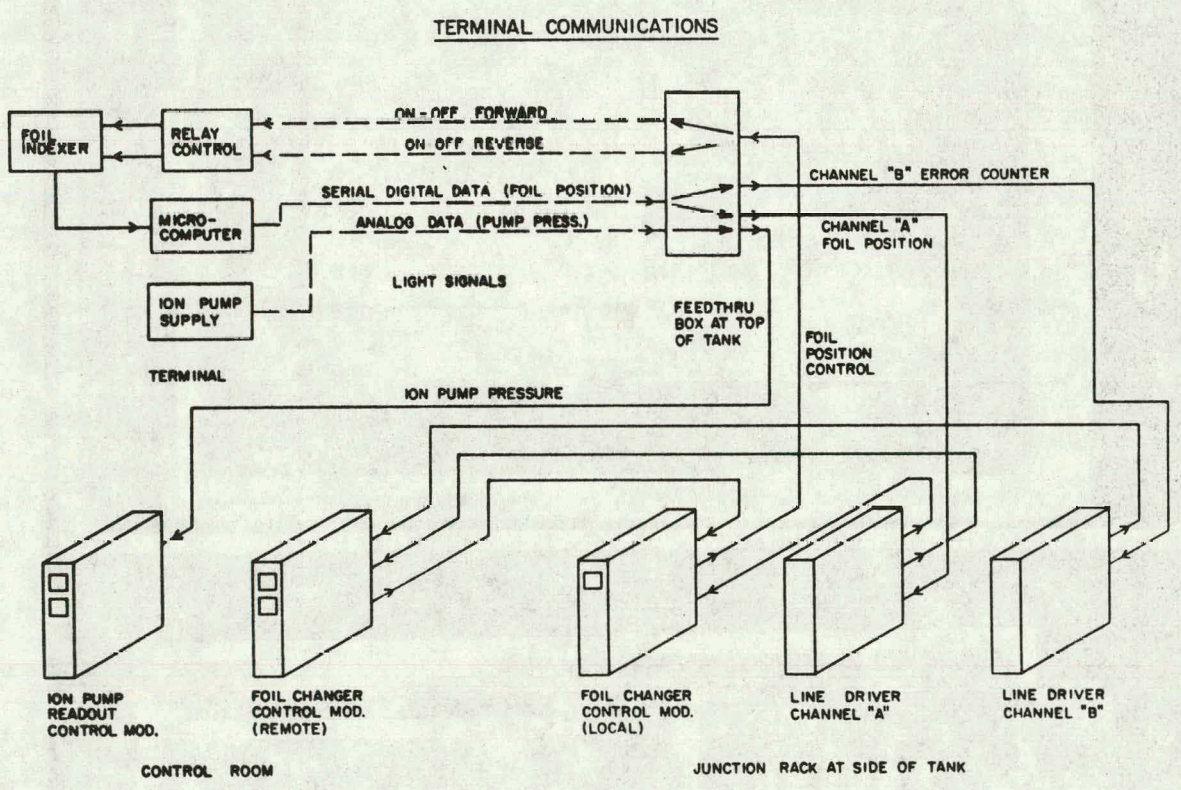

Fig. 2. Block diagram of the foil indexer and terminal communication system.

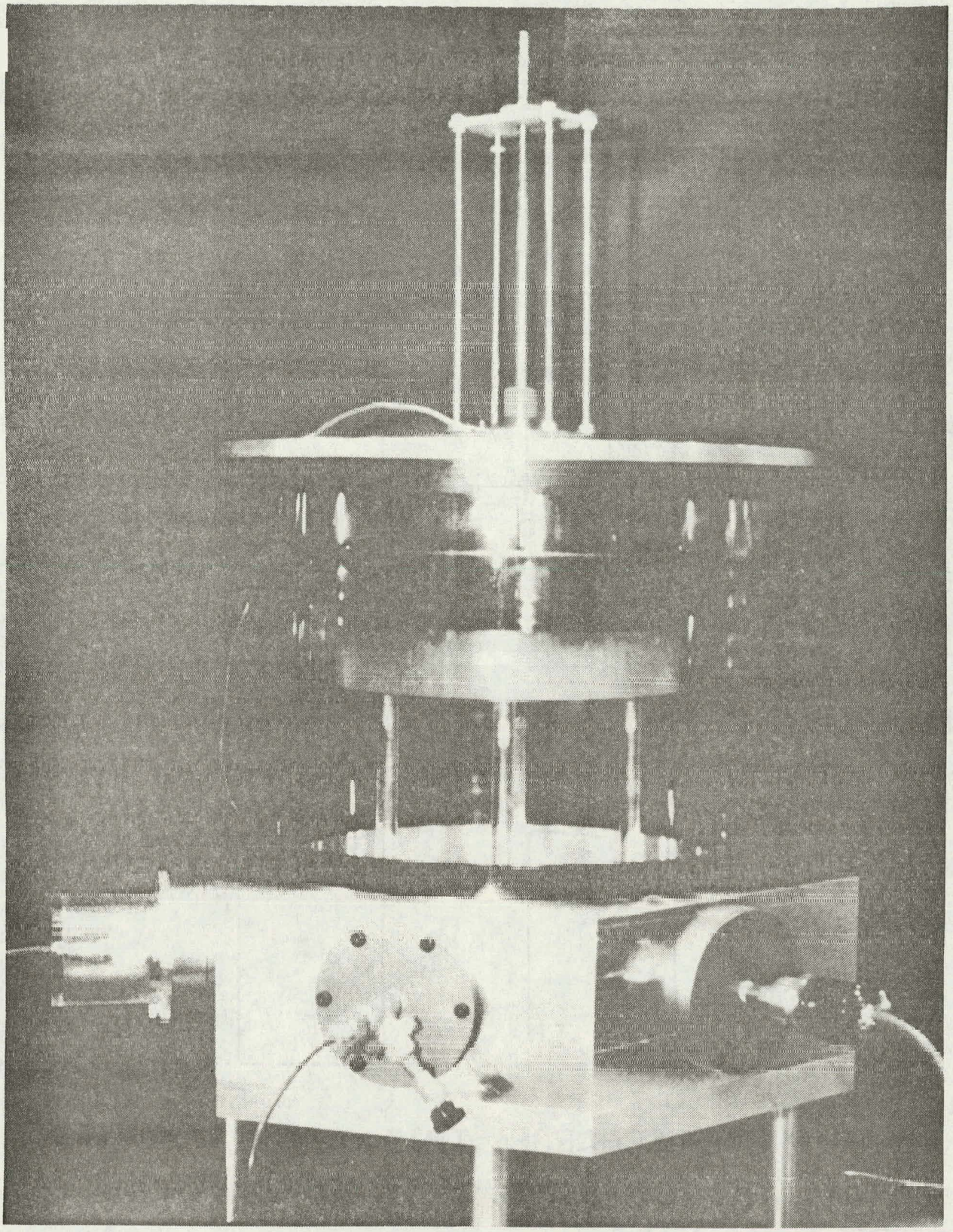

Fig. 3. The apparatus used for the production of glow discharge carbon foils.

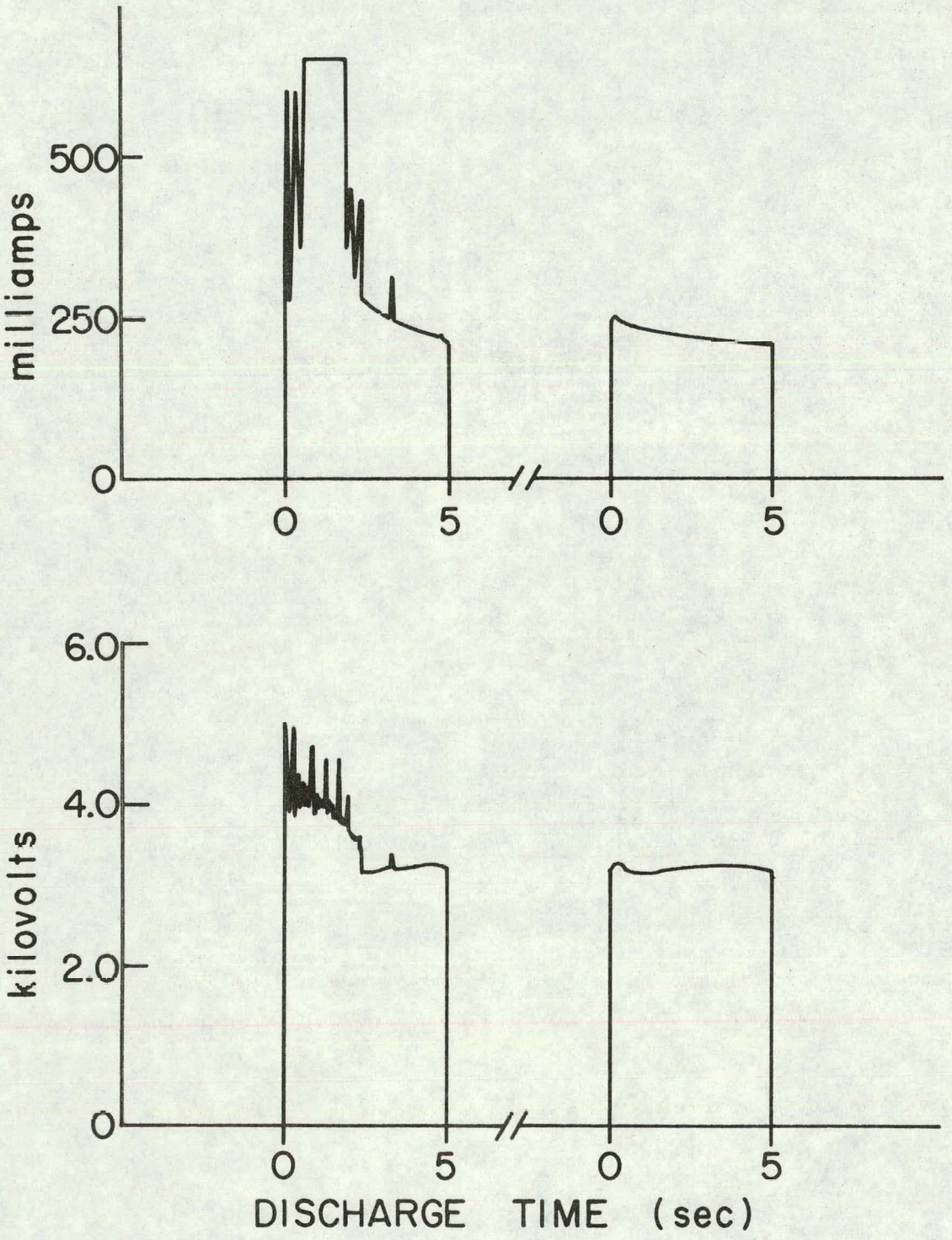

Fig. 4. Typical voltage and current characteristics of a glow discharge.

A Simple Ethelyne Cracking Apparatus for the Preparation of Carbon Films

by

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Presented by Niel Burn

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ABSTRACT

Long lifetime carbon stripper foils are essential to the operation of heavy ion tandem accelerators. The glow discharge cracking of ethylene gas produces carbon films that are more resistant to radiation damage than those prepared in the conventional manner. A modified form of the glow discharge apparatus and the preparation and the characterization of these foils are described.

INTRODUCTION

Tandem electrostatic accelerators make use of negative ions on the "upward" journey to high voltage terminal and positive ions on the "downward" leg. The positive ions may have many electrons removed, in some cases even all of them, and generally the more removed the better because for a given terminal voltage the higher the charge the greater the energy. The process of changing the incoming negative ions to an outgoing (multiply charged) positive ion is known as "stripping". It is accomplished by a thin layer of matter called a stripper. At first stripping was done in a gas but now both gas and solid strippers are used. Solid strippers have the advantage of giving a higher average positive charge and hence higher output energies, or often more importantly, higher output intensities at a given high energy and are thus crucial in many experiments. However, they

suffer from a serious disadvantage, that of radiation damage which, since they must be very thin (5 $\mu g/cm^2$) to avoid loss of beam particles and energy definition, causes them to break. The rate of damage increases with the mass of the ion and is such that in typical tandem operation, a standard carbon film (carbon is universally used for the purpose) lasts for weeks in a hydrogen beam but breaks in an iodine beam in the order of one minute.

For more than a year the tandem accelerator at the Chalk River Nuclear Laboratories has been using relaxed cracked ethylene foils[1] prepared by a procedure that was first employed at the Daresbury Laboratory in England[2]. The development of these foils has increased the foil lifetime in an iodine beam to about one hour (Figure 1). with the result that the use of foils for stripping of ions as heavy as iodine is now practicable, whereas the former limit was in the iron region.

APPARATUS

Ethylene gas can be readily dissociated in a high voltage discharge between two electrodes which results in carbon being preferentially deposited at the cathode. The apparatus shown in detail in Figure 2 has been used successfully at Chalk River during the last year to produce longer lifetime carbon films. It consists of two 140 mm diameter metal electrodes spaced 100 mm apart in a vacuum chamber with gas inlet and high voltage feedthrough. The electrodes are isolated from the discharge so that only the metallic face of the electrode is exposed.

Carbon is deposited on the surface of the cathode. Our procedure varies from that of others in that a metal collector plate and constant gas pressure is used. The cracking is performed for a very short period of time (5 seconds) in a DC discharge so that the increase of pressure due

to the breakdown of the gas and the related drop in current is not significant. Earlier experiments have proved that films prepared by RF discharge are less resistant to radiation damage than those prepared by the DC glow discharge method.

PROCEDURE

A chromium plated brass disc, 130 mm in diameter, is coated with a sodium chloride film, 10 $\mu g/cm^2$ thick, and transferred to the cracking apparatus. The chamber is evacuated to a pressure of 1.33×10^{-3} Pa (10^{-5} torr) and the ethylene leaked into the system to a pressure of 10.7 Pa. The ethylene is cracked at a potential of 2.5 kV for a predetermined period of time, usually 5 seconds.

DETERMINATION OF FOIL THICKNESS

1. Energy loss of copper beam in a carbon film

A thickness measurement in *situ* in the tandem terminal was performed in the following way. A beam of 81 MeV Cu^{7+} produced by gas stripping was analysed at a terminal voltage reading of, for example, 10.1066 MV. The precise absolute calibration of the generating voltmeter (GVM) was unimportant since the thickness determination depends only on small differences. A stripping foil was then inserted ahead of the gas canal and it was found that in order to obtain the same analysed energy the voltage had to be increased to 10.1160 MV. The energy loss in the foil was thus $9.4 \times 8 = 75$ keV. From Northcliffe and Schilling, the stopping power of copper in carbon at 10 MeV is 22.9 keV/$\mu g/cm^2$ [4] so that the stripper foil thickness was ≈ 3.1 $\mu g/cm^2$ with an estimated error of about ±10%.

2. Weighing the carbon deposit

To determine directly the thickness of the carbon films, a metal disc of 135 mm diameter with a 74 mm hole at the center was placed over a 100 mm diameter aluminum disc. Ethylene was cracked for five second intervals and the gas renewed after every cracking. The disc was weighed at every second cycle. This procedure was repeated ten times. Time versus areal density of carbon deposition was plotted (Figure 3).

$$\text{Areal density in g/cm}^2 = \frac{\text{weight of deposit}}{\text{area}}$$

The calibration determined that the resulting film from a five second cracking period is ~3 $\mu g/cm^2$ the thickness being directly proportional to time, at a given gas pressure.

CONCLUSION

Stripper foils prepared by this method in combination with the foil relaxing technique[2] are now in routine use at the Chalk River tandem accelerator, typical thicknesses being 3 $\mu g/cm^2$ at the terminal and 10 $\mu g/cm^2$ at the second stripper.

Further research, for example on cracking at higher voltage and collecting carbon on heated substrates, is now being carried out by other laboratories. If that work proves successful the present technique can easily be modified to incorporate any improvements required.

The authors wish to express their sincere thanks to N. Burn, Dr. J.C.D. Milton, and Dr. H.R. Andrews for their invaluable assistance.

[Editor's note: Discussion of this presentation was postponed until after the following report by Charles Jones.]

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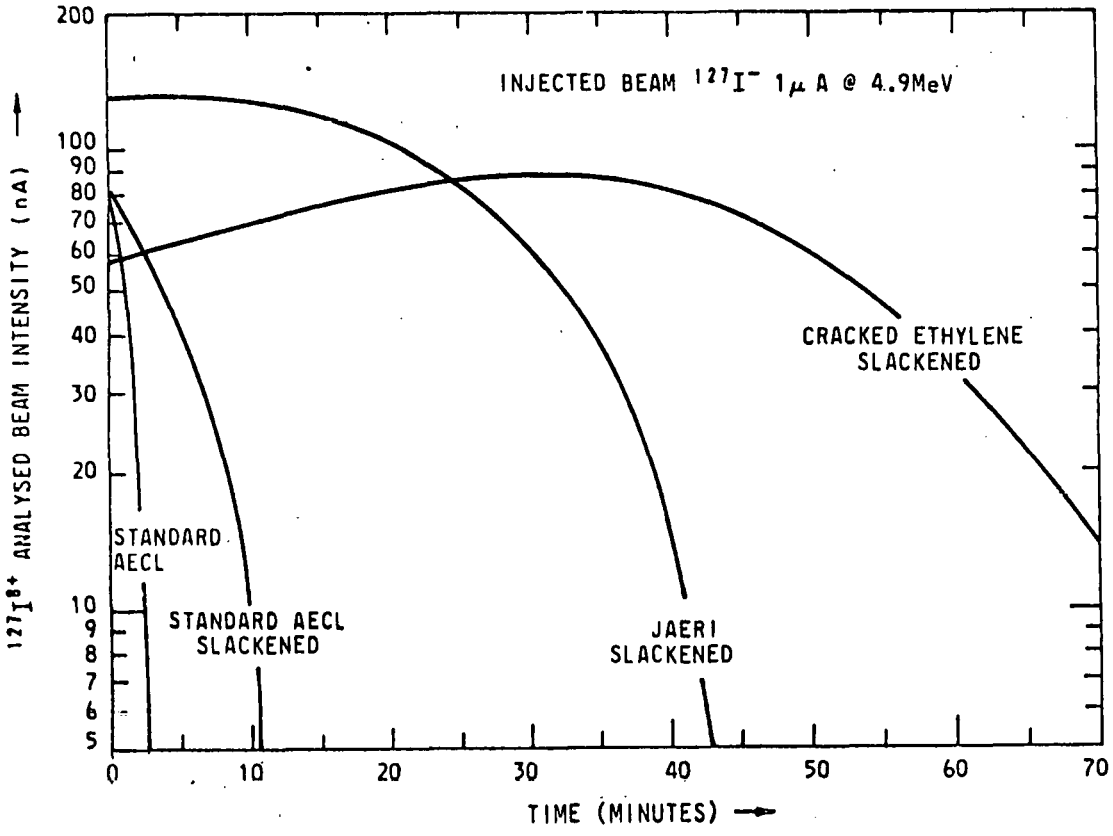

Fig. 1. Typical variation of analysed beam intensity with time for various foil strippers in the terminal of the MP tandem accelerator.

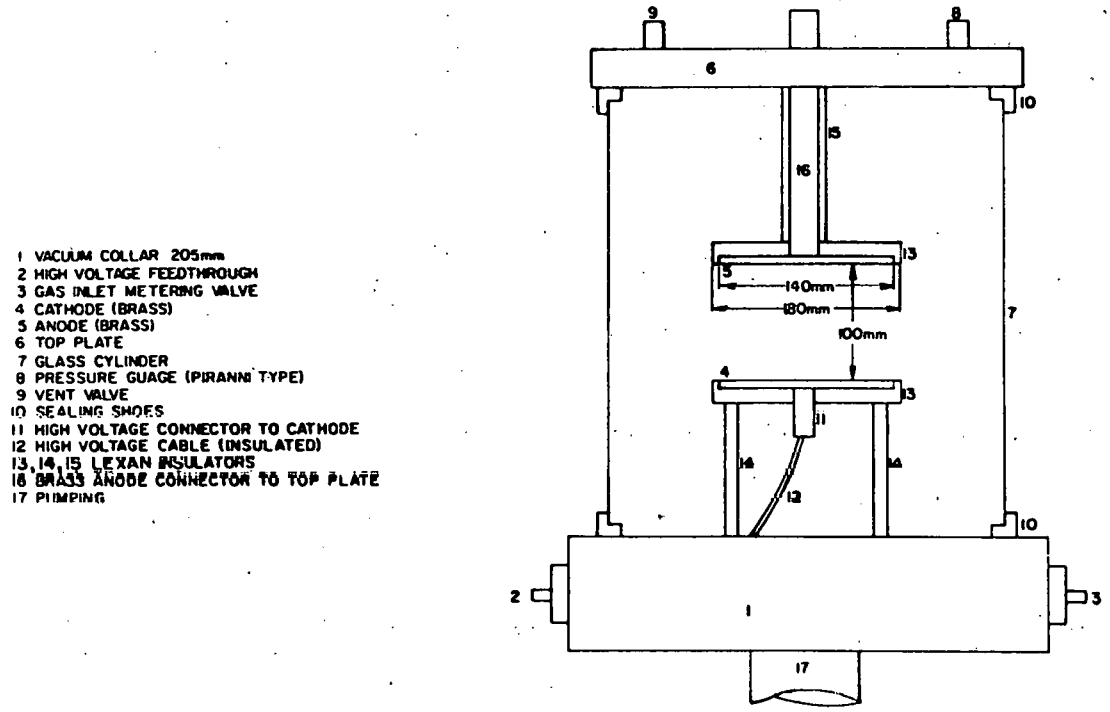

Fig. 2. Schematic of the ethylene cracking apparatus.

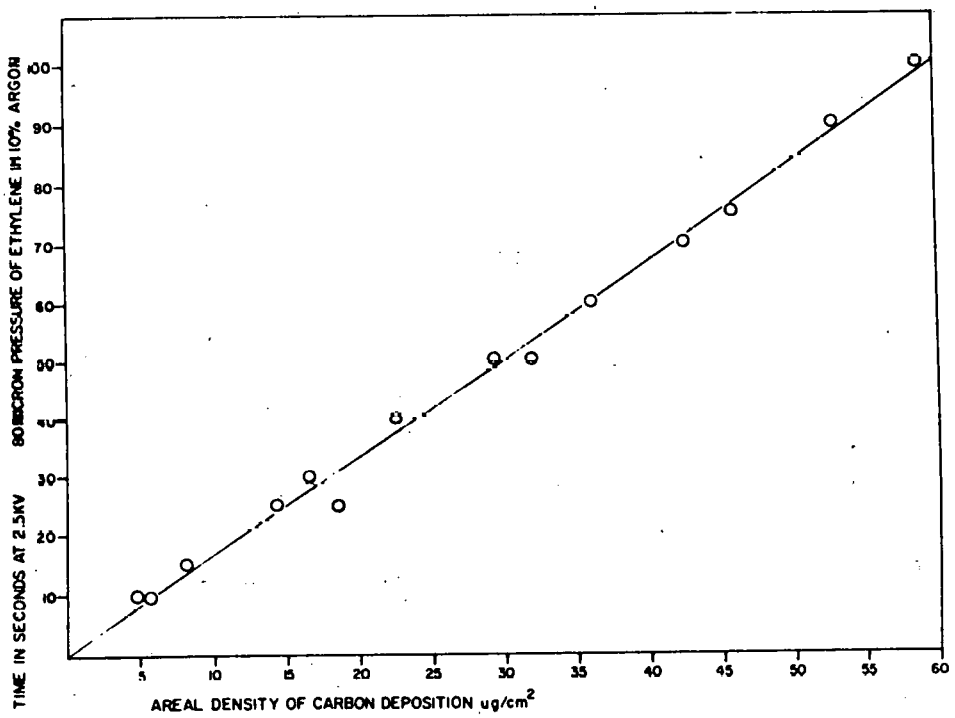

Fig. 3. Calibration of carbon thickness versus cracking time.

Notes on Stripper Foil Lifetimes*

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Last year David Weisser reported on measurements which we had done at Oak Ridge on the lifetime of carbon foils exposed to 10-MeV chlorine beams from our EN tandem accelerator. These measurements confirmed that glow discharge foils exhibit significantly enhanced lifetimes in comparison to evaporated foils and further, that glow discharge foil lifetimes showed a distinct correlation with thickness, thicker foils lasting a longer time. Although of considerable interest to us, these measurements had the disadvantage that quite a long time was required to test a foil. Specifically, the time required to break a glow discharge foil with the 0.5-pμA chlorine beam obtainable from our EN tandem accelerator was of the order of 12 hours. Therefore, we were motivated to develop a more rapid way to test foils which would still be predictive of their behavior when exposed to energetic heavy ion beams.

My talk today concerns in part what we believe to be a solution to this problem. The technique, which was developed by Ron Auble with the assistence of Dave Galbraith, is based on bombarding carbon foils with 20-keV nitrogen ions in a General Ionex Sputter Bell. This apparatus, which was developed for target fabrication, is equipped with a duoplasmatron which is the source of a heavy ion beam, usually argon, which is accelerated and used to sputter refractory materials onto substrates. For the present application, the duoplasmatron was set up to produce a 20-keV nitrogen beam and the Sputter Bell was equipped with a multiple foil holder and a suppressed Faraday cup. With this apparatus it was then possible to irradiate carbon foils with known fluences of 20-keV nitrogen ions.

The first result obtained from these measurements was that the lifetime of unslackened glow-discharge foils was of the order of 10 to 15 times longer than that for unslackened evaporated foils. (Our criterion for lifetime is rupture.) This is in good agreement with measurements at higher bombarding energies. In addition, as expected, the time required to break a foil was measured in minutes rather than hours.

Before discussing these measurements further, I will now digress and discuss a theoretical method for estimating foil lifetimes which has been developed by Ron Auble as part of this work. This method is described in detail in an accompanying paper[1] which was prepared by Ron Auble and Dave Galbraith for presentation at the Ninth World Conference on the International Nuclear Target Development Society that is being held in Gatlinburg at the same time as this Symposium. The method is based on the idea of estimating the number of displacements which occur per incident ion and then assuming that foil lifetime is inversely proportional to the product of the number of displacements per incident ion and the fluence.

Results for the Sputter Bell measurements, along with other measurements at higher bombarding energies, are shown in Fig. 1 where I have plotted experimentally observed lifetimes on the vertical axis versus corresponding theoretical lifetimes, calculated with the formulae of the following paper[1], on the horizontal

*Research sponsored by the Division of Basic Energy Sciences, U.S. Department of Energy under contract W-7405-eng-26 with the Union Carbide Corporation.

axis. To simplify this figure I have, with one exception, drawn balloons rather than error bars, but the size of the balloons is a measure of either an estimated uncertainty for a single measurement or the spread in observed values for a number of measurements. All of the measurements summarized in the figure are for unslackened glow-discharge foils. Data from Chalk River (CRNL), the Daresbury-Harewell collaboration (D-H), and the 10-MeV ^{35}Cl ORNL data were taken from a recent compilation by Robin Tait[2]. The cross-hatched balloons labeled ORNL acceptance tests are for single foils used in the acceptance tests of the Oak Ridge 25-MV tandem accelerator.

Two essential results may be noted in Fig. 1. The first is that foils fabricated by glow discharge from different gases[3] and bombarded by 20-keV nitrogen ions do not show significantly different lifetimes. The second is that the lifetime theory developed by Ron Auble appears to be a fairly good representation of foil lifetime over a mass range from ^{14}N to ^{127}I and an energy range from 20 keV to 10 MeV. A corollary of this second result is that tests of foil lifetime with keV-energy ion beams can be a meaningful predictor of lifetimes at bombarding energies corresponding to the use of carbon foils as terminal strippers in tandem accelerators.

References

1. R.L. Auble and D.M. Galbraith, following paper.

2. N.R.S. Tait, Proceedings of the Fifth Tandem Conference, held at Catania, Italy, 9-12 June 1980 (to be published in Nucl. Instrum. & Meth.)

3. With the exception of some of the Daresbury-Harewell measurements, "ethylene" on Fig. 1 means a mixture of 90% ethylene and 10% argon. The acetylene used in preparation of the ORNL foils was welding grade acetylene. Mapp gas is a mixture sold by Sears, Roebuck and Company for use as a torch fuel.

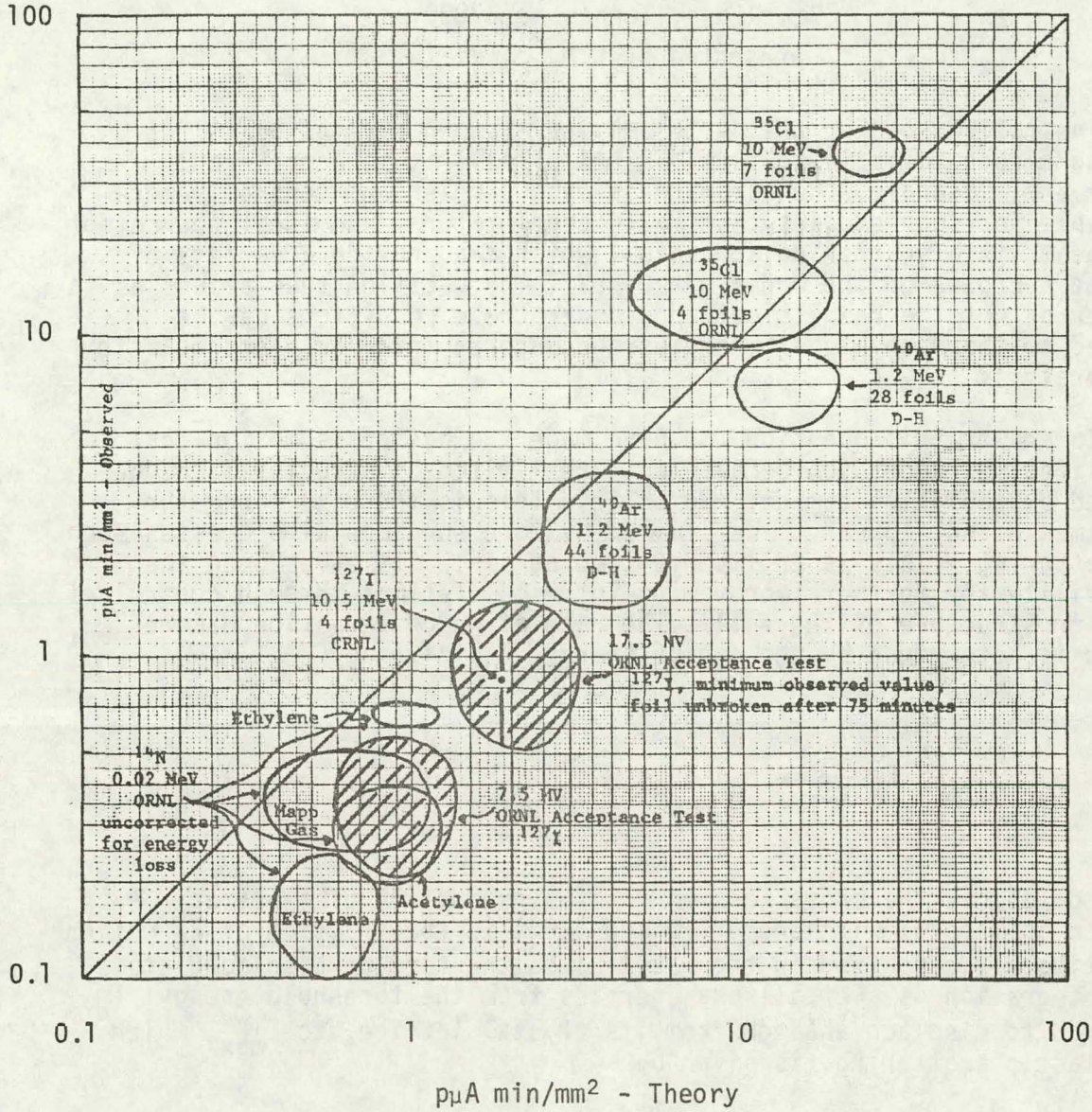

Fig. 1. Lifetimes for glow-discharge carbon foils. The theoretical lifetime plotted on the horizontal axis is defined as:

$$\tau = \frac{k\ E(eV)}{Z^2 M} ,$$

where k = 0.0073 × thickness ($\mu g/cm^2$) - 0.010 for glow-discharge foils with thickness in the range 2-10 $\mu g/cm^2$.

A PROCEDURE FOR THE RAPID EVALUATION OF CARBON STRIPPER FOILS

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presented by C.M. Jones

The motivation for trying to use very low energy ion beams for testing carbon foils is obvious. First of all, it is considerably faster and, secondly, the hardware is much simpler and generally more available for such studies than large accelerators. An ideal source for such beams is a sputtering apparatus, which can provide a variety of ions at energies on the order of 20 keV. The question then arises as to whether or not the results obtained under these conditions can be related to the much higher energies encountered in large electrostatic accelerators.

To answer this question, we have made the basic assumption that foil lifetimes under ion bombardment are limited by structural changes induced by radiation damage. We believe this is the case since the lifetimes do not appear to be limited by more fundamental processes such as sputtering. This is an important point since it leaves open the possibility for further improvement in foil lifetimes through control of the microstructure of the foil. The rate at which radiation damage occurs is determined by the damage energy, E_d, given by

$$E_d = \int_{T_d}^{E_{max}} E_R \frac{d\sigma}{dE_R} \nu(E_R)\, dE_R \qquad 1)$$

where $d\sigma/dE_R$ is the cross section for the production of a primary knockon atom (pka) with energy E_R and $\nu(E_R)$ is the fraction of E_R which is ultimately deposited in the material in the form of displaced atoms. The integration is over all pka energies from the threshold energy, T_d, required to displace an atom from its crystal lattice, to E_{max}, which, for elastic scattering, is given by

$$E_{max} = \frac{4\, M_1\, M_2\, E_o}{(M_1 + M_2)^2} \qquad 2)$$

where M_1, M_2 are the masses of the incident and target atoms, respectively, and E_o is the energy of the incident beam.

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*Operated by Union Carbide Corporation under contract W-7405-eng-26 with the U.S. Department of Energy.

For low energy heavy ions, the reaction cross section can be replaced by the Rutherford scattering cross section,

$$\frac{d\sigma}{dE_R} = 6.51 \times 10^{-18} \frac{(Z_1 Z_2)^2}{E_o} \frac{M_1}{M_2} \frac{1}{E_R^2} \ (m^2 eV^{-1}). \qquad 3)$$

The damage efficiency can be calculated using the accepted[1] expression which, for carbon, is

$$\nu(E_R) = \left[1+0.4336 \left(\frac{E_R}{5687}\right)^{1/6} + 0.0513 \left(\frac{E_R}{5687}\right)^{3/4} + \frac{E_R}{5687}\right]^{-1}. \qquad 4)$$

Using these expressions, we have calculated the damage energies for several different incident ions. We found that most of the damage is produced by pka energies less than 10 keV, and have used this fact to derive an easy to use expression to estimate the damage energy. In addition, the damage efficiency varies slowly over the energy range from 40 eV, which is the value of T_d assumed in this study, to 10 keV, and can be approximated rather well by a linear function of E_R. With these approximations, the damage energy for carbon becomes

$$E_d = 8.64 \times 10^{-17} \frac{Z_1^2 M_1}{E_o} \ (m^2 eV). \qquad 5)$$

Carbon foil lifetimes can, therefore, be estimated easily from

$$T_{foil} \propto \frac{1}{E_d} = k_{foil} \frac{E_o(ev)}{Z_1^2 M_1} \ (p\mu A \cdot min \cdot mm^{-2}) \qquad 6)$$

The proportionality factor, k_{foil}, will depend on the detailed microstructure of the foils and will, therefore, be different for foils made by different techniques. The empirically determined constants are found to be $k_{foil} = 0.0018$ for vapor deposited foils, and $k_{foil} = 0.0073\ t - 0.010$ for glow discharge foils of thickness $t(\mu g \cdot cm^{-2})$. The latter is for foils made using ethylene plus 10% argon and 2 -2.5 kV bias. Measured lifetimes are found to be in remarkably good agreement with estimates from eqn. 6 as shown in tables 1 and 2. Measurements at much higher energies have been reported for vapor deposited foils[2] and the agreement is much poorer, measured values being up to ten times the estimates from eqn. 6. Further studies at higher energies should, therefore, be made to determine the energy range over which these estimates can be applied.

These results suggest that lifetime measurements made with very low energy ion beams can be readily related to energies and ions used in large accelerators. We have, therefore, used this technique to test

glow discharge foils made with different hydrocarbon gases. The results, shown in table 3, have been disappointing thus far. Foils made using welding grade acetylene and with MAPP gas have given lifetimes which are essentially the same as those made with ethylene + argon. Thus, it appears at present that further improvements will have to find their origins in other areas, such as annealing or the use of refractory compounds having more isotropic properties than graphite.

References

1) D. G. Doran, J. R. Beeler, Jr., N. D. Dudey and M. J. Fluss, USAEC Report HEDL-TME-75-76 (1973).

2) G. Frick, V. Chaki, B. Heusch, Ch. Ricaud, P. Wagner, and E. Baron, Revue de Physique Appliquée 12, 1525 (1977).

I. LIFETIMES FOR VAPOR DEPOSITED CARBON FOILS

Incident Ion	Energy (MeV)	Lifetime (μgA·min·mm^{-2})	$\frac{k_{foil} E_0}{Z_1^2 M_1}$
^{35}Cl	20	3.4[a]	3.6
^{35}Cl	10	1.8[a]	1.8
^{127}I	10	0.066[b]	0.051
^{127}I	4.5	0.030[b]	0.024
^{40}Ar	1.2	0.33[c]	0.17
^{40}Ar	4.8	1.0[c]	0.67
^{14}N	0.020	0.021[a]	0.053[d]

II. LIFETIMES FOR GLOW-DISCHARGE CARBON FOILS

Incident Ion	Energy (MeV)	Lifetime (μgA·min·mm^{-2})	Thickness (μg·cm^{-2})	$\frac{k_{foil} E_0}{Z_1^2 M_1}$
^{35}Cl	10	33[a]	6	33
^{127}I	10	1.2[b]	≈10	1.8
^{40}Ar	1.2	3.0[c]	5-10	4.1
^{40}Ar	4.8	>28[c]	15	37
^{14}N	0.020	0.64[a]	≈6	0.98[d]

a) Oak Ridge National Laboratory
b) Chalk River Nuclear Laboratories
c) Daresbury Laboratory
d) Uncorrected for energy loss in foil

III. LIFETIMES OF CARBON FOILS IRRADIATED WITH 20 keV NITROGEN IONS

Vapor Deposited Foils ($\approx 5 \mu g/cm^2$)

$T = 0.021 \pm 0.010$ $\mu A \cdot min \cdot mm^{-2}$

Glow Discharge Foils

Ethylene $T = 0.64 \pm 0.07$ $\mu A \cdot min \cdot mm^{-2}$ ($5\text{-}7 \mu g/cm^2$)

Ethylene $T = 0.15 \pm 0.10$ $\mu A \cdot min \cdot mm^{-2}$ ($3\text{-}5 \mu g/cm^2$)

Acetylene $T = 0.29 \pm 0.11$ $\mu A \cdot min \cdot mm^{-2}$ ($4\text{-}7 \mu g/cm^2$)

Mapp Gas $T = 0.37 \pm 0.14$ $\mu A \cdot min \cdot mm^{-2}$ ($3\text{-}7 \mu g/cm^2$)

[Editor's note: The following discussion concerns the four preceding papers on the subject of carbon stripper foils.]

Den Hartog: I want to mention something that I left out of my talk. We have taken the opposite approach to that of ORNL as far as trying to test foils very rapidly. We accept that we cannot test them rapidly under real beam conditions. That is the purpose of the microcomputer and indexer. We also have a data acquisition system so that we can now keep track of the lifetime of every foil that we use.

Jones: That's a good thing to do.

Chapman: I had heard that cracked ethylene foils are more fragile than evaporated foils. Is that still the case and, if so, doesn't that make it more difficult to handle them?

Den Hartog: We use the same technique that Gallant used and I think we use the same thickness used at Oak Ridge. We then use a collodion isoamyl acetate mixture to coat the foils. That puts about 100 $\mu g/cm^2$ of plastic on the surface to give them strength. The coating is rapidly evaporated by the beam.

Jones: Our collodion is closer to 5 or 10 $\mu g/cm^2$.

Weitkamp: Does anybody ever see any problems with that collodion inside the vacuum system?

Den Hartog: We used formvar for years. We haven't seen any problems since we switched. We have ion pumping in the terminal and high vacuum tubes. It pumps down rapidly and within 5 or 10 seconds the pressure is back below 10^{-8} Torr.

Burn: We observe the same thing. When you put beam on a foil the pressure increases slowly and the collodion is gone within a few minutes.

Den Hartog: I'd like to invite anybody who is producing these foils to send them to us for testing in our machine. We intend to do this next week with some foils from several other labs. We hope to get some comparative statistics under similar conditions.

Jones: On the basis of observing two foils it is my personal opinion that we don't see evidence for the kind of gradual decrease in beam intensity that Neil described. For one of the foils the beam was constant until the foil ruptured, and then it went to zero. For the other foil that we ran for a long time the beam was constant. We probably have pretty good vacuum in that region, about 10^{-8} Torr. That behavior is entirely consistent with the observations that we made with a chlorine beam from the EN tandem. Foils made with a glow discharge will thicken slightly and then begin to decrease in thickness due to sputtering. Evaporated foils thicken substantially and usually break without ever decreasing. It is a very interesting question why your beam appears to gradually go down.

Burn: The rapid growth that you're talking about, Charles, is that for low energy beam tests?

Jones: I'm talking now about the very small number of measurements that we made with foils in the accelerator.

Burn: We have observed many foils where the beam intensity will slowly decrease and at some point it may rupture. When we were doing tests with ion beams on the cracked ethylene slackened foils we left one of them to see how long it would go. The beam dropped down to the 5 or 10% intensity level and stayed there for well over a day. It ran for about one hour and then gradually dropped off in intensity. Now maybe the foil had broke in the middle and we were using the fringes.

Jones: What is the vacuum where the foil is?

Burn: It's not a very good vacuum, probably on the order of 10^{-6} Torr.

Jones: That's a significant difference from the situation in our accelerator. You may be cracking carbon on the surface.

Saylor: I have a comment. We at Pittsburg are using cracked ethylene foils from an rf discharge which I described at last year's SNEAP meeting. The one thing we see is that they fracture very uniformly. Of the 1-cm foil area the foil will be gone over half the area, but the edges will be covered with foil. Do people know how much foil area is being bombarded by the beam? And another question, could this drop off be due to a hole forming and getting bigger?

Burn: On the tests that we did in the terminal, we removed the foils afterwards. We found one or two of them cracked near the edge of the beam shadow, but the vast majority were still intact.

Saylor: How big was the beam spot?

Burn: It's on the order of 8 mm in diameter. The foils are about 16 mm diameter.

Den Hartog: Our experience is similar to Neil's. The foils we take out are not broken. They've decreased to 70% of initial current but are still intact. Also, it's hard to say how big the beam spot is because the beam gets steered to different positions. But we see about an 8-mm shadow.

Saylor: We do see this roll off but we don't spend any time doing experiments on it. We also see the sudden fracture. I wonder if people can tell at what point they break.

Den Hartog: It's impossible to tell I think when a foil is broken inside the terminal.

Yntema: That's not quite correct because in fact when the foil cracks and breaks then the negative ion beam enters the high-energy tube.

Burn: We do a fair amount of work on heavy ions so people will be accelerating beams of Si, S, Cu, Ni, and others. Generally the experimenters will not tolerate very much of a drop. So when the beam intensity has dropped to, say, 80% of the initial intensity we will then go on to another foil. But the history of the foil is logged. If we later run another experiment with, say, C, O, or N beams, then we can go back and reuse those same foils again.

Saylor: Are they as good as a new foil for the lighter ion beams?

Burn: Yes.

Noé: I presume that this drop in analyzed beam is from multiple scattering due to thickening of the foil. Is that the generally accepted explanation?

Rowton: I think John is probably right about that. People tend to accept that explanation. I have a recommendation that I'd like to see at least one lab follow. As anyone who has had any dealing with foil preparation or target making knows there seems to be a lot of what we call black magic associated with it. At least one source of that I tend to believe is the residual gases around the target making systems. Has anyone put in an RGA or mass spectrometer there and actually looked at the gases during the glow discharge or evaporation? That kind of information can help reproduce the system from one lab to another, or even from one evaporation to another, and likewise in the terminal of the machine.

Burn: I'd like to make a comment on the procedure that Joe Gallant uses. Once when I needed a thermocouple gauge to use as a monitor in a system where I was blanking off the system and leaving it sit for several days, I borrowed one of the gauges that Joe was using. When I did the measurement the outgassing rate seemed phenomenally high. I then leak checked the gauge and found that the feedthroughs were leaking. The epoxy was porous. So I decided to use a new gauge and I found the same thing. I checked 5 gauges. Two that Joe was using and two that he had in stock had leak rates that were very similar. So, in fact, Joe may be cracking a mixture of ethylene and Chalk River air.

Rowton: I think this demonstrates what I was saying. There are large variations in conditions.

Den Hartog: Contamination is very important. I think at Chalk River there was measured a 3% hydrogen content, whereas at Heidelberg they measured 30% hydrogen content.

McKay: Neil, are you going to bottle Chalk River air and supply anyone who wants it?

Burn: I was going to bring that up. Perhaps we can send everyone a free sample with the SNEAP proceedings.

Conditioning Rebuilt Accelerating Tubes

Edgar D. Berners
University of Notre Dame

In May 1980 the accelerating tube vacuum system of our FN tandem was opened for installation of a new foil changer and realignment of the tubes. During the second week after opening, it was found one morning that tube section 2 had broken into two pieces sometime during the night. About two weeks later, tube section 1 also broke.

It seems clear from examination of the broken pieces that in both cases a glue joint between glass insulator and aluminum electrode failed. Over the lower three-fourths of the seal the glue pulled away from the electrode without leaving a trace behind. The broken tubes fell onto the string shields. (During rebuilding we installed string shields under the high energy tubes to act as catchers in case one of these should break sometime.)

The broken tubes were the original tubes first run at the factory in 1967. Total time at voltage was 60,000 hours. We do not know why they came apart. The accelerator was subjected to severe vibration over a period of several months during building contruction going on just a few feet from our lab, and that may have been a factor. However, we have learned from Lionel Fell that a tube built by Dowlish, with vinylseal, and stored for 12 years, came apart the same way when it was taken out of storage.

All four of our inclined field tubes were rebuilt at Potentials, Inc. in Round Rock, Texas. The rebuilding and vacuum testing took four weeks. The tubes were unloaded in South Bend on July 14, and the first pumpdown started on July 25. Conditioning began on August 3.

The tubes are pumped by Sargent-Welch turbo-molecular pumps with refrigerated baffles. The effective speed of each pump is 200 ℓ/s. After seven days of pumping on the new tubes the pressure at the low energy and high energy ends was 7 μTorr and 3 μTorr, respectively. After another 10 weeks the high energy end had stabilized at 0.2 μTorr and the low energy end was 0.7 μTorr and still decreasing very slowly.

Until this year our conditioning monitor consisted of three Geiger tubes mounted on the outside of the tank, opposite the terminal and the column mid-sections. For conditioning the rebuilt tubes we installed photomultiplier tubes to monitor light emitted by microdischarges inside the tubes. This scheme was first used at Rochester and was further developed at Brookhaven. We mounted windows on five of the ports on top of the tank and coupled a 6655A phototube to each window with a short light pipe. The signals from the tubes go to summing amplifiers so that all channels are displayed on one oscilloscope trace and one recorder channel.

The light pulses accompanying microdischarges have roughly a Gaussian shape and are 5 to 10 ms wide. They appear to contain a large number of very short duration pulses. At 1100 Volts on the 6655A the amplitude of the anode signal was about 10 mV for an average-sized pulse. There is almost always a burst of X-radiation or bremsstrahlung in coincidence with each light pulse, and the Geiger counting rate often saturates for the duration of the light pulse. If the dead time of the Geiger tube is 0.5 ms, there can be 20 counts in a 10-ms discharge.

We have also observed hard radiation not accompanied by any detectable light pulses (anode signal less than 1 mV). During conditioning we decided to assume that this hard radiation was produced by a discharge that involves only electrons with no secondary ions and that such a discharge would not damage the tubes. We therefore accepted higher levels (up to 5 mR/hr) of this radiation whenever that was necessary to keep the conditioning process going.

Conditioning started with the appearance of light pulses at 2.0 MV. Whenever the Geiger level was under two times background (~0.1 mR/hr) and the light pulse rate was only a few per minute, the voltage could be raised by 10 kV. If the Geiger level went over 2 mR/hr or if the light pulse rate went over one per second, then the voltage had to be reduced by 10 kV. These rules were rather elastic, but most of the time we obeyed them as given. Our intention was to produce microdischarges at the lowest rate that could be maintained more or less continuously.

Figure 1 shows the terminal voltage as a function of time during the first five days. The gaps are overnight shutdowns; the two sudden drops were forced by very high radiation levels or by sparking. Figure 2 shows all of the conditioning up to the time of this year's SNEAP meeting. Each vertical bar shows the increase in voltage during part or all of an 8-hour period. The time between conditioning runs was mostly given to experiments with terminal voltages less than the highest conditioning level. Note how far the conditioning threshold goes down between conditioning sessions, and how rapidly the machine conditions upward after a long period of experimental running, especially at the 45th day. Also shown is the highest operating voltage (9.655 MV) before the tubes broke.

Looking into the rebuilt tubes, we could see that the electrodes' apertures were not perfectly aligned, but had apparently random rotations about the tube axis of perhaps as much as ±1 degree. These variations appear to be unimportant, because the performance of the rebuilt tubes has been highly satisfactory. The high energy vacuum is as good as ever; the low energy vacuum is about four times the best ever but it is still improving. The transmission for protons from the charge-exchange source has been about 60%, and for heavy ions from the 2.5-MV injector it has been about 100%.

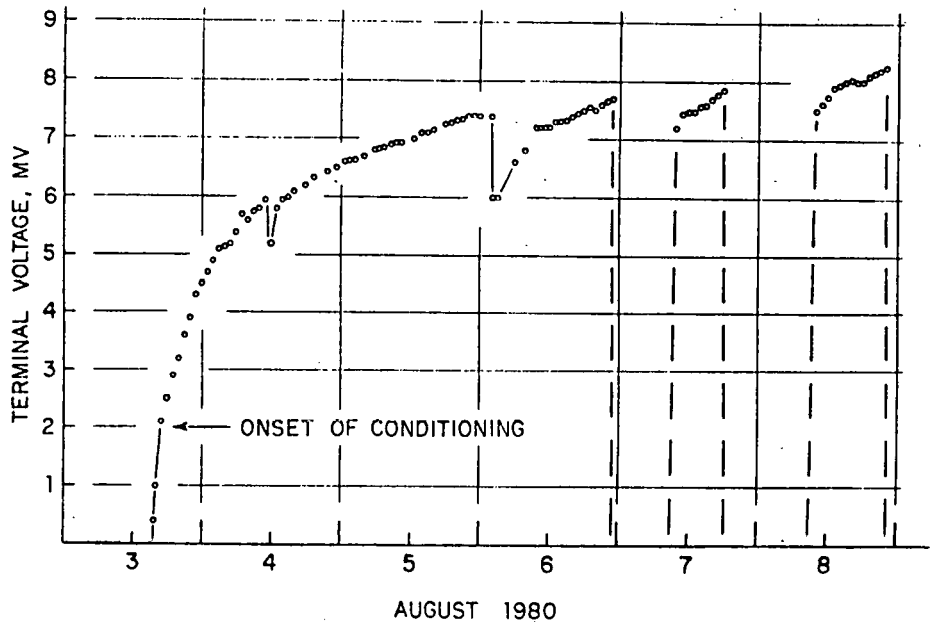

Fig. 1. Conditioning level during the first 6 days.

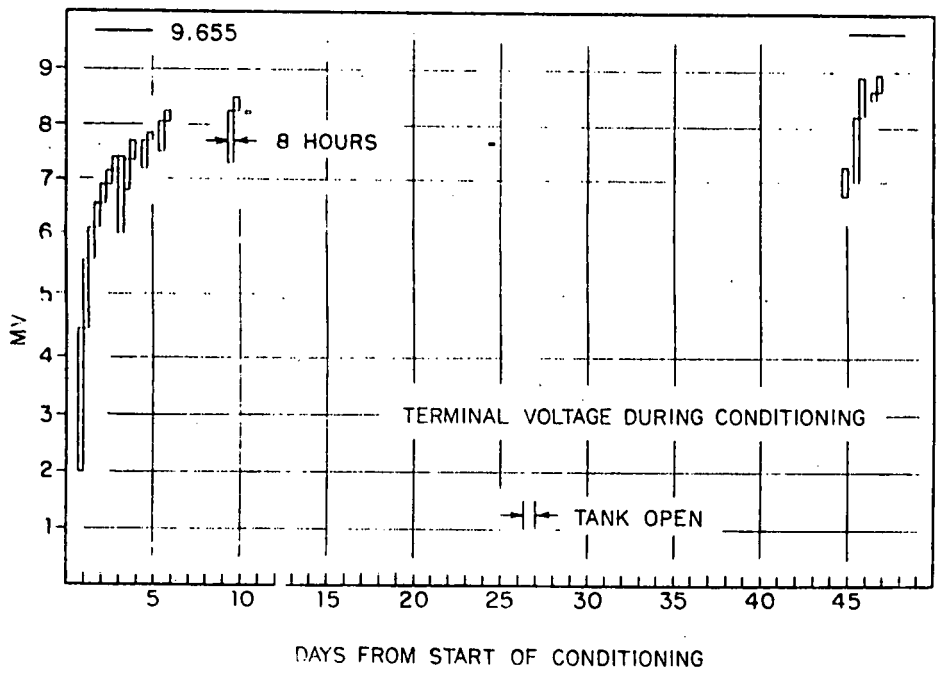

Fig. 2. Conditioning during the first 50 days.

Saylor: When you saw the radiation without the light, what did you do to get past that point?

Berners: We kept on raising the terminal voltage. This was after a rather long discussion on whether or not that was an appropriate response.

Saylor: When you first saw it did you go down in voltage while you discussed?

Berners: Oh, yes. The first time this happened we were at about 6 MV. We immediately went down to 5 MV which made everything very quiet. Then in a very relaxed way we tried to figure out what had been happening. We decided we could go on.

Jones: What is your hypothesis for normal conditioning?

Berners: For normal conditioning phenomena there is a discharge that involves the production of ions which travel to another electrode and there produce secondary ions which in turn go back and forth. The gas liberated by the sputtering operation of the electrodes emits the light that we see from the tubes. Now, this is not a hypothesis that I made up. I've heard this idea of what happens in a microconditioning discharge for a long time.

Weitkamp: I might mention that you can see this with the eye if you're dark adapted. I don't know what the X-ray level is so it may not be too healthy. But at least in the case that I saw the light seemed to occupy the whole tube.

Berners: Once, some time ago, I saw a glow like that over a distance of about 10 to 15 hoops next to the terminal on the high-energy side. I was looking in after about 20 minutes in the dark. The glow would come and go.

Den Hartog: I think you might mention what you were talking to me about the other day. While you were doing this there seemed to be no voltage excursions whatsoever.

Berners: That's right.

Den Hartog: And no vacuum fluctuations.

Berners: Well, the vacuum fluctuations were most visible when we had a complete discharge of the terminal. They did not always occur, but they often did. There sometimes were very intense bursts of conditioning, far more intense than we intended them to be. This caused us to turn down the terminal voltage by a few tens of kilovolts to make them go away. Those sometimes were accompanied by a vacuum excursion. Almost all of the conditioning events that I described and drew the picture of produced no discernable effect on terminal voltage.

Trainor: Did you say that's the result of monitoring the GVM with an oscilloscope?

Berners: No. That is the fast pickoff, the positive mushroom signal that is displayed on the oscilloscope. We don't put the generating voltmeter on the oscilloscope.

Trainor: I would recommend that everybody do that sort of thing because it has dc response and at high frequencies it works just as well as the mushrooms. We've observed lately that you can classify tube discharge events based on the structure of the GVM trace. For instance, in a tank spark the GVM will go to zero very rapidly, in nanoseconds. The terminal voltage will restore itself through a series of exponential curves that go up. If you have tube discharges over several sections of tube, then there's a much slower decrease in the terminal voltage which is exponential with time constants of some tens of milliseconds because of the limited electron current available to redistribute the charge on the column. After that event the charge again redistributes itself with the same series of exponentials. If you have a charge redistribution event like you're describing, then the terminal voltage must change by some amount.

SOME EXPERIENCE WITH FIBER OPTICS

by

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Until the relatively recent development of fiber optics technology, control of the equipment in the high voltage terminal of accelerators was accomplished by the use of insulating rods or strings. Although reliable and simple these systems are bulky and expensive, and limited in number simply because of their physical size. Small diameter optical fibers on the other hand can provide hundreds of control channels in the space normally occupied by one conventional control rod.

The first simple minded attempt to establish a fiber optic link in the Queen's 4 MV Van de Graaff was made in 1973. The link consisted of a standard Bausch and Lomb glass fiber optic bundle in a vinyl jacket strung, freely suspended, from the terminal to ground with intermediate connections to the column every 18 column planes where the bundle passed through the column shield plates. The fibers were led, by means of an epoxy pressure seal, through the tank base out into the laboratory. A small test lamp in the terminal, energized by the terminal alternator, was the light source. On preliminary tests the bundle was found to be working normally with all fibers active.

The machine was voltage conditioned to 4.1 MV in the usual way with the occurrence of several tank sparks in the process. Subsequently, on examination, the fiber optics were found to have failed, with no transmission of light from the test lamp. The fibers were removed from the machine 86 days later on a machine tank opening for other maintenance.

Examination of the fibers showed that the glass of the fibers had become highly discoloured at the terminal end in the manner characteristic of glass exposed to a high X-ray flux. This however could not explain the catastrophic failure of light transmission before any X-ray exposure had occurred. The fibers were found to be shattered at several places inside the vinyl sheath, probably at the points that had been in contact with the column shield plates. It was not clear however whether this damage was due to mechanical or electrical transient effects or both. The vinyl sheath showed some signs of electrical arcing damage.

In view of this experience a new link was designed. The fibers in this link were Valtec PC-10 polymer clad silica (PSC) fibers with high resistance to radiation damage. The fibers were supplied as single strands in kilometer lengths and are shipped with a rubberlike outer protective coating of "Tefzel". The link consisted of about 30 single strands, and four looped strands to be used for calibration and test purposes. The bundle of fibers was fed through a lucite tube which was then sealed at the ends and filled with Hysol HD 3561 epoxy casting resin for mechanical rigidity. Every effort was made to eliminate trapped gas bubbles from the epoxy by tilting and turning the tube, and eventually by evacuating it, but the gas bubbles

could not be entirely eliminated, and persisted in the cured resin. In spite of this it was decided to install the link in the Van de Graaff for tests in a spare carbonized location. Again, the fibers were led out of the pressure tank into the laboratory through an epoxy sealed pressure fitting. Tests made on the looped fibers showed these to be functioning normally to transmit light with the tank pressurized to 260 psig, the normal operating pressure.

The accelerator was run on a normal voltage conditioning sequence. At 2 MV on the terminal the voltage was observed to be erratic and this eventually fell to zero, and could not be brought up. Clearly, the terminal had shorted to ground. A check of the looped fibers in the optical link showed these to have failed, with no light transmission now apparent. The machine was opened for examination.

The optical link was found to have suffered massive electrical discharge damage with arc tracks clearly visible all along the fibers and in many cases penetrating radially through the walls of the tube in the regions where the link had passed through the column shield plates. The link had in fact become an electrical conductor through the carbonized tracks, thus effectively shorting the terminal to ground. Figure 1 is a photograph of some of the more interesting damage seen.

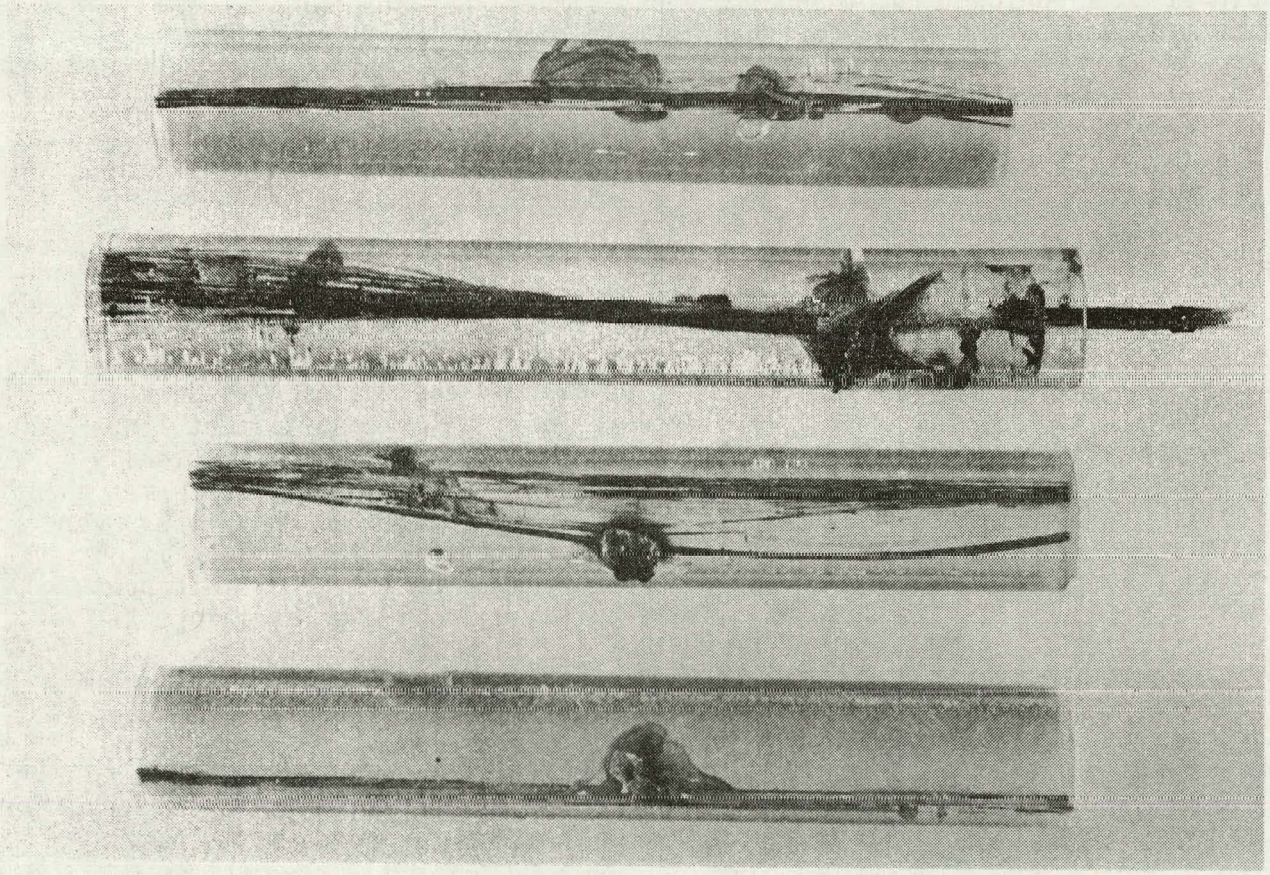

Fig. 1.

On close examination it was seen that the electrical discharge had occurred mainly in the outer "Tefzel" protective coating of the fibers with accompanying damage to the core of the fibers as a result of mechanical shock or localized micro-discharges. Short lengths of the fibers removed from a section of the link showed the glass to be still optically clear even where the coating was very black from the discharge. It was also apparent that at least to the 2 MV voltage to which the link had been exposed, the gas bubbles in the epoxy had not been the cause of the breakdown.

In future it is proposed to fabricate a link in which the protective coating has been removed from the fibers. Also, a better method of casting the epoxy resin to allow air or gas bubbles to escape might be to slot the tube longitudinally for filling. Any bubbles which accumulate on the epoxy surface can then be machined off later.

Walker: Henry, why did you put the fibers in lucite tubes?

Janzen: Just to get mechanical rigidity. In a previous test when we had not done this, but just strung a bundle of fibers up the column, they did not last. We suspected that the damage was a mechanical damage due to mechanical transients.

Rathmell: I wouldn't recommend the epoxy either and I probably wouldn't recommend the lucite tube, nor the "Tefzel" coating.

Janzen: No. I certainly don't recommend that. The reason we tried the lucite tube was that we had lucite rods in the machine which seem to stand up quite well. The epoxy, I would have supposed, would be as good as the lucite.

Lund: Our radial shorting rod that I described in the lab report is remarkably similar to these in the sense that we fooled around quite a bit before we found an insulating string which would survive in the machine environment. I think that the essential point is that no matter what you do with epoxy, you're never going to get a homogeneous rod and that's a fundamental difference from a lucite rod. When we were looking for monofilament line we wanted something homogeneous. Something that works quite satifactorily is the weed-eater cord from electric and gas motor-driven trimmers. It's a plastic 0.1 inches in diameter. But it had to be located so that it was very closely coupled to the electrostatic field. If it's out in the open it wouldn't work. It must be very close to the gradient column. I interpret this as being related to surface charges building up on the insulator.

Quin: That's true for anything out in the open. You have to use ground planes periodically along the column. We have many cords that run to the terminal, and every 50 or so sections we put a plane so that the electrostatic distribution of charge does not spark along the length of the cord. If you run things close to the column or put in planes periodically to segment the charge distribution, they'll last a lot longer.

Den Hartog: Let me add my voice to the clamor. That's the result we get, too. We use fiber optics in our machine and so long as they are graded and attached to the column we don't have any problems. I think we have them attached every other plane. We've had some cables in for about a year and a half.

Janzen: Some people seem to be able to run fiber optics up their columns without much trouble, but we have not succeeded yet.

McKay: In fine old SNEAP tradition I'm going to say that we've done something that I guess everyone else has done. We run fishing line. We went down to the local hardware store and bought some fishing line. Some of it runs in shields and some of it runs in the open.

Letournel: We have used fishing line in a CN for more than 20 years. In the MP we have a 3 mm-diameter line that is completely open.

Janzen: I might make one comment. I think our machine runs at gradients that are much higher than most machines run at.

Berners: I'm sure I remember a previous SNEAP meeting where someone reported that the lucite control rod in tubular form shattered.

Janzen: That was me. The machine will not tolerate tubes of any type running up the column.

DEFLECTOR SYSTEM FOR 2 MeV ELECTRON BEAM*

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INTRODUCTION

In our laboratory, we have an HVEC-type AK 2 MV electron accellerator that we have converted to make pulses of 1 nsec and longer. A team of scientists uses this pulsed beam for microdosimetry and biology studies on cell cultures. Some of the cell culture experiments require split-doses of electrons; i.e., a pulse of electrons, a delay time, and another pulse later. Because the doses are small, the dose due to "background" beam from the column during the inter-pulse period is significant and leads to errors in the overall experiment. To reduce this effect, a deflector system was installed so as to deflect the beam away from the target except during the desired pulse intervals. It succeeds in reducing the dose due to background effects by a factor of 100. Our deflection plates are about 12" long, separated by about an inch, so that 6 kV of deflection potential moves the 2 MeV electrons 1/2" or so off the target onto a beam stop. An experiment controller takes care of undeflecting the beam and pulsing the accelerator.

DISCUSSION

The switching time can be calculated knowing that we have

*This paper is based on work performed under United States Department of Energy Contract DE-AC06-76RLO-1830.

about 50 pF of capacitance to move through 6 kV with a maximum current of 3 amperes. By using

$$i = C(\Delta V/\Delta t)$$

we arrive at a switching time of 100 nsec. To withstand the voltage and current involved, a 3E29 vacuum tube was used (see Fig. 1), and switching was performed by connecting the 3E29 cathode to ground using three 2N6661 VMOS transistors (see Fig. 2). These VMOS units are much faster than bipolars and have been used by the author in test circuits to make 100-volt pulses in 50 ohms with about 10 nsec rise times. In the present case, they act as current sources limiting at about three amperes when switched on at the gate. The 3E29 grids are held at 100 volts with a current limiting resistor and the screens held at 240 V with another resistor. The cathode is pulled from 240 V to ground to turn on the tube and then released to cut it off. Three transistors are stacked to achieve the voltage capability, as they are rated at only 90 V each. The gate of the lowest transistor is switched from 0 V to +15 V by an active circuit at +15 to achieve maximum switching speed.

Several plate pull-up resistors are available to the user, the choice depending on the pulse length used and the recovery time desired. For short pulses and short recovery time, the 5170 ohm resistor may be selected, giving a recovery constant of about 260 nsec. The pulse length must be limited so as not to exceed the 10 mA current limit of the supply. For longer pulses, resistors of 52 k and 3 M are provided.

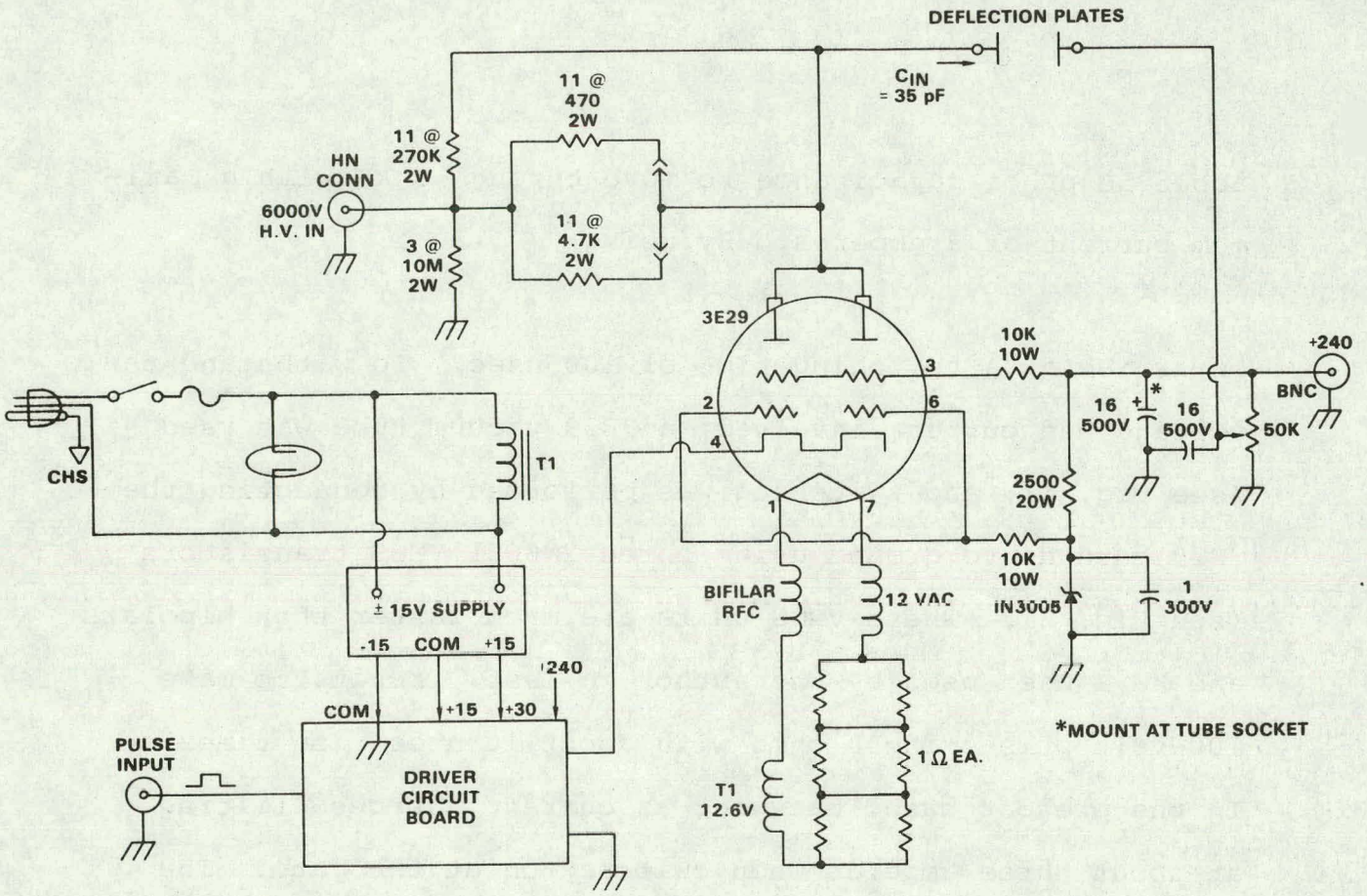

Fig. 1. High voltage circuits.

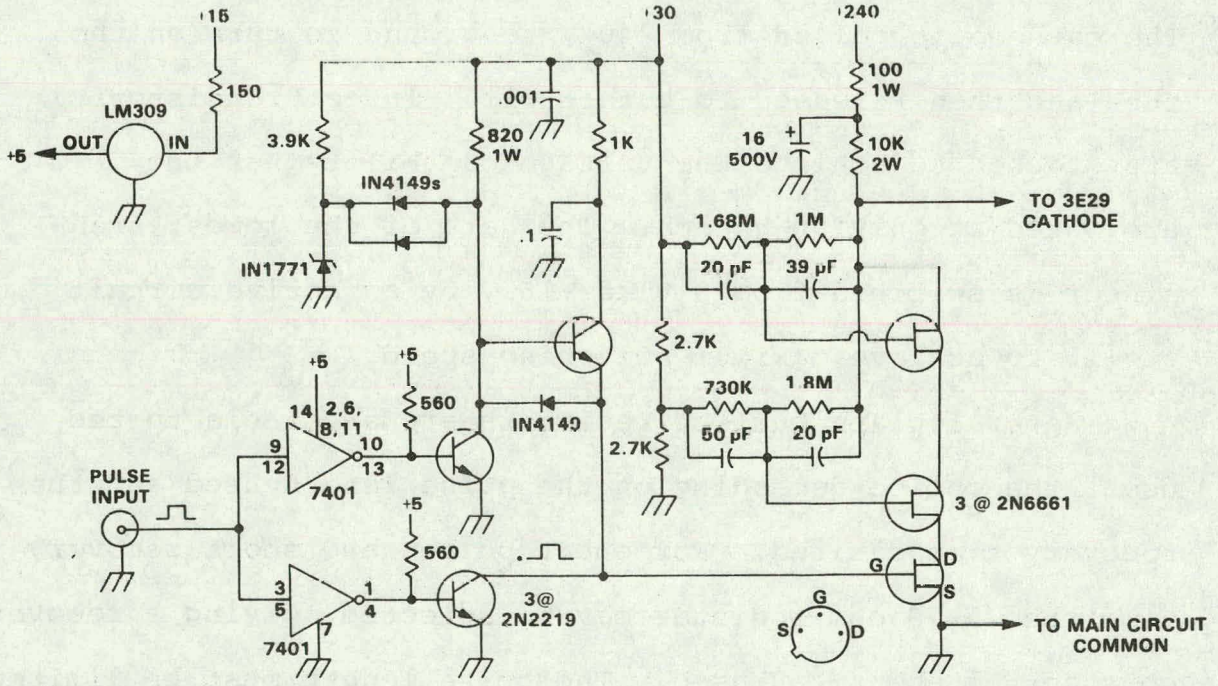

Fig. 2. Driver circuits.

STATUS OF THE OXFORD FOLDED TANDEM

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The Oxford Folded Tandem (Fig. 1) has been described in the SNEAP proceedings (1978), in a Laboratory Report (Oxford Nuclear Physics Laboratory 71/79) and most recently in a paper submitted to the CATANIA Conference (1980). This paper will outline our operating experience over the past two years, draw attention to some attractive features of the design and outline some of the improvements we plan to make over the next few years.

Operating Experience

Since the conversion was completed in 1978, the folded tandem has operated for a total of 6500 hours. During the test and commissioning period of six months from July 1978 to January 1979, operation was limited to short tests of 400 hours total duration. Helium beams were accelerated at energies up to 21 MeV (7 MV terminal voltage) and satisfactory transmission was achieved after field clamps were fitted to the 180° magnet to correct the fringing fields. During this period the infra-red data link was made fully operational and the Middleton and RF ion sources were completed.

In the first six months of experimental use, from January to July 1979, the accelerator ran for a total of 2200 hours, 1500 hours being experimental and 700 machine development. The maximum terminal voltage was 9.3 MV and 750 hours were spent above 8 MV. A number of modifications to improve performance and reliability were made and the 180° magnet box, which had developed leaks due to cracking of the stainless steel walls, was repaired temporarily. About half the running was with heavy ion beams from the Middleton source, the remainder being with helium and neon beams from the terminal source and helium from the charge exchange source.

In the period August 1979 to January 1980, there was a further small increase in operating time to 2300 hours, in spite of prolonged shutdowns to change the charge belt and to cure pressure sensitive leaks in the terminal. The maximum terminal voltage achieved was 9.8 MV and 1000 hours were spent above 8 MV. Experimental use increased slightly and machine development time was reduced.

In the four months February-May 1980, a total of 1750 hours of operation was achieved, of which 1600 was experimental and 850 above 8 MV. Making allowance for the Easter shutdown and for time lost in finding and repairing pressure sensitive leaks, this pattern of operation would correspond to 5000 hours of useful operation per year, which is considered to be reasonably satisfactory.

Since the start of experimental operation in January 1979, the tank has been opened 31 times, 14 times for scheduled maintenance and 17 times to correct faults. The average run duration is 180 hours and the longest run 520 hours. The frequency of scheduled maintenance resulted from the limited

capacity of the second foil stripper (used for He^{2+} and Ne^{2+} beams). A new stripper is being designed with more foils to overcome this limitation. Vacuum faults were the most frequent cause of unscheduled maintenance. As well as the cracks in the walls of the 180° magnet box several leaks were traced to large diameter flanges sealed with copper gaskets. Most of these leaks developed on depressurizing the tank. Lack of stiffness in the flanges resulted in overcompression of the gasket as a result of flange deflection caused by the external gas pressure. On reducing the external pressure, the flange would relax and, because of the lack of elasticity of the copper, a leak would develop.

The second common fault was sparking in the column, due either to displaced belt guide bars or to broken or disconnected resistors. The grading resistors are mounted nude between screening plates and must be assembled selectively to compensate for variations in length and concentricity. Some assemblies are subjected to mechanical stress when fixed in position and are liable to break in service. There is, so far, little evidence of electrical damage to the resistors.

Another recurring fault is loss of cooling water from the 180° magnet cooling circuit. The source of the loss has not yet been identified but it seems probable that CO_2 from the tank gas enters the water circuit, dissolves and, when pressure is reduced, causes the circuit to overflow.

During the various high voltage runs and conditioning periods the machine has sparked many times. Column breakdowns are more common than tank or intershield sparks; tube breakdowns, with accompanying vacuum rises, are very rare. None of these breakdowns has caused any damage to the data link or terminal electrical systems nor are there any signs of internal breakdown in the accelerator tubes. There are several superficial external tracks across tube insulators but these do not appear and indeed would not be expected to affect performance.

One component that is sensitive to surge damage is the corona probe, used to adjust the D.C. level of the intershield, and at present located in the central port of the lid of the vessel. This has on several occasions suffered insulator failure and the life of the needles is unacceptably short. It is planned to move this to a side point near the bottom of the intershield where it is hoped that it will not absorb a significant amount of surge energy.

Vacuum

An operating pressure can be achieved with the 450 ℓ/s turbo molecular pump alone, but in practice the four 500 ℓ/s ion pumps are always running during operation. A typical pump down time is 30 minutes to 10^{-4}mbar, 150 minutes to 10^{-5}mbar and 500 minutes to 2.5×10^{-6}mbar. The ultimate pressure, without ion pumps, is $\sim1.5\times10^{-7}$mbar in the terminal. With all ion pumps running, the ultimate pressure is between 3×10^{-8} and 3×10^{-7}mbar at the ion pumps, assuming the system to be leak free.

Voltage and Radiation

The highest voltage achieved so far has been 9.8 MV. Progressive operation has shortened the time needed to condition to high voltage and has

reduced the X-radiation levels observed chiefly around the upper part of the L.E. tube. These levels depend on vacuum and beam conditions and are typically less than 5mrad/h measured outside the tank near the L.E. tube and level with the terminal. Tube conditioning is difficult to monitor because of the absence of detectable vacuum activity in the accelerator tubes. The voltage appears to be limited by random breakdown in the column. In retrospect this has usually been traced to specific defects such as broken resistors, eroded connections or misplaced connector springs. Deconditioning is only significant after the tubes have been exposed to gas. Under good vacuum conditions, the machine returns to high voltage with very little reconditioning.

Beam Transmission

A number of beam transmission measurements were recently carried out, involving beams of various masses and energies. Although not complete yet, these measurements provide useful information about the problems and limitations of the present ion-optical arrangement of the Folded Tandem.

The tests were carried out at terminal voltages of 4, 6 and 8.5-9MV with beams of lithium, carbon, fluorine and silicon ions, but not every combination of the above masses and terminal voltages has been investigated. A complete set of measurements has been carried out for the carbon beam and is shown in Fig. 2(a,b). This graph indicates not only the overall transmission ($T_{3,7}$) of the accelerator from the entrance to the negative tube to the exit of the analyzer magnet but also the transmission at various stages through the machine, such as: transmission along the negative tube $T_{3,4}$; around the 180° magnet in the terminal $T_{4,5}$; down the high energy tube and around the 90° analyzer $T_{5,7}$ (this quantity is the geometrical average of the corresponding quantities for the different charge states). The transmission along the negative accelerating tube is essentially 100% for voltages above 5 MV approximately (Fig. 2a). Transmission around the 180° magnet appears to be limited by scattering in the foil stripper which becomes less severe as the terminal voltage is increased. The limitations imposed by beam scattering will be eased once provision has been made to move the gas stripper tube out of the beam path when foil stripping is being used. The rapid drop of transmission along the high energy tube ($T_{5,7}$) with terminal voltage, is not so easy to understand. One possible explanation could be provided by Fig. 2b which shows the variations of $T_{5,7}$ with different charge states for three different terminal voltages. The transmission decreases with decreasing charge state and this decrease is much more pronounced at lower terminal voltages. The behaviour could be due to poor vacuum in the region of the 90° analyzer magnet which destroys the slower moving ions (low q, low V_T). Further tests would be required to clarify this particular point. The overall transmission of the machine ($T_{3,7}$) is shown in fig. 3. In this figure, as in the previous one, we define transmission as the quantity

$$\sum \frac{I_q}{qI_o}$$

where I_q is the measured current at charge state q and I_o the injected negative ion current. For a given mass, the transmission of the machine increases

as the terminal voltage is increased; for a given terminal voltage, however, the transmission deteriorates with increasing mass.

Special features of the design

For the user, the most attractive feature of the folded tandem has been its ability to produce beams of fully stripped lithium, carbon and oxygen ions. The yields have been adequate for a variety of experiments, especially those involving large solid angle, high efficiency γ detectors.

The control system and the new voltage stabilizer have been outstandingly successful and no difficulties have been encountered with the operation of the 180° magnet in the terminal. A belt specially manufactured for us by HVEC will shortly be installed and this will, we hope, eliminate or greatly reduce stretching which has been a problem with the belts currently in use.

Improvements

The folded tandem is in regular use for experiments and so there is little time for our accelerator team to make improvements to the machine itself. Although improvements were made, notably in the control system and vacuum system, during the conversion, funds were limited and much remains to be done to what is, basically, a very old accelerator.

We are currently installing new oil free compressors and a large vacuum pump to improve the gas handling cycle. At the same time leakage rates are being reduced so that it will be possible to use gas mixtures containing a substantial amount of expensive SF_6 and this will improve the voltage capability of the accelerator. This is no sign of tube limitation at present, so we feel that 11 MV will be within our grasp.

We plan eventually to install an HVEC laddertron to replace the belt. This would eliminate the belt changing operation which is time consuming and difficult with our tightly packed high voltage terminal. Ripple would also be reduced and the machine interior would be largely free of dust.

We are also watching with interest the development by GENERAL IONEX of a new cryo pumping system which does not consume terminal power and we may replace one of the ion pumps (the gas stripper pump) in due course.

Although we are obtaining good yields of fully stripped light ions, double stripping still has a role in the production of heavier ions which are required for our atomic physics programme. The voltage stabilizer has been designed with double stripping in mind, but as yet no experimental work has been carried out. Double stripping calls for independent control of terminal and intershield potentials; intershield control will be achieved via the liner/generating voltmeter system and terminal stabilization via a variable stripper potential controlled from beam slits in the usual way.

We intend to retain the terminal ion source since the capability of neon and intense He^4 beam production is an important feature of our program.

We would like very much to install a buncher and nanosecond pulsing system, but it is unlikely that funds for this development will be available in the foreseeable future.

Conclusion

The past two years' operation has shown that the Oxford folded tandem is a reliable accelerator capable of further development and improvement.

Rowton: On your Conflat flanges, do you know if they were being pulled up metal to metal?

Allen: Yes, I think it is a complex question. I think they were being pulled up metal to metal and I suspect that the flanges were not quite as strong as they should have been. The leaks invariably came when we released pressure and we think the joints just came apart a little bit.

Rowton: Pulling them metal to metal is probably bad. If you leave a gap then you have spring energy stored in the flanges.

Allen: Yes. We have done some separate experiments in a pressure tank and found that the standard Conflat does give trouble after I forget how many cylcles -- quite a large number, but that the aluminum wire joints seem to stand a very large number of cycles.

K. Ziegler: What is the pressure difference and what voltage do you hold in your machine?

Allen: About 200 psi nitrogen and carbon dioxide. With that mixture and before we put tubes in while making the conversion we did get almost 12 MV. But since then we are running sulfer hexaflouride and with tubes it doesn't run above 9.5 MV.

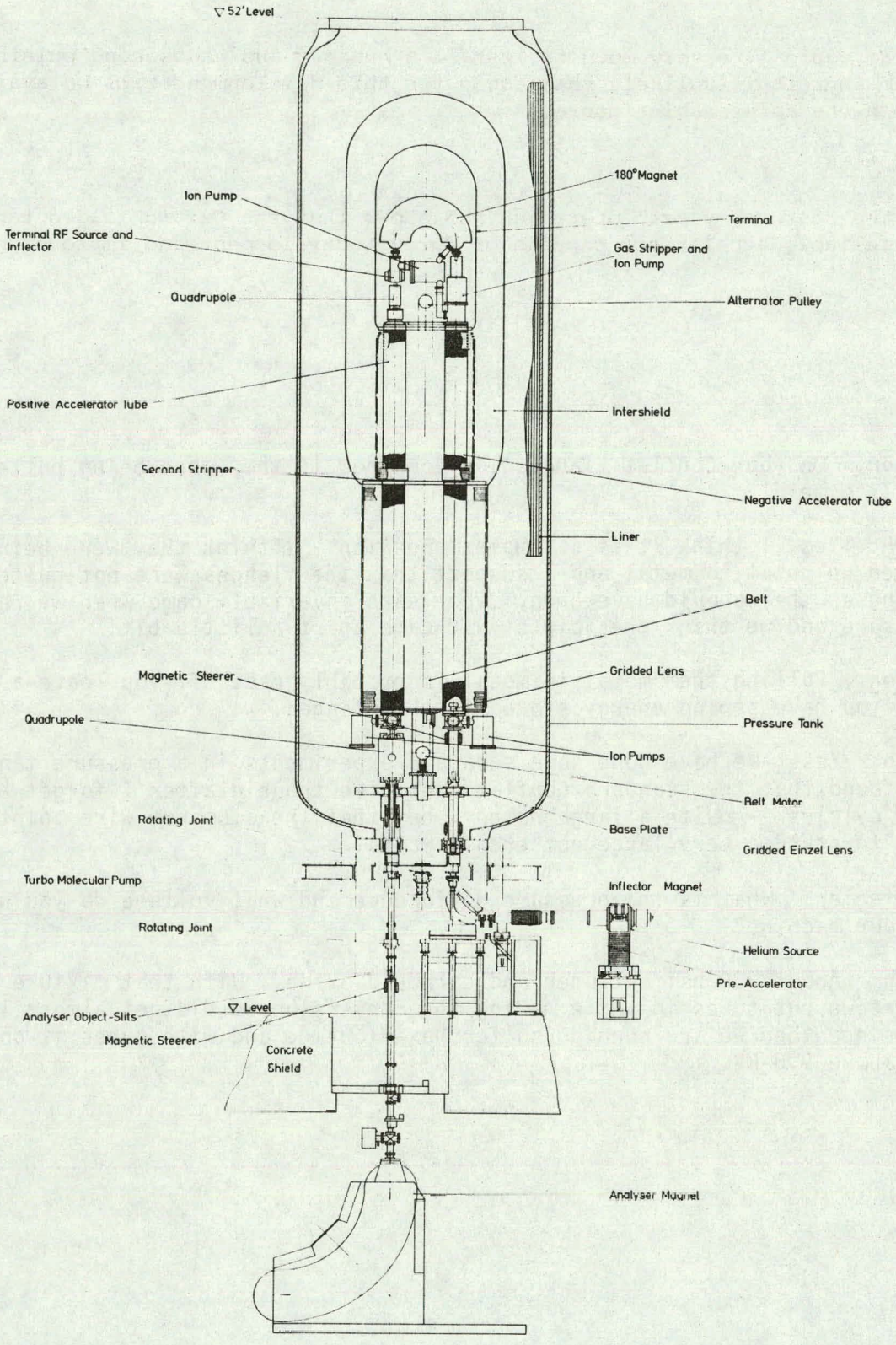

Fig. 1. The Oxford folded tandem.

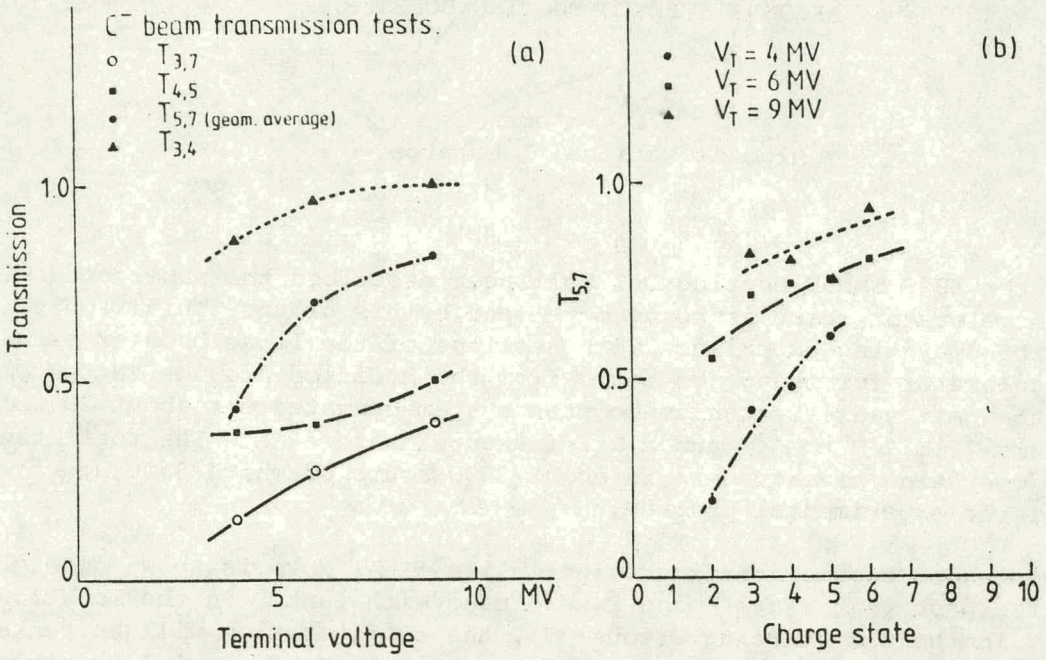

Fig. 2. (a) Carbon beam transmission at various points through the accelerator, as a function of terminal voltage. (b) Transmission of a carbon beam along the high energy tube as a function of charge state for three different terminal voltages.

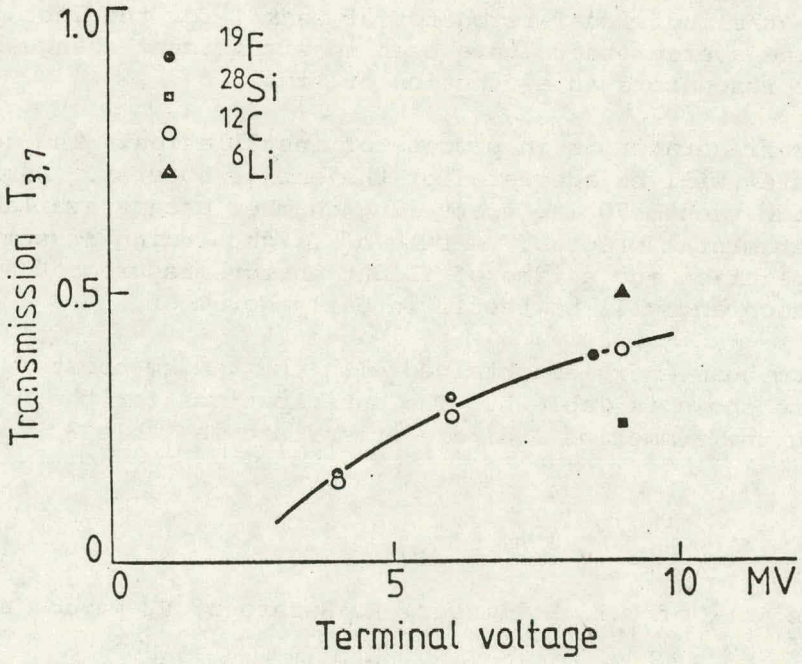

Fig. 3. Overall transmission of the accelerator as a function of terminal voltage.

OPERATING EXPERIENCE WITH THE ARGONNE SUPERCONDUCTING BOOSTER

J.L. Yntema*
Argonne National Laboratory

At the 1979 SNEAP Meeting Dr. Bollinger described the superconducting linear accelerator which is being developed by his group. This report covers the operating experience with sections of the linac booster as a post accelerator for heavy ion beams from the modified Argonne FN tandem. During the past year the tandem-booster system operated for about 20 weeks with beams from ^{16}O to ^{34}S and a brief excursion to ^{40}Ca. The total tandem-booster operating time to date is about 5000 hours of which 3500 hours were used for the experimental program.

The tandem booster configuration in its final form is shown in Fig. 1. During the past year tanks C and D were used with tank D in the location of tank B. In the run starting October 21, the configuration will be tanks A, D, C. In the Spring the final configuration A, B, C, D will be used. However, we expect tank B to have initially 4 resonators with the final 4 to be added in the fall of 1981.

The total tandem booster experience to date is summarized in Table 1. Since there will be a sufficient number of low β resonators ($\beta = 0.06$) in the configuration in the beginning of November, we will be able to extend the mass range of part accelerated particles to mass 64. The operating experience with the individual resonator of tank C for the last 18 months shows that on the average there have been no significant changes in the behavior of the resonators as a function of time.

A new He refrigerator is in process of installation. The new refrigeration system will be adequate for the entire booster. During the year the new high vacuum 70 in. scattering chamber became available for use in the experimental program. A time of flight tuning system was brought into operation and a time of flight energy measurement system is nearing completion and will be tested in early November.

The maximum beam energies obtained with the tandem-booster system through June are shown in Table 2. The anticipations for the November run and runs during the summer of 1981 are summarized in Table 3.

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* Reporting on work of L.M. Bollinger, R. Benaroya, R. Pardo, and K.W. Shepard

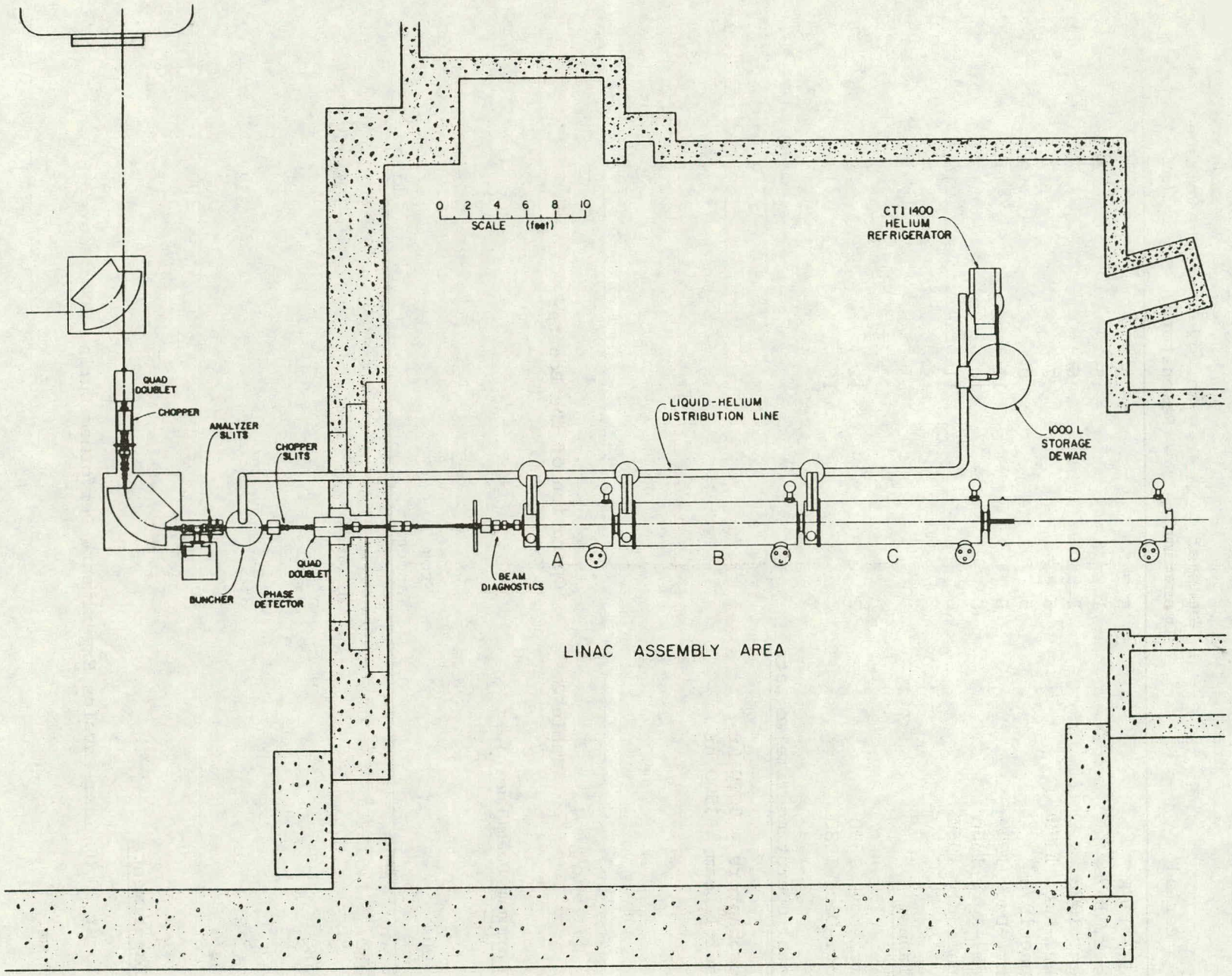
0 2 4 6 8 10
SCALE (feet)
CTI 1400
HELIUM
REFRIGERATOR
1000 L
STORAGE
DEWAR
LIQUID-HELIUM
DISTRIBUTION LINE
QUAD
DOUBLET
CHOPPER
ANALYZER
SLITS
CHOPPER
SLITS
QUAD
DOUBLET
BUNCHER
PHASE
DETECTOR
BEAM
DIAGNOSTICS
A
B
C
D
LINAC ASSEMBLY AREA

Fig. 1.

Table 1 Booster Operation

Experience

Period	Weeks of Operation	No. of Resonators	Max. Accel. Voltage (MV)
June 1978	0.7	2	1.6
Sept. 1978	1.2	5	4.1
Dec. 1978	2.5	6	5.4
Mar.-Apr. 1979	6	7	7.6
June 1979	5	8	9.3
Oct.-Dec. 1979	8	11	11.3
Feb.-Apr. 1980	6	10	10.5
June-July 1980	6	11	10.5
Planned			
Oct.-Nov. 1980	2	12	12
Nov.-Dec. 1980	4	16	15

Total Operating Time to Date

Resonantors ~ 5000 hr
Useful Beam ~ 3500 hr

Table 2 Operation of the Booster

Maximum Beam Energies

Ion	From Tandem	From Linac
^{16}O	70 MeV	155 MeV
^{28}Si	77	191
^{32}S	85	232
^{34}S	76	190

Experiments

About 20 Nuclear-Physics Experiments Performed During 3000 Hours of Beam Time.

Table 3 Booster Performance (Maximum Energies)

			10/80		11/80		6/81	
ION	q_1	q_2	MEV	MEV/A	MEV	MEV/A	MEV	MEV/A
^{16}O	6	8	151	9.4	166	10.4	190	11.9
^{24}Mg	8	8	171	7.1	189	7.9	215	9.0
^{24}Mg	7	11	195	8.1	230	9.6	270	11.2
^{32}S	9	9	185	5.8	214	6.7	245	7.6
^{34}S	9	14	242	7.6	274	8.6	322	10.1
^{37}Cl	9	9	174	4.7	223	6.0	246	6.7
^{37}Cl	9	14	235	6.4	291	7.9	332	9.0
^{40}Ca	10	10	194	4.9	235	5.9	273	6.8
^{40}Ca	9	14	219	5.5	282	7.0	334	8.3
^{58}Ni	10	10	94	1.6	197	3.4	257	4.4
^{58}Ni	10	19	94	1.6	356	6.1	436	7.5
^{64}Ni	10	10	94	1.5			243	3.8
^{64}Ni	10	19	94	1.5	341	5.3	435	6.8
^{64}Ni	9	19	85	1.3	322	5.0	424	6.6
^{74}Ge	9	20					430	5.8
^{80}Se	9	20					411	5.1

Assumptions				
Lo-β	{ no. of Resonat.	0	4	7
	{ Accel. Voltage	0	2.8	5.0
Hi-β	{ No. of Resonat.	12	12	13
	{ Accel. Voltage	12.3	12.3	14.0

Noé: What determines the schedule for finishing section B?

Yntema: Well, it's especially a question of money, of course, and it's a question of machine shop time. The critical portion of machine time is the electron beam welding of the resonators. We have two electron beam welders who can do a reasonably good job at it. The time for our resonators to go through the shop is approximately 59 days. So we are limited and we are not the only users of the electron beam equipment.

FLORIDA STATE UNIVERSITY SUPER-CONDUCTING BOOSTER*

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ABSTRACT

A super-conducting linear accelerator booster is to be added to the existing super FN tandem Van de Graaff accelerator installation at Florida State University to achieve an effective 12 MV additional acceleration voltage. A description of this booster, the tandem modifications to accommodate it and the present state of this project are described.

1. Introduction

In the fall of 1977, it was decided that the research needs of the laboratory required an increase in our energy capability to 10 MeV/nucleon, and that these needs could be met through the next decade by installation of a 17 MV tandem Van de Graaff accelerator.

The chief problem with such a proposal was the lump sum nature of the funding required and the high cost involved (over $11 M). Therefore several alternative options were considered, a large linear accelerator, a tandem plus cyclotron combination and a tandem plus linear accelerator combination. The first two of these have the same difficulty as the large tandem - a very large capital investment. The tandem plus linac combination can provide the required boost at a moderate cost and in addition to this large cost advantage, the construction time required to add a linac should be much less than that required for the other systems. Both a stand-alone tandem and the tandem plus linac combination have easily variable energy and precise energy control making them quite suitable for the proposed research. The pulsed beams from the tandem plus linac combination make it possible to construct an inexpensive heavy ion spectrometer that has most of the capabilities of more expensive systems. The time structure of the beam will not extend significantly the time required for the vast majority of experiments.

The options open to us from among the linear accelerator energy boosters already under development are well represented by the Heidelberg[4,5], Stony Brook[2,6,7], and Argonne systems[1,3]. We have decided, because of limited resources, not to develop a distinctly different system. A schematic comparison of the systems discussed are shown in Fig. 1.

The first choice, superconducting or ambient temperature, was not too

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* Supported in part by the National Science Foundation.

difficult. There are advantages of accessibility and modularity to a room temperature device such as the Heidelberg booster, but the capital cost per MV of the Heidelberg room temperature linac appears to be about twice that for the Argonne superconducting booster. Operating costs for electric power are about a factor of 5 greater for the Heidelberg booster and electric power costs are likely to be of increasing importance in the next decade. The additional cost for cooling water is also directly proportional to the primary power cost. The largest portion of the power consumption for a super conducting linac is for the refrigeration systems.

This decision would not have been so easily justified a few years ago when power costs were a minor factor and when superconducting technology was not as well developed. But now such systems have reached a satisfactory level of development, operated successfully and appear to be much more cost effective.

During the latter part of 1978 and the spring of 1979, an assessment of the relative merits of the superconducting alternatives was made and the Argonne system selected for our booster. The basic philosophy and total cost for comparable systems based on either technique are similar. Both use superconducting pill-box shaped resonators with 'split ring' drift tube mountings. The Cal Tech-Stony Brook design operates at 152 MHz with a lead superconductor plated onto a copper structure. The Argonne design operates at 97 MHz using niobium and niobium explosively bonded to copper as the superconductor. The Cal Tech-Stony Brook resonators are much cheaper to construct but the acceleration per resonator is much less. The refrigeration requirements for the Argonne design may be somewhat higher for the same energy boost. Operation at the higher frequency is sometimes a disadvantage from a nuclear physics standpoint but may be advantageous as regards phasing control.

The possibility of vacuum accidents must be considered and the stability and longevity of the niobium resonator appears to be considerably better than the lead plated alternative. Replating of the lead is a comparatively simple process but would probably need to be done more frequently than electropolishing of the niobium cavities to maintain performance. Conclusive data is not available but the consensus among cryogenics experts appears to be that niobium is the superior superconductor and behaves much better than lead at R.F. frequencies.

The development of the Argonne booster is further advanced than the Cal Tech-Stony Brook system. Sections of the Argonne booster, that almost exactly match our requirements, have been in operation. During a portion of an experimental run, which began in October 1979, the average energy boost per resonator was in excess of 1 MV. The system operated for more than five weeks accelerating beams of ^{16}O, ^{28}Si, ^{32}S and ^{34}S and during this period the system was phase locked over 90% of the time.

Based on these considerations the decision was made to construct our booster based on the Argonne system.

2. Tandem Linac Facility

The plan of the complete installation is shown in Fig. 2. The beam from the existing tandem will pass straight through the existing 90° analyzing magnet, through the end wall of the accelerator vault and into the

new linear accelerator vault where it will be bent by a new 90° analyzing magnet onto the axis of the linac. This method will cause a minimum disturbance of the accelerator operation and experimental program during construction and will retain the old beam lines and target room for experiments which do not call for use of the linac even after construction is complete.

The approximately 1-ns beam pulse from the pre-acceleration pulser will pass through a post-accelerator cryogenic buncher which will compress this beam into a 50-ps pulse width. It will then pass through the ten accelerating resonators to a switching magnet which will deflect the beam into the desired beam line, there will initially be three such experimental lines. Facilities will be available to strip the beam to a higher charge state half way down the tandem high energy column and before injection into the linac. A debuncher will be installed between the linac and the switching magnet to adjust the longitudinal phase space to match the experiment in progress.

3. Resonators

The basic resonator is shown in Fig. 3. It consists of a cylindrical box made of niobium explosively bonded to copper. The two end plates have niobium bonded to both sides to avoid thermal distortion and are connected to the body by a niobium ring compressed by bolted flanges. The split ring and drift tube assembly is made of niobium and welded into the body of the resonator. The whole assembly is electron beam welded and electropolished. Tuning is achieved in several stages. Firstly during manufacture, the body and the niobium end sealing rings are sized to bring the cavity to approximately the correct resonant frequency. This is further adjusted by deformation of one end plate whilst the resonator is warm by the position of a machine screw. Then a pneumatic device mounted on one end plate distorts this plate to achieve a slow tuning that maintains the resonant frequency within the range of the fast tuner which has a control window of approximately 150 Hz. The fast tuner is a VCX (Voltage Controlled Reactance) which operates using 4 pin diodes controlled by circuitry outside the cryostat.

4. Solenoid Lens

The transverse motion of the beam is controlled by superconducting solenoid lenses located at the entrance, exit and after each pair of split ring resonators. These are housed in the same cryostat as the resonators and share a common liquid helium supply with them.

5. Cryostats

The cryostats that house the cryogenic buncher and rebuncher are shown in Fig. 4 and each houses a single resonator.

The linac accelerating structure is composed of several resonators housed in a common horizontal cryostat as shown in Fig. 5. It is anticipated that the FSU booster will consist of two such cryostats either of which will be able to be pumped, cooled and conditioned off line, then being brought into service without loss of vacuum or cooling. In these cryostats the resonators are mounted on an optical bench formed by two large horizontal tubes which also act as supply and return lines for liquid helium.

6. Beam Bunching

The two stage beam-bunching system is designed to bunch 75% of the D.C. beam into pulses < 100 ps wide at a frequency of 97/2 MHz.

The system consists of a pre-acceleration pulser shown in Fig. 6. The voltage between the two grids is arranged to be very close to the ideal saw-tooth form by suitable adjustment of the phase and amplitude of the fundamental [48.5 Hz] and three harmonics in open ended and closed ended resonant tubes. The post-acceleration buncher is a resonator of the same type used in the accelerating structure and this compresses the ≃ 1ns pulse from the pre-acceleration pulser to < 100 ps for injection into the linac.

A post tandem chopper removes the small background of unbunched ions between pulses and a phase detector, consisting of a helical resonant structure, dynamically couples the phases of the pre and post acceleration bunchers.

7. Tandem Accelerator

The super FN tandem that will be used as an injector to the linac has required very little modification. The belt charging, resistance grading and stainless steel inclined field tubes have, after some initial teething troubles, performed very reliably for several years. We therefore decided to retain these and felt that we would be able to achieve the ≃ 1ns pulse width through the accelerator that was required. Pulsing tests have subsequently proved this to be correct. A fast stabilizer using a modulated stripper foil and an infra-red light link was installed and the stabilizer of the pre-accelerator was rebuilt to minimize beam energy and hence transit time instabilities.

The low energy beam line required modification to accommodate the pre-acceleration pulser so the opportunity was taken to rebuild this whole section using stainless steel beam pipe, Conflat seals and cryo-pumping.

The high energy beam line from the accelerator to the switching magnet has been rebuilt using stainless steel beam pipe and Conflat seals. This new system accommodates the high energy cryopumping station, the cryostat, the sweeping plates and the phase detector.

8. Status Report

The pre-tandem pulser and new low energy beam line were installed last year. This year the resonator cryostat was completed and the resonator installed and tested off line. In August this cryostat was installed between the 90° and beam switching magnets. Considerable changes were made to the high energy vacuum systems to protect the resonator. A vacuum protection system was built and installed with pneumatic valves on either side of the cryostat and sensing units in all experimental beam lines. The pump under the switching magnet was replaced by a turbomolecular one. The two pumps before and after the 90° magnet were replaced with a single turbo pump, pumping into both beam lines by means of a 6 inch pumping link.

The sweeping plates and phase sensor were also installed, the former

before and the latter after the 90° magnet.

The high energy pumping station was replaced with a cryopump also in August. This was of the same type installed in the low energy line last year. We have been very satisfied with these pumps. The low energy one has now been operating for over a year with no serious problems and the efficiency of the system is indicated by a recent foil change. The tube was let up to dry nitrogen and the foils changed. The system was evacuated to approximately 100μ with a mechanical pump and then opened to the cryopumps. By the time the operator had returned to the control room, both high and low energy vacuums were $<10^{-5}$ mm. The vacuum continued to improve rapidly to approximately 10^{-7} mm.

Testing of the resonator with beam is just starting. It has been cooled to liquid helium temperature in its new position and R.F. tests completed. Beam tests will be conducted during the next weeks.

The building to house the linac booster is well under way. The architect has completed the plans and the contract is about to be bid.

9. Conclusions

Work on the FSU booster is proceeding well and will be completed in three stages. One and two, which are funded, will see completion of the linac vault and target room. The completion of the bunching system into the old target room and the new linac vault and some initial sections of linac acceleration. Application for federal funds has been made for phase 3, to complete the project, and favorably received.

The ion energies that will be available are shown in Fig. 8 together with the Coulomb barrier for A on A, $(Z^2/2A^{1/3})$.

10. Acknowledgments

We wish to acknowledge the close cooperation and assistance of the Argonne National Laboratory who have supplied us freely with information and drawings and have agreed to construct the resonators for which they have developed the special techniques of niobium fabrication.

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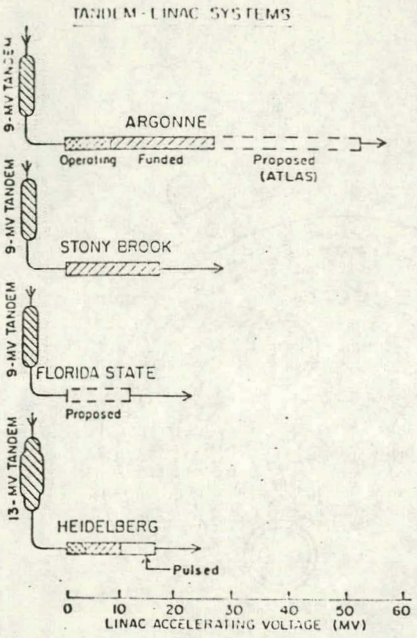

Fig. 1. Comparison of the tandem-linac systems.

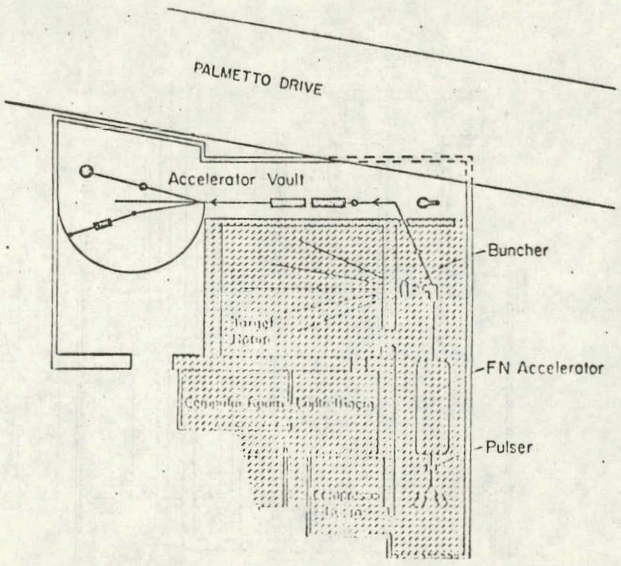

Fig. 2. Plan of facility showing new linac vault and target room. Shaded area is existing installation.

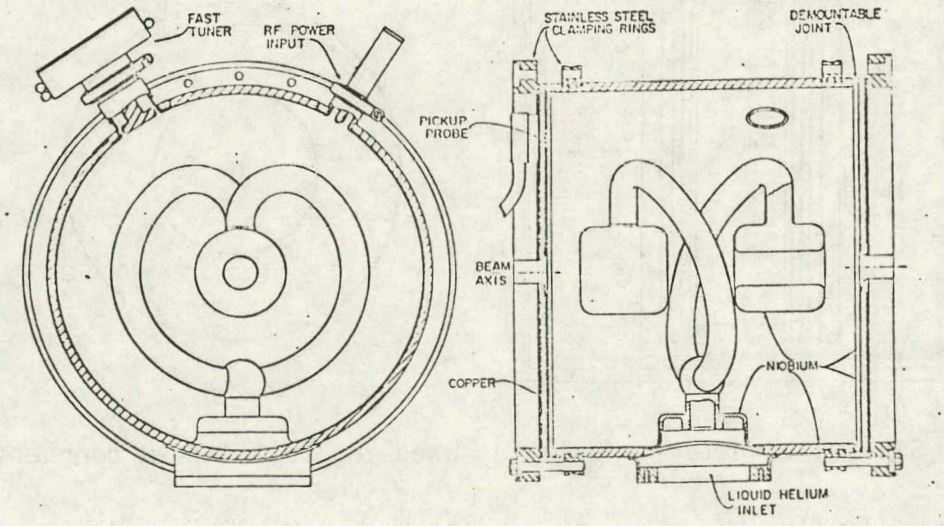

Fig. 3. Superconducting split ring resonator for $\beta = 0.105$ [from ANL Atlas proposal].

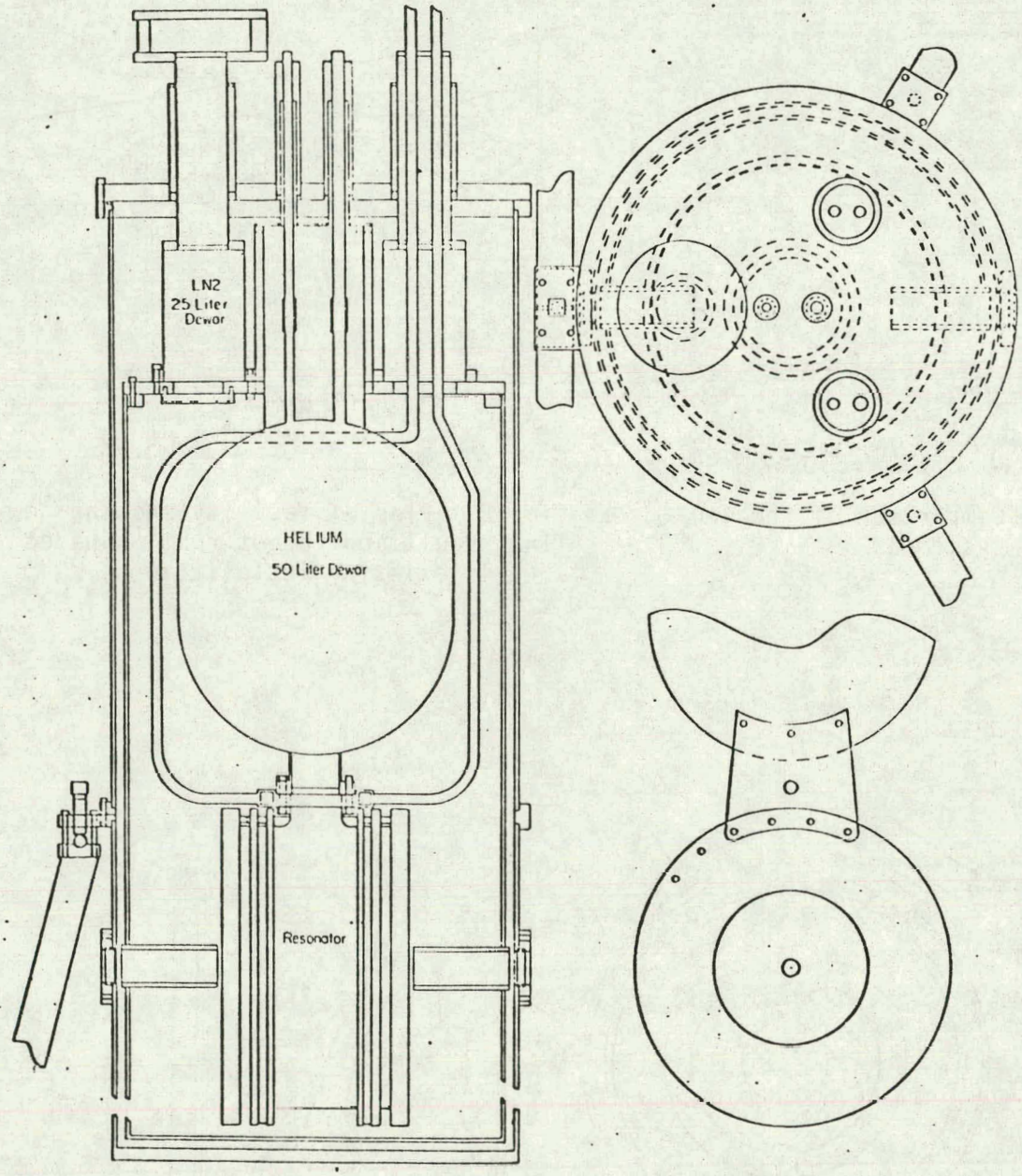

Fig. 4. Single resonator cryostat as used for post-tandem buncher.

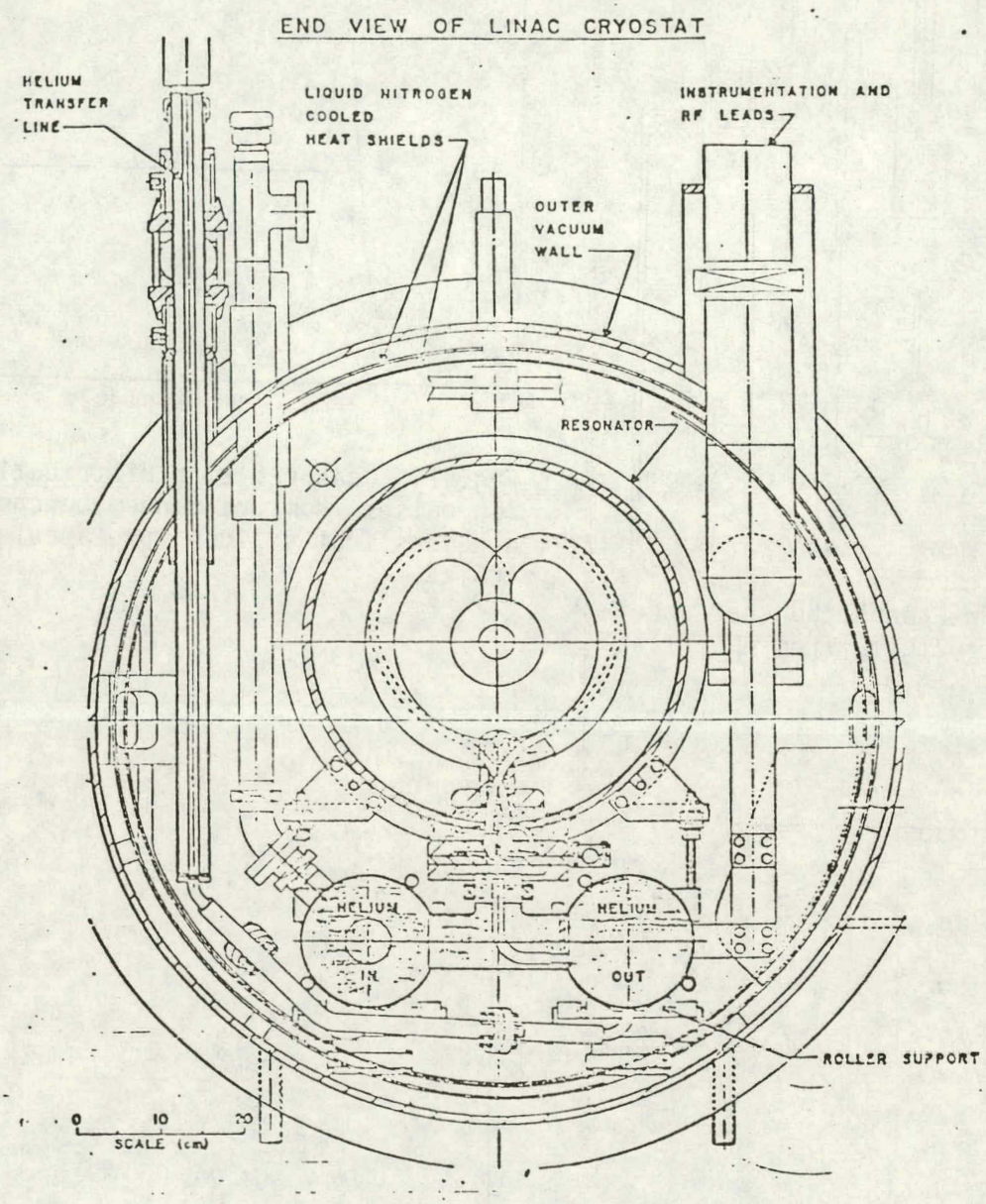

Fig. 5. End view of beam line cryostat [from ANL Atlas proposal].

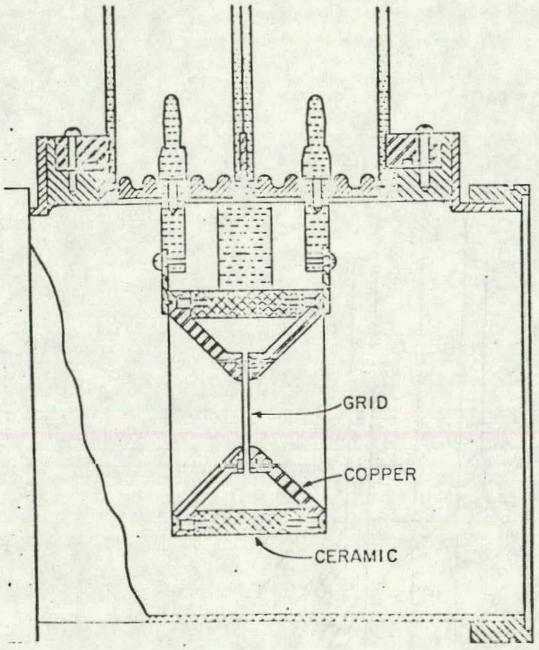

Fig. 6. Pre-tandem buncher accelerating structure [from ANL Atlas proposal].

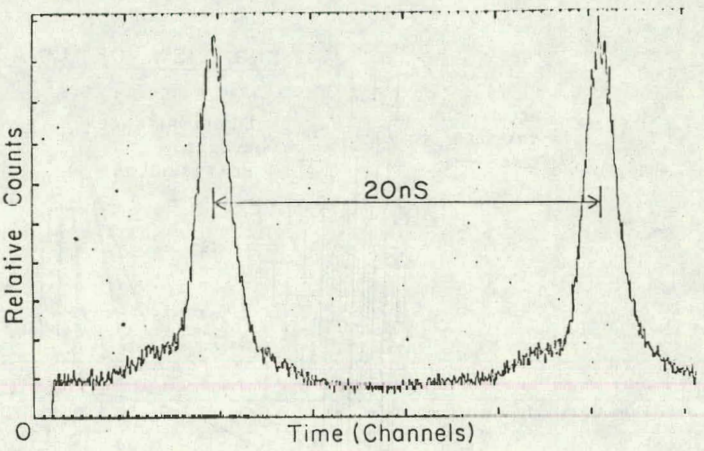

Fig. 7. Measured time distribution of ion pulses from pre-tandem buncher. Oxygen ions from silicon, pre-accelerator 100 kV.

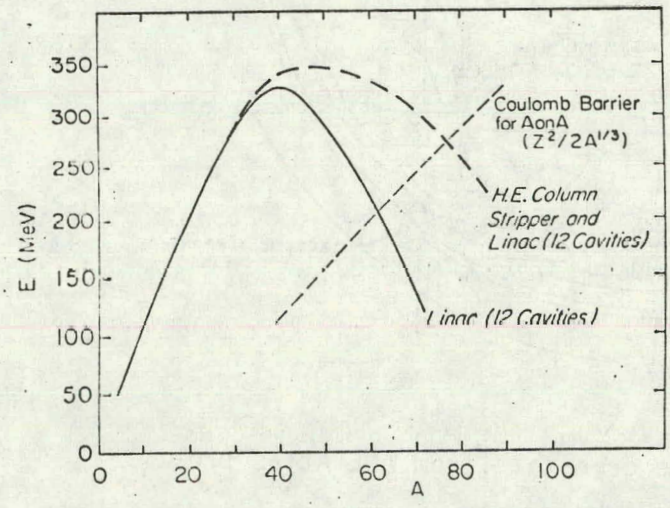

Fig. 8. Maximum beam energy vs. projectile mass for alternative stripper locations. The Coulomb barrier for A on A is also indicated.

Walker: Ken, I missed where your pointer was headed when you were pointing at the various turbo pumps.

Chapman: One is on the beam line immediately before the 90° magnet and the other is under the switching magnet.

Walker: What is their pumping speed?

Chapman: They are effectively 6-inch throat pumps. I'm not sure of the pumping speed.

Walker: Are they Leybold's latest model?

Chapman: As far as I know they are Leybold's latest. We have been well satisfied with the pumps and I must say we've been less satisfied with our frequency changer. They delivered us two, one which was inoperative when it arrived and another which died within half an hour of being switched on. They then sent us an emergency one which has worked beautifully. But it's very interesting to take the cover off and see that instead of the tiny transformer and tiny heat shields in the one they supplied initially this one has much larger transformer and heat shields. They've now taken that back and supplied us with a repaired version of one we shall keep. It apparently is working quite well, but it has had very little running time as yet.

Walker: We have just bought two, both of which had to be sent back because of bearing problems. We have two controllers. I talked to the people at McMaster some time ago to find out if we were able to get schematics for the controllers for our pumps. Apparently Leybold is subcontracting their controllers to various manufacturers and everything comes back to them epoxy encapsulated.

Chapman: Well, it's a little bit more difficult than that. In fact, they were not able to supply us with a schematic of the frequency changer because no two of the frequency changers were the same. The two they sent us had entirely different circuitry in them.

Walker: I know of four different manufacturers.

Chapman: So this appears to be going through a stage of development at the moment.

McKay: Our experience with Leybold pumps is similar to Ken's except that we've managed to destroy a couple of pumps as well. The rumors we hear from the sales representatives are that Leybold in the States is fed up with the German power supplies and they are likely to start building their own. Apparently the manufacturers of the power supplies see no real reason why they should supply schematics to Leybold and, therefore, Leybold can't tell us what's in them.

Noé: Without going into details, we have had the same kinds of problems. I have a question about bunching. What beam was that you were using?

Chapman: Oxygen.

Noé: Can you say a word about the injection energy and stability?

Chapman: The injection energy was approximately 100 keV. The stability initially was certainly poor. I'm not sure what the magnitude of that was. Supposedly with the new stabilizer the stability is of the order of 50-60 volts.

Noé: Do you have a phase feedback?

Chapman: Yes.

Den Hartog: What do you anticipate to be the effect on bunching of using inclined-field tubes?

Chapman: The worry is transit time. I think the indications from the tests we've done so far is that this is not going to be troublesome. Until we have succeeded in phase locking the buncher in with the pulser it is difficult to be definite about this. The stainless steel tubes that were put into the machine when it was installed in 1970 are effectively as good as the day they went in. So if they perform in this function we will be very happy with them. There's some radiation blackening certainly and there's some polish lines on the glass, but these probably occurred in the first few hundred hours while the tubes were conditioned.

Lindgren: Hearing this talk of turbo pumps, I have a question. Does anybody know a good vertical turbo? I've heard disparaging remarks about Welch vertical turbos and now Leybold. What is a good one?

Den Hartog: Well, we don't have any trouble with the power supplies with Welch units.

Lindgren: How about the bearings?

Den Hartog: Oh, have lots of trouble with the bearings. But I've heard that if you run them at half speed they will last forever... or at least twice as long anyway.

Billquist: That's not as funny as it sounds. The 1500 l/s Welch has a switch for high speed (1500 l/s) and low speed (1000 l/s). So if what Pat says is true and you need a turbo pump, that would be all right.

Lindgren: We plan to use them on the negative-ion injector where we have to pump helium. We also want to use a cryopump for normal use.

Upgrade of MP-6 and MP-7

by

Robert Lindgren
Brookhaven National Laboratory

In 1979 the installation of CTI model 8 cryopumps and GE 500-ℓ/s ion pumps was completed on MP-7, at both ends of the machine, to replace the mercury diffusion pumps. This resulted in the substantial savings in power and liquid N_2 costs of about $13,000/year. To date we find the use of these pumps has given us no more trouble than the diffusion pumps. The ion gauge readings are comparable, about 1.0×10^{-7} Torr. In April of this year MP-6 was shut down for two and one half months, during which time we kept MP-7 on line, and installed the:

1) 3-chain Pelletron
2) Power drive shaft
3) 14-inch HVEC acceleration tubes
4) Foil changer in the 5-6 dead section
5) Ion pump in the 6-7 dead section
6) Caddock resistors with capacitive plates and inductances, 600 MΩ
7) Cryopumps on both ends of machine as on MP-7

NEC supplied the power shaft which drives two 3-kW, 3-phase alternators in the terminal, one insulated for 100 kV, the other at terminal potential (to power the negative ion source), and one alternator in the 6-7 dead section for the pump power supply. The 7.5-hp motor is direct coupled, as are the alternators. So far, this drive shaft has run flawlessly.

The Pelletron charging system, running in the positive terminal mode, also performs extremely well. Ripple is less than 100 volts. But due to our limited supply of insulating gas mix, 140 psig in MP-7, 0 psig in storage, that leaves us 65 psig for MP-6 and we feel a limitation of about 8 to 9 MV+ for the present.

We find a limitation of about -7.5 MV in both MP-6 and MP-7 even though MP-7 has 140 psig in it. Above this terminal voltage it becomes unstable and occasionally, spontaneously drops several MV. A higher than normal inductor voltage is required, and the measured charge density on the down run of the chains is very low, as observed on the inductive pickoffs. We have tried a down charge supply in the terminal of MP-6, but this did nothing to alleviate the problem. This requires further study.

The one problem which has been most persistent in limiting the performance of MP-7 has been the lack of reliability of the resistive voltage dividers used along the tube and column. Resistors changing values, opening or being physically damaged have caused recurring difficulties. The simplest and least expensive method of all the systems we've tried is now being implemented for both machines. This is a pair of caddock resistors with a spark gap across them, mounted between capacitive plates.

These parallel plates are 1/16-inch aluminum, and are mounted on 4-inch long plexiglass insulators around which the inductances are wound. The protection of the resistor is mainly based on an LC network to attenuate the high frequency components of the voltage transients so as to allow sufficient time for the protective spark gap to fire before excessively high gradients are reached. This system has been tested out in MP-7 for over a year at terminal voltages up to 14.1 MV in tubes 2 and 5 and column section #4. We are scheduling two weeks early in November to complete the installation of this resistor type in MP-7. Incidentally, the cost of one resistor assembly is about $30. Our system was adapted from the Rochester and Daresbury design, with the inductances added, in the hope that with ever increasing terminal voltages, the resistors would continue to survive.

MP-6 has already had the resistor assemblies mounted in the column. The tube electrodes are electrically connected to the column, with the normal springs which are attached to the acceleration tube electrode with 2-56 screws, the column end of the spring wire is clamped under the head of a screw, to the column. Two 300-MΩ resistors are connected in series.

We have work in progress on shaped stainless steel terminal shields for both accelerators, to replace the horizontal bars, for improved electrostatic surfaces similar to the Rochester terminal.

We had expected to have improved voltage regulated extraction power supplies for both negative ion injectors, but unfortunately the General Ionex 1304 units did not survive very long, so they are being sent back for repair.

Future Plans

Acceleration tube extensions: After the success of Strasbourg, we are currently studying with HVEC the feasibility and costs of adding electrodes and insulators to our present tubes. We would wish to make them as long as possible, and still have space in the dead sections to have additional ion pumps, as well as the bellows. We are committed to the ion pumps as we have them in house now. They are Varian 110-ℓ/s pumps, with the NEC drive shafts for the alternators. We also plan to drive the terminal alternators with these drive shafts. The length of the modified tubes will have to be compromised with installation of the ion pumps.

Noé: Do you see any problem with the Pelletron when you extend the tube?

Lindgren: No, the Pelletron is mounted above the tube. We don't think there should be any problem. There's enough spacing from the tubes to anything that's near them.

Noé: My question was based on the remark that someone made that the field lines would have to bend somewhat more sharply.

Lindgren: It's a good subject to think about, but I hope we don't have any difficulties.

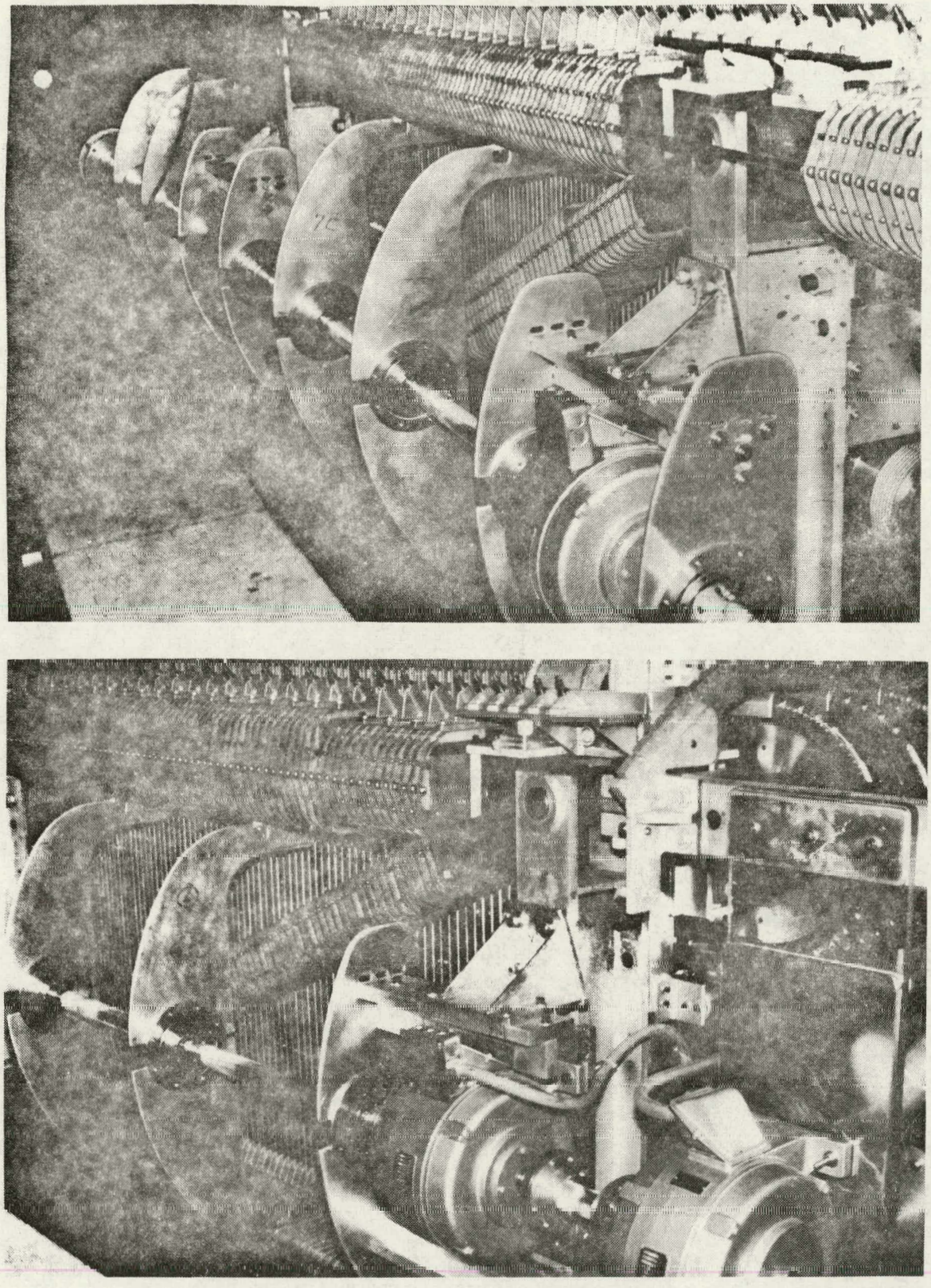

Fig. 1. NEC drive shafts and alternators in dead section (top) and in the terminal of MP-6 (bottom).

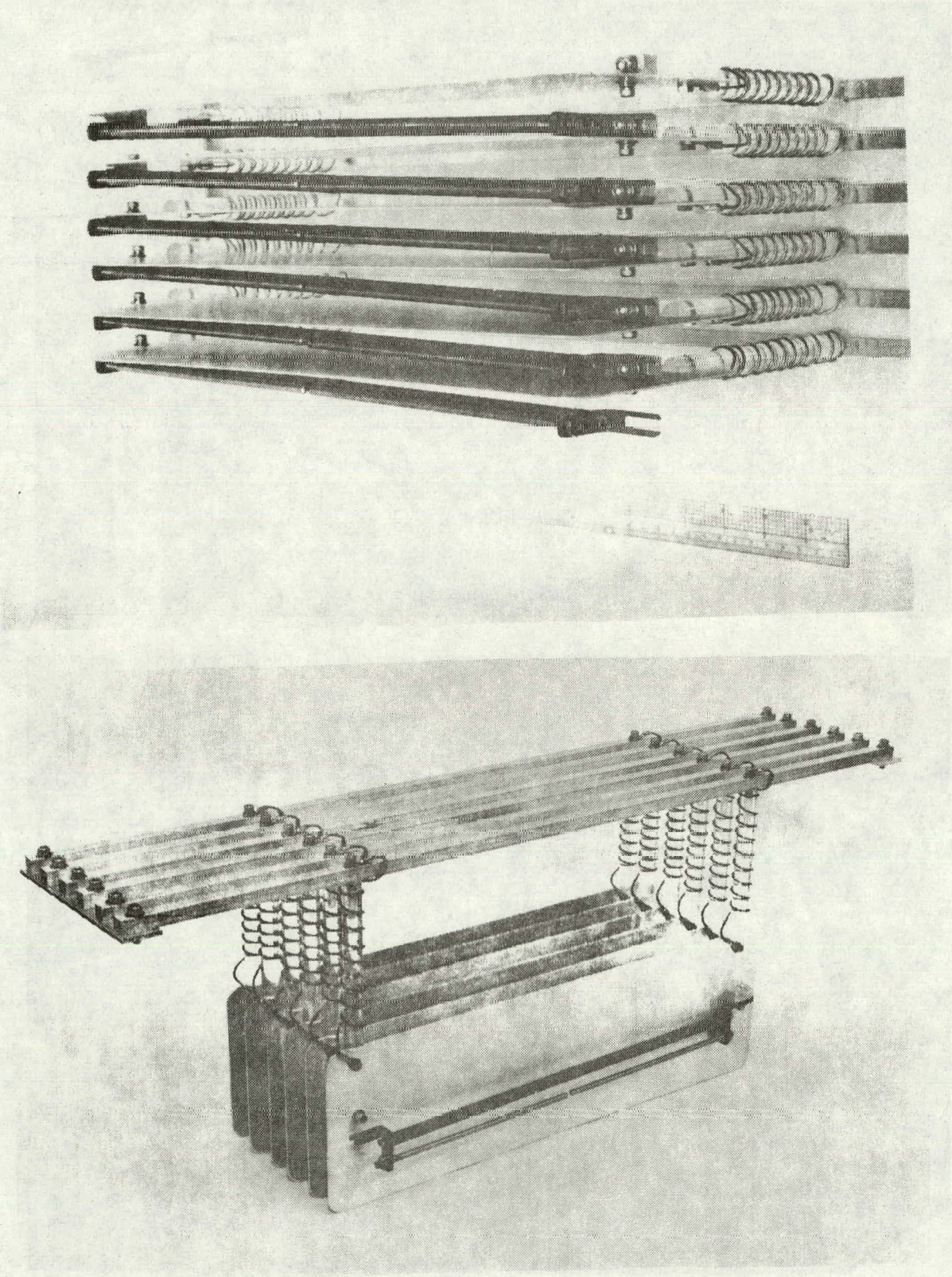

Fig. 2. Two views of the BNL column resistor mounting arrangement.

Upgrading of Strasbourg MP-10

by

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Centre de Recherches Nucleaires de Strasbourg-Cronenbourg

ABSTRACT

We have replaced the standard 72-inch tube sections with new 88-inch tube sections taking advantage both of the protrusion of the electrostatic field toward the dead sections and the space left there. A new tube has been tested separately up to 4.6 MV and also in normal MP running conditions for three months. After some difficulties with broken tube glass, the tests of the entire ungraded machine have been carried out up to 15.75 MV in August of 1980.

Twice without accelerating tubes we made column tests up to 16 MV and 17 MV with belt charging and at 100 psi SF_6 pressure. With a tube the MP machines are limited to about 13 MV. It appears that a tube voltage gradient of 18-20 kV/cm is a practical limit of exiting accelerating tubes. For an inclined field tube the voltage is almost proportional to the length.

Looking back at the tests without accelerating tubes in an MP, the normal configuration of the electric field inside the column shows a natural penetration of this field into the dead sections and into the terminal and a protrusion outside the LE and HE columns. Nowhere inside the column is there a high electrical stress, the field being in the range of 20 kV/cm or one-tenth the stresses found in some places outside of the column. So keeping the same 13-MV voltage tube gradient it is natural, from an electrical point of view, to lengthen the tube sections. For such an arrangement the tubes and the column have separate resistor chains in parallel and they are connected only at the dead sections. Our project consists of lengthening our 72-gap tubes (with 70 live gaps) by 16 gaps in order to get 88-section tubes. All of the individual tube sections in the MP are identical except for the number 1 tube.

One characteristic of this tube, different from the original, is its symmetry with respect to a central axis. Either end can be used for beam input or output. This is important if we wish to keep the previous voltage conditioning, and so we can move a tube from the LE side to the HE side. The beam optics calculated by HVEC shows only moderate modifications and predicts the same transmission. The dual post-foil stripper has been kept in the dead section between tubes 5 and 6. The tube flange thickness had been reduced from 75 mm to 36 mm at first but now it is 45 mm. The last inclined field (IF) electrode on both sides of the tube section protrudes 8 inches inside the dead sections and outside the LE column on both ends of the LE tube. The first terminal grounded electrode of the number 5 tube protrudes 10 inches inside the terminal because of a special arrangement of our post-foil stripper which includes a special bellows in the dead section between tubes 5 and 6 where the last IF electrode protrudes only 6 inches. Smaller bellows are used in each dead section. The terminal stripper has been rebuilt with the length reduced by about 15 inches.

For the electrostatic field penetration the horizontal plane is the worst case and Fig. 2 shows a rough sketch of the equipotential lines. Smooth plates are placed in these locations, and for a better match to the field pentration lines, the gradient rods have a special round shape. They are pushed outside toward the column and dead section compared to the original straight design. The 88 tube resistors are 1200 MΩ as are the column resistors. The spark gaps are 3 mm for the tube resistors and 3.4 mm for the column resistors. They are placed above the tube. The tube supports have not been modified, but have been moved inside the dead sections.

In May, 1979 we installed a new 88-inch stainless steel tube in the number 7 position. The tube had flanges 36 mm thick and metallic gaskets like the other tubes. The tube was tested alone up to 4.6 MV and then left in position for normal running until August 1, 1979. All the 72-inch tubes were then removed and sent to Burlington to be lengthened.

When we received the seven tubes lengthened to 88 inches we had difficulties with many broken glass insulators. It took some time before finding the reason. The new termination flanges were marginal in the isolation of the end insulators from deformations produced when making up the bolted joints. This problem was aggravated by residual stress in the new glass insulators. We tested different tube end terminations and finally decided on the solid chicken flanges. The SF_6 security valves are still in place to deal with the problem of gas loss in the event of a break under pressure. We had three glass breaks, one on tube number 1 and two on tube number 6. The breaks did not cause vacuum leaks and we have repaired them in situ.

Voltage tests have been carried out on individual tubes up to 4.7 MV except for tube number 1 which held between 3.7 and 4.0 MV. Twice we reached 13.5 MV on six tubes together (numbers 2, 3, 4, 5, 6, and 8). During this conditioning we had problems with many tube resistors, and the machine was opened several times. The long spark gaps across the tube resistors had loosened where the spark gap blades are screwed onto the resistor end termination. We found epoxy between the blade and the resistor.

The machine has now run many times at 15.5 MV with 75% transmission of a proton beam through the 90° analyzer. The machine went to 15.76 MV and is very stable. We still run with an ordinary belt but mounted in a half-open structure as shown in Fig. 3. We occasionally see sparks at high voltage, but apparently no damage occurs to tubes, resistors, or other components.

About 15 days ago we installed our second post-foil stripper between tubes 5 and 6 and accelerated a beam at 15.6 MV. At the end of last week we resumed our research program. Physicists had been unable to perform experiments for more than one year and we cannot tie up the machine for too many tests now. However, we hope to be operating at 16 MV very soon. We run with 8 kg/cm^2 SF_6 pressure with a 10% air contamination. We also have two Cs radiation sources of 3 Curies each in the pressure vessel.

We have made some other modifications to our machine. First we installed two SF_6 security valves between the accelerating tubes and the LE and HE drift tubes. They are operated pneumatically by SF_6 gas. Second, there are eight independent systems for short circuiting individual beam tube sections. Each one consists of a spring-operated take up reel with a steel rope return connected to a nylon string. All are manually operated from outside the tank. The third modification is a set of decoupling plates between resistors. During column tests at 15 MV we broke 50 new column resistors. After installing 90 decoupling plates between column resistors throughout the machine we repeated the tests and no resistors broke. We now have 90 plates for the column resistors and about 100 plates for the tube resistors.

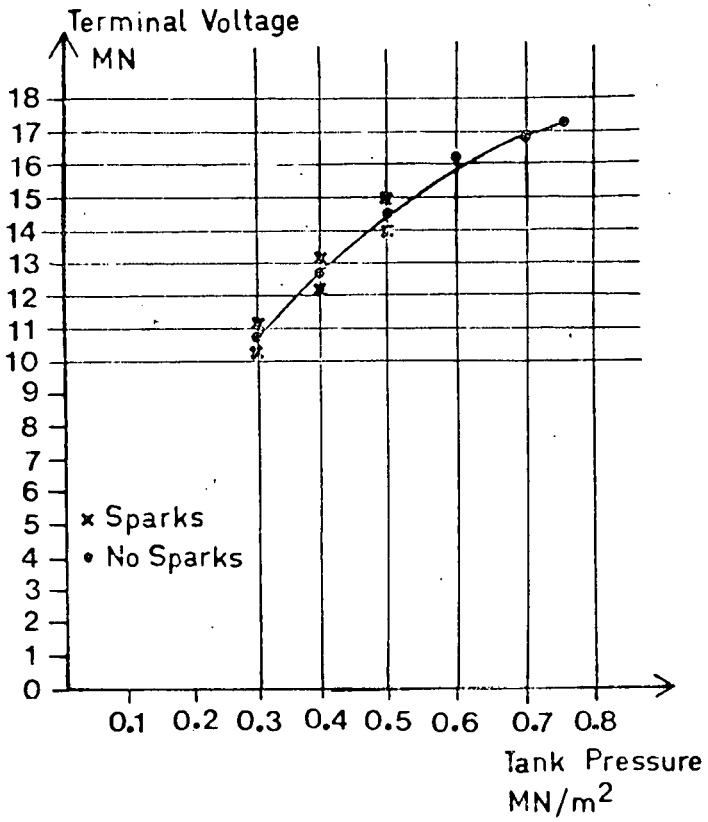

Fig. 1. Conditioning characteristics of the MP column without tubes.

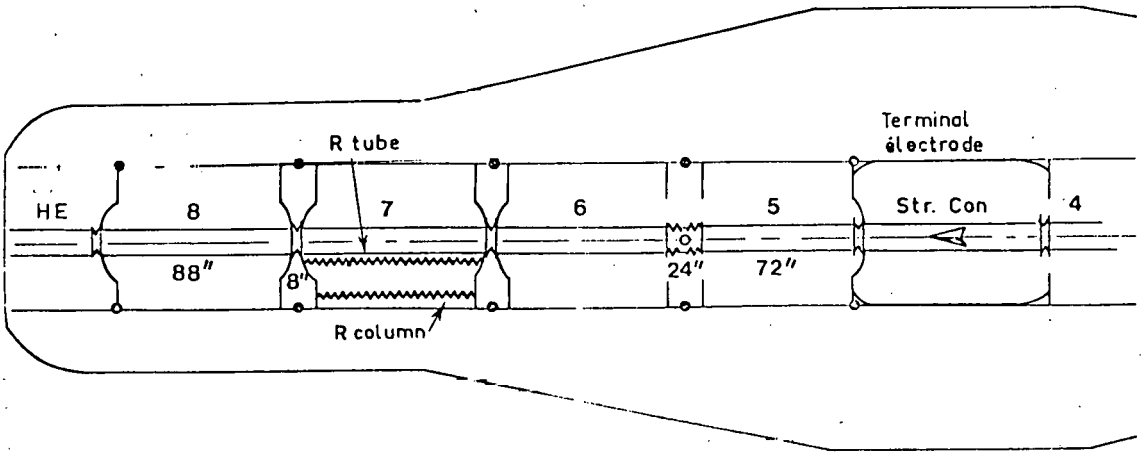

Fig. 2. Sketch of the HE column showing the equipotential lines with new 88-inch tubes installed.

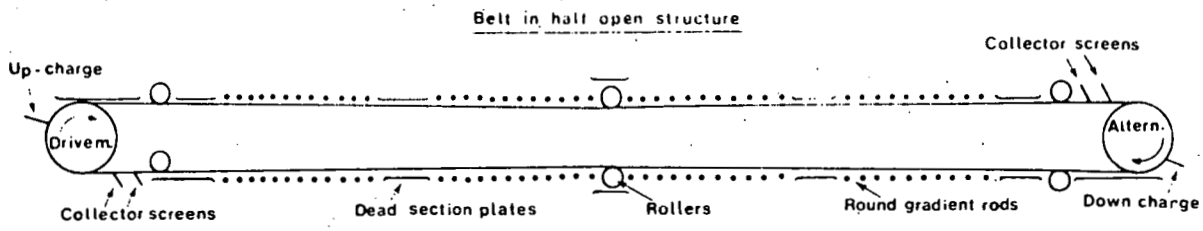

Fig. 3. Arrangement of the charging belt in the half-open geometry.

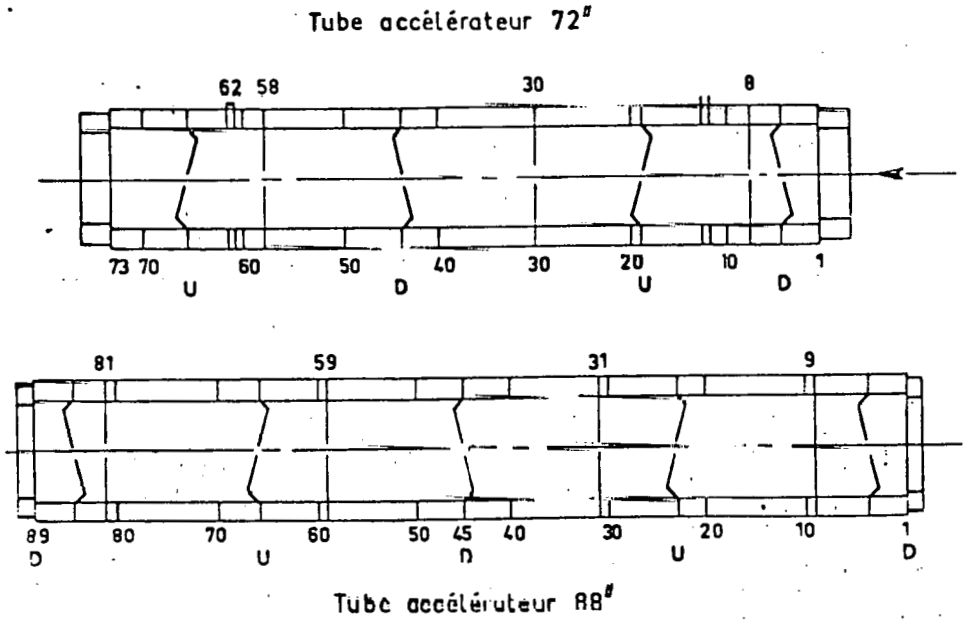

Fig. 4. Comparison of the 72-inch tube and the new 88-inch tube showing the orientation of the inclined field electrodes.

Chapman: These small bellows: are they welded bellows or formed bellows?

Letournel: I'm not sure but I think they are welded bellows.

Lund: Is there any provision to bias those apertures in the bellows sections?

Letournel: We attach the aperture at one side of the bellows but the aperture is at the middle of the bellows.

Lund: Can you apply voltage to them?

Letournel: No.

SNEAP Business Meeting

McKay: I notice that I'm scheduled for three-quarters of an hour. I don't think I'll take quite all that. There are a few things I would like to mention. There has been some questions as to the history of SNEAP and it might be interesting to those of you who are new to the organization. This is actually the fourteenth session of SNEAP, which just shows how precocious I am. I'm only 29 years old and here I am the father of this. There are fifty members of the organization at present, member being defined by the strange and mysterious rules of the secretariat (that's the name for my bottom filing cabinet drawer). These are made up of six commercial members and there are 44 institutions. Exactly half of them are American and half of them are from outside. We are represented on every continent except Asia, and I'm working on a couple of possibilities there. That just shows how extensive the Northeast really is.

I would like to repeat Don Walker's sentiments from this morning for the very smoothly run SNEAP. Having done it before myself, I know what problems can be involved. So I like to express my personal thanks and the thanks of everyone else in the organization to our hosts at Wisconsin.

The next item on the agenda is the financial report. We have a very complex accounting system. I xeroxed the accounts from the start of the organization on this page for any one who wants to examine them. We have something like $1,146.79 (Canadian) which is about 16% less if you translate to American. Those funds are available particularly for universities that run SNEAP to help cover expenses. We ungenerously feel that the big national labs can cover all the expenses, but sometimes it's difficult for a university or a smaller lab to take on the responsibilities. So that is the function of these funds. If you are going to bid to host SNEAP there is some money to cover it. Also, regarding finances, I would like to appeal once again to everyone to hand in his membership form. The most important thing is that I get the bottom section with an up-to-date mailing list so that we can keep contact with people. I've collected just about enough money to get home so I'm not too worried about the membership fee.

About the 1979 proceedings: I've talked to Charlie about it and they are well under way. He said that they have almost finished all the typing on them and they should be out in a matter of two or three weeks. I know there have been some problems there. Charlie was sick just after SNEAP and it is very difficult to do the minutes if you can't get at them right away.

There are at least two other items to discuss. I think it would be good at this point to talk about the nature of SNEAP and whether we like the way it's developing, or whether we have some suggestions on how we should change for the next year. This should serve as a guide for the next year's sponsor. And, of course, we have to pick someone out. I have a few comments, some of my own and some that I picked up from people who have left already. The lab reports that started last year in Philadelphia I think are very good. I think we might want to compress them a little bit. Perhaps instead of having written and oral reports from everyone, people could indicate whether they have something they would like to present at the open session or whether they will just give out the written report and allow people to get in touch with them. How do people feel about that?

Chapman: I think it would be quite nice if it were possible to have the section on the lab reports on the second day. You will get printed lab reports on the first day and have an opportunity the first evening to look them over. If there was a section for asking questions that were brought out by the written lab reports that would also be nice.

Billen: I agree. I thought that the main drawback of the system this year was that we spent a lot of time shuffling papers and did not get a chance to read a report before the author was supposed to get up and presumably summarize his report.

Larson: Has anyone thought of the possibility of asking each organization to reduce its report to one page or perhaps one sheet printed on both sides just to minimize the paper? This should be just a summary of the items of interest plus perhaps the names of people who could be contacted with regard to that subject. Anything worthy of more detailed description should be given in a session devoted to a specific topic. I think also with regard to condensation that if you expect 30 people to talk, that is going to take up a large fraction of the day. I think that's very inefficient use of time.

Berners: I really did not feel burdened by the amount of paper that was passed around in the lab reports. I was thinking that one way to reduce the length of report sessions could be to open them to questions only, and not ask for a presentation.

McKay: Especially if we had this on the second day.

Berners: The presentations are simply in printed form. Then during the report session there would be questions to the authors.

McKay: That would mean that you would have to come prepared to defend what you said.

Walker: That cuts into Monday evening, too by the way.

Noé: It would be best if these reports were available on Sunday evening.

McKay: I was very pleased to see how many people actually brought them. We can try to get them out earlier. The only other comment I've heard from people is that perhaps we should have the meeting a little bit more open, with more time for discussion. I had an interesting comment from the Los Alamos people about the business meeting. Their expense account people look at the schedule and conclude that 3:30 p.m. is the end of the real session. The business meeting doesn't count, so they're told to come back today. There is a request from the Los Alamos people that the business session (which is usually very short) be stuck in the middle of the meeting somewhere. Then our "real" session can go till 4:30 or 5:00 or whatever is appropriate on the last day. That amount of space could be very usefully devoted to open general discussion.

Walker: I agree, John. It seems to me that most SNEAPs in fact turn out to be two and a half day conferences rather than three. This might encourage people to stay around.

McKay: I didn't realize that there was this administrative problem. Are there other comments that other people would like to make on what we can do to improv the organization?

Burn: At lunch time I heard a comment about the Tuesday afternoon outing. I personally would like to see that kind of thing stay because I think that if you have a three-day meeting where you continually listen to presentations you get to a point where you want to get out for awhile. I think that a lot of useful work can also be accomplished on, for example, the boat trip. You know people are talking about their common problems. I would like to see that stay.

McKay: I think it's useful, too.

Noe: I think it should follow the discussion of laboratory reports, though.

McKay: That's a good idea. That's a good description of the second day that is shaping up. You could start with discussion of lab reports in the morning, and have the business session right before lunch. Then the afternoon would be left for the so-called social session.

Billquist: I have a comment on the subject of openness and the formality of discussions. I think that you may have noticed the number of people at this meeting who have not said a word. I think they tend to be the technical professionals as opposed to the scientific professionals (McKay: That's a pleasent distinction.), and I think it happens because the setting becomes scientific and if we're a little late close to break time discussion gets cut short. And in the conference room we've been very serious; there have been no jokes. I was tempted to ask Ed when he was giving his talk why he didn't follow what's become fashionable: to test his machine for voltage without the tubes in it. I didn't know if the levity would go over. Those of us without Ph. D.'s or masters degrees tend to feel inhibited just because of the format that gets tighter and tighter in terms of formal presentations.

McKay: I think that the goals of formal presentations and the size of the group have all gone against some of the informality. I think it is worth trying to preserve that or to bring it back.

Billen: We started out with the idea of trying to keep as much time as possible for discussion, and we initially had quite a small list of people, I thought, who were going to speak. It's suprising how the number of contributions can grow and how long some of them can become. It is difficult with formal presentations to reserve adequate time for discussion. You don't like to tell someone that he has only 5 minutes and then be forced to tell him to stop before he's finished. I don't know an easy solution.

McKay: Well, this is something that has to be left up to the people organizing the meeting, because they are the ones that have to do it. If you go way back to the ancient days, we had one ten-minute talk per session. Maybe it is time to be a little more ruthless and say we really don't think we need a full talk on this subject, and could you just make your comments where they work in. It depends on how nasty the organizer wants to be.

Den Hartog: I'd hate to see the number of presentations decrease because I think there is enough information in them that we all should be exposed to. There is no such thing as a ten-minute talk, apparently. Maybe we should have the session chairmen come a day early and learn torture techniques or something. Nobody wants to shut somebody off, but if we just did that, then the time for the informality that people desire would be here.

Chapman: If the lab reports are reduced, that will give us more time.

McKay: One of the things I was thinking about at lunch was that since the bigger accelerator conference is going to be at Oak Ridge in the Spring of next year, this could be an advantage for SNEAP. Some of the more formal presentations might fit in there better and we could get back to our informality.

Walker: When the first notice comes out there is usually a call for contributions such as "are you willing to talk about something" or "would you like to have so-and-so discussed". The minute you do that you're asking for some degree of formality and perhaps people feel obligated to prepare something.

Billen: I know that there are some people who can come to a conference like this only if they are going to present a paper.

McKay: I don't know how we can get around that sort of administrative ruling.

Burn: If you remember, John, back in the old days when we had only one ten-minute presentation by someone at the beginning of a two-hour session. The discussion following that would often get a little ragged and disorganized making it very difficult or impossible to produce any kind of coherent proceedings. I think the formal talks do tie things together. I agree with Pat; I wouldn't like to see the number reduced. I think that Jim has hit a fairly good balance. And if we reduce lab reports it should further ease the situation.

McKay: We could have three ten-minute presentations over a two and a half hour period.

Larson: I would like to suggest that the business meeting might fit well into the end of a free-running bull session on the second day just before lunch. Another comment. I find that if the discussion of individual papers is put off until several papers on the same subject have been presented, it is very stifling of questions.

McKay: Well then, I guess the consensus is that we've got to schedule more time for discussion to flow when something useful is happening.

Berners: I think you can really hold the papers to ten minutes with the standard alarm clock that rings a few minutes before the end and at the deadline and with a determined chairman. I also think that it is worth the trouble to try to achieve that because I missed having the unlimited discussions after papers that we used to be able to have when the meeting was a lot smaller. It is well worth the effort to try to get back to that.

Lindgren: One way of getting more time and still allowing more people to present papers (people who will, therefore, be able to attend the meeting) is to have them come and *present* the papers. They can be published in the minutes.

McKay: I can think of some things where it would be extremely good. For example, some of the electronics material is very impressive on the screen, but it's very hard to get much out of it. But it can be an extremely valuable thing to take home.

Burn: I agree with Ed that it is better to cut the length of the presentations than to cut the discussions as we did this time. There were often a half dozen people wanting to ask questions when discussion was cut off.

Larson: Giving a paper in ideal fashion is very difficult. Many speakers spend 8 minutes introducing the paper and then when the first bell rings they try to speed through the paper. That's a matter of personal disipline. There is the poster session. It's a way of getting many papers in without spending the time talking about them. For circuits and subjects of that nature I think that a poster is better than a presentation.

McKay: These are all things for next year.

Billquist: In order to really change anything at all we need to have someone hosting it who understands the changes he wants to make and has the backing of his home organization to do it. The problem is that we have had this same kind of discussion for three years in a row now. There have been attempts to change the format before. We have to decide who is going to host the meeting and put the pressure on that person.

McKay: That's the last item I had to discuss. We are getting to the point where we are going to have to draw lots to see who has it. Seattle has offered to have it next year, also Rochester, and we are very interested in taking it back to Canada to McMaster as well. I was thinking that perhaps next year you people who are interested in hosting SNEAP should send a letter saying so before the meeting so that people have a better chance to consider it. It's tended to be my phoning around before getting a feeling for who might be interested. It's really not the best way to decide where it should go. My own personal plug for taking it back to McMaster is that it has been five years now since it's been in Canada. We would like to see people at our lab.

Connolly: With the international meeting being in the United States next year, perhaps Canada would be the logical one to have SNEAP next year.

Walker: I'll go along with that Charlie. Also since John has several years of organization under his belt and if he's sincere about wanting it back at McMaster, then I would move that we go to McMaster next year.

McKay: Well, I've got two proxy votes for Seattle. Los Alamos would like to go to Seattle and presumably Seattle would too.

Den Hartog: Is there any possibility that we decide two years in advance instead of one. It might help the institution prepare a little.

McKay: A year is probably enough time. I would like to have proposals in a little more firmly though, and decided on at the middle business meeting. As you can see more than half the people are not here now.

Den Hartog: Jim was just saying that he had to reserve this building one week after the last SNEAP meeting ended.

Billen: That may be unique to this campus.

McKay: No, it is a problem in some places.

Burn: If you remember about five years ago one of the objections to having it in Canada was that there were restrictions on travel for all people in the U.S. Does that still apply?

McKay: Well, there was a rule about conventions. There has been a treaty signed by the appropriate heads of government but it hasn't been passed by your legislative people. I think there are two questions I would like to put to a vote. Where would we like to have it and also at what time of year should we have it. We could have it on campus, for example, in August, but if we have it later on in the year (we're thinking of the American Thanksgiving) we would have to arrange it in a hotel or perhaps at a resort some place away from the lab. Let's first vote on where we're going to have it. All those in favor of having it at McMaster... all those in favor of having it anywhere else... Well, it looks as though it will be at McMaster.

Burn: That's right John. You know how it should be run.

McKay: The other question I would like a few comments on is what do you think of having it not in a university but in a resort somewhere in a nearby area if we could find a cheap one.

Burn: Niagra Falls?

Den Hartog: That's a resort?

McKay: That's one possibility. Do you have comments on that?

Den Hartog: I would like to vote against August.

McKay: That's the next question. How many people would like the idea of finding someplace that's actually away from the institution?

_____: Toronto?

McKay: We Hamiltonians ignore that as a suburb. Does anyone object to that?

Walker: You might have trouble selling it.

McKay: I don't think so. It could also be cheaper. That's one question then that people are very open on. Now what about the question of having it in August as opposed to later in the Fall? I think we can put it to a vote. How many people would prefer August? It's nicer then... There's about 8 or 9 for August. How many people would prefer it later?... About five. I assume that there are some that really don't care when we have it.

Norton: When we went to set this meeting date there were very many objections to August and September from the group as a whole.

McKay: We have had it in August.

K. Ziegler: The international conference is in April. Don't get too close to that.

McKay: That's right. I can think of reasons for having it in the Fall. The reason I would like to have it in August is that I can have it on campus.

Den Hartog: What's the advantage of having it on campus?

McKay: We can have student accommodations which is quite good. The campus essentially becomes a conference center for the summer. Another thing we did for people who don't know the history of SNEAP was that we changed the time to March one year and went to Florida. Well, I think then that people don't have any violent objections to any of the things we have talked about. I think it will be dictated to a large extent by what practical arrangements we can make.

Billquist: I have no objections myself, but I recall the comments from people at several labs who are involved in operations that lead to personnel problems in the Summer with vacations.

McKay: I think there are real problems with that.

Den Hartog: Summer tends to be one of the busiest times.

Larson: May I change the subject?

McKay: Well, if there are not more comments on that and people are willing to let me do what I might intend to do.

Larson: I would like to recommend again the reasonably severely edited transcript of the conference. I thought it was getting too bloated and had too much redundant or irrelevant language in at least the last couple of volumes that I've seen.

McKay: I would certainly add my voice to yours and recommend to the editors to be very egotistical and to chop everything you think isn't really useful. I've done it in a rather strong fashion on a couple of occasions and had no objections. Now I'm not sure if that meant people agreed with me or just that they didn't bother reading it. I think everyone understands the difficulty of doing minutes and feels that the editor should take a free hand to produce a useful volume of information even if you don't catch every golden nugget that we uttered.

Gingerich: I gather that this organization does not have a constitution. I was wondering if that might not be such a bad idea to have one because it seems a little unusual that hosting should determine the flavor of the conference.

McKay: The organization consists of the following. I have one filing drawer which is SNEAP and promise to try and keep up the mailing list. Beyond that whoever takes on the conference does all the work. It makes for a very mobile organization and I like the fact that it changes from year to year.

Walker: If it was a little more formal, John, you could have a secretary.

Gingerich: The reason I was asking is that I expected this to be a very practical conference. This is the first year I managed to get here in three years. The other times when I showed the programs to my boss he said "No way, it's all ion sources". I think for a couple of years it was pretty heavy on ion sources. I am not interested in whether I know the progress of the 800-billion volt tandem in Outer Mongolia.

McKay: I think the value of the conferences very often depends on the informal sessions. It's hard to express that in a program. I know it is a problem convincing smaller labs to let their people come to it. I'm not quite sure what we can do about it.

Larson: First, I'd like to voice a vote for keeping it as informal as possible and as little organized as possible. I think that's a great asset. And if I may tread on some toes, this is about the fourth or fifth year I've been here at my own expense. As far as I know, no one else is ever stopped from going somewhere at his expense. The fact that I appear as a consultant may lead you to believe that I make a lot of money. But if you want to trade your salary for mine, I'll do it on the spot, sight unseen. I think people can pay out of their own pockets if they're interested in what they are doing. So if your organization won't support you, why not pull out a few bucks and come on your own?

McKay: That's an interesting thought. We tend not to look at that option, I must admit.

Larson: You could probably get here on paid vacation which beats my method.

McKay: Well, I don't have anything more to discuss. If no one else does I'll turn the meeting back over to Jim with great thanks and let him formally close the meeting or introduce any more suprise sessions.

Billen: Thank you. The meeting is formally closed.

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Our Reference
Your Reference

20th August, 1980

Dr. J.H. Billen,
Department of Physics,
University of Wisconsin,
1150 University Avenue,
Madison,
Wisconsin 53706,
U.S.A.

Dear Dr. Billen,

Thank you for your letter of the 12th August, 1980, regarding the forthcoming SNEAP meeting. As it happens I sent a telex yesterday to Mr. Norton at N.E.C. saying that unfortunately it will not be possible to send anybody from Daresbury. I had hoped that we could be represented, but in October we shall be starting the next stage of high voltage tests on our accelerator. The people who would be suitable for giving a talk at SNEAP are just the ones who will be most involved here. So, with great regret, I am afraid I have to decline the invitation.

If it would be of any interest, we could produce a short report on where we have got to, for circulation at your meeting. If you would like us to do that, please let me know. *

I hope you have a very successful meeting. The programme you sent looks very interesting.

All good wishes.

Yours sincerely,

Bob Voss

R.G.P. Voss

* Have had a telex from Greg Norton asking that we do produce a report. That is O.K., then.

Daresbury Laboratory
Science Research Council
DARESBURY
WARRINGTON WA4 4AD
Telephone: WARRINGTON 65000 Ext.
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Our Reference
Your Reference

27th November, 1980

Dr. G. Norton,
National Electrostatics Corporation,
P.O. Box 117,
Graber Road,
Middleton,
Wisconsin 53562,
U.S.A.

Dear Dr. Norton,

This letter is, I am afraid, too late to do any good, but I want to apologise to you for our complete failure to send you any material on the Nuclear Structure Facility for the recent SNEAP meeting.

The present state of the project here is keeping all of us very busy, and we realised, with considerable embarrassment, that we had left things too late. I can only repeat that I am sorry. I am sure, however, that you had a most successful meeting, unmarred by our negligence.

All good wishes.

Yours sincerely,

Bob Voss

R.G.P. Voss

TELEPHONE CALL

FROM: Mr. Larry Rowton
Los Alamos Scientific Laboratory

DATE: January 15, 1981

TO: G. A. Norton
NEC

Larry reported that there was an accident involving the service platform with the vertical Van de Graaff accelerator (McKibbon Machine). The service platform abruptly dropped approximately 18-20 feet.

The mechanical winch uses a double worm gear drive. This system is over thirty years old and there are no counterweights. The first set of gears disengaged and allowed the platform to drop freely.

They plan to add on-board brakes to the guide rails.

THE UNIVERSITY OF ROCHESTER
ROCHESTER, NEW YORK 14627

NUCLEAR STRUCTURE
RESEARCH LABORATORY

January 15, 1981

Mr. James H. Billen
Physics Department
University of Wisconsin
1150 University Ave.
Madison, WI 53706

Dear Mr. Billen:

Enclosed is a copy of a report we prepared as a result of a human error which caused the loss of the insulating gas inventory from our Van de Graaff accelerator. Our motivation in sending this report is that a similar costly accident could probably happen in most other laboratories, and even worse is the possibility of injury or death from not being adequately prepared to deal with the depletion of oxygen from the accelerator area or basement.

In particular, the health hazards need to be well understood by any laboratory members because this situation could develop with N_2-CO_2 machines as well as those with SF_6, if the inventory of gas is large enough.

Sincerely,

Terence S. Lund
Assistant Director, Operations

TSL:kpm
Encl.

Summary of the Loss of SF_6 Incident

October 25-26, 1980

T.S. Lund

Introduction

An incident occurred over the weekend of October 25-26, 1980 which resulted in the loss of approximately 90% of the inventory of insulating gas at the Nuclear Structure Research Laboratory of the University of Rochester. At the time of the incident, the insulating gas was a mixture of about 40% SF_6 and 60% N_2. The total inventory of the mixture was about 115,000 atmospheric cubic feet, and the replacement cost is estimated to be $60-$70,000.

Background

The accelerator tank had been opened for repair of cryogenic terminal pumps during the week preceeding the incident. The terminal pump repair was completed on Friday, October 24, 1980, and at about 16:00 hours, Kinney pump was turned on to evacuate the air from the accelerator tank prior to filling with the insulating gas mixture. During the time of the terminal pump repair, only the low energy beam right (south side of the tank) manway and the bottom manway had been opened. No other viewports or pressure seals had been disturbed.

Preliminary Events

The standard procedure for evacuating the air from the tank states that after 8-10 hours, the thermocouple gauge on the Kinney pump intake should read 300 microns or less. On Saturday morning, after at least 16 hours of pumping, the Kinney pump intake pressure was still greater than 750 microns. The accelerator technician who was monitoring the gas handling operations that weekend decided to let the pumpdown continue until the vacuum reached the specified 300 microns. At 17:00 hours, Saturday evening the vacuum still read 750 microns, and the accelerator technician realized the vacuum would not improve to 300 microns, for whatever reason, and went ahead to begin to fill the tank with insulating gas by following the published check list of the laboratory. The reason that the tank vacuum would not come down to the indicated vacuum of 300 microns is that there are a large number of seals on the tank that can leak under vacuum but not under pressure. The tank viewports are constructed this way and often have small leaks under vacuum which limit the ultimate vacuum that can be attained.

Subsequent Events

The procedure to fill the accelerator tank with gas says that the accelerator input gauge should not exceed 75 psia. This pressure essentially establishes the flow of gas from high pressure storage into the accelerator tank because of the conductance limitations of the piping. Also, the thermal

effects of the expanding gas limit the rate at which the accelerator tank can be filled. There is no provision for pre-heating the incoming gas. In order to be conservative, the accelerator technician on duty set the flow at something lower than the 75 psi maximum outlined in the procedure check list.

Another step of the tank filling procedure says to keep the Kinney pump running, with the pneumatic ball valve between the tank and the pump closed, and open the Kinney ballast valve in order to purge the Kinney pump oil of water that may have condensed in the oil during the evacuation of air from the tank. Two or three hours are necessary for this clean-up of the Kinney pump oil and it is convenient to do it during the tank filling operation.

The procedure in filling the tank is to leak check the manway gaskets and any other seals (such as view ports) that may have been opened during the previous tank entry whenever the tank pressure reaches 20-30 psi positive pressure. The manway gaskets, in particular, may often leak with a small pressure differential, and then seal themselves as the tank pressure increases. For this reason, it is difficult to leak check the manway gaskets at low tank pressures, and the gaskets will almost always seal as the tank is filled. A certain amount of experience and judgment is necessary during this leak checking to decide if the door gaskets are going to seal. In short, this is potentially a sensitive part of the tank filling procedure.

At this particular time, after the filling began, the accelerator technician had been checking on the gas handling system for about 12 hours, and he went home for some time until the door gaskets needed to be leak checked.

The technician returned at about 02:00 hours Sunday morning. He noticed that the Kinney pump was off, and restarted it. He also leak checked the two manway doors that had been opened, and left the laboratory in less than 15 minutes without realizing that anything could be wrong.

The technician returned to the laboratory at about 08:00 to finish filling the tank and to start recirculating the tank gas to dry it. When he looked at the gauges in the basement, he immediately realized that something had gone wrong, because all of the gauges read nearly zero. He went back upstairs to the control console and read the pressure on a precision gauge. The gauge read 20 psia instead of about 135 psia and he immediately closed all valves in the system and leak checked the door gaskets again. The low energy beam right door seemed to indicate a leak. Because of the excitement and nervousness over the incident, he was not sure what could have gone wrong, and he telephoned for some advice. Other people from the laboratory staff were not available to work on the problem on Sunday, and so it was decided that the remaining gas would be pumped into storage and that the leaky door gasket would be checked on Monday morning. He was cautioned, on the telephone, to check for a depletion in the oxygen level in the basement because SF_6 is heavier than air. Upon checking, he found

that the oxygen level was 4-5% below the normal air content. At least it was safe to proceed to pump the remaining gas back into storage.

Follow-Up Events

Monday morning, October 27, 1980, through discussions with the accelerator technician who had been on duty, the preliminary events and subsequent events as described above were recorded as rough notes. The indication of a leak on the low energy door could be investigated by at least three different ways: (1) open the door and inspect for proper installation of the gasket, (2) enter the tank through another door (using Survivair equipment) and inspect the gasket, and (3) fill the tank with nitrogen and leak check. In the first instance, upon opening the door, the gasket could fall out, as sometimes happens, and it could not be determined if the gasket was improperly seated. Choice (2) was slightly difficult, and possibly dangerous and so it was decided to refill the tank with some N_2 in other storage tanks.

While the tank was being filled, some calculations were made to try to understand the accident. With the time involved and the volume of gas lost, the leak rate was at least 150 CFM (12 hours and 120,000 cu. ft.). That large a leak rate, ruled out the possibility of a small door gasket leak. The machine was pressurized to about 60 psia, and there was no detectable leak at the door gaskets.

There are only two possible ways for gas to be lost from the gas handling system, assuming the system stays intact. That is, (1) through the tank let-up valve which takes air from the basement and lets it into the tank after all of the insulating gas is removed, and (2) the Kinney vacuum pump which exhausts outside the building when removing air from the tank.

The tank let-up valve is normally closed and the handle removed. Three people remembered that the valve was closed on Friday afternoon, and the handle was removed. The technician on duty during the weekend was also certain that he did not touch the let-up valve. Also, had the insulating gas been completely vented into the basement, or even into the accelerator room through a leak in the door gasket, the oxygen level in the basement could have been sufficiently depleted by the SF_6 that the technician might have been overcome by oxygen starvation. Because of these factors, the let-up valve has been ruled out as a possible source of the loss of insulating gas.

As he thought over the whole incident, the accelerator technician could not be sure that he did close the pneumatic Kinney pump ball valve between the pump and the tank, even though that step was checked off on the check list. The two things that are known are that the Kinney pump was left running when the gas filling operation began (standard procedure) and the Kinney pump was off when the technician leak checked the doors during the night. The Kinney pump is interlocked so that it turns off if the exhaust pressure exceeds 5 psig (20 psia). When the Kinney pump turns off, the pneumatic ball valve will close. If the Kinney pump pressure at the intake

side is greater than 10 psia (10" vacuum), the Kinney exhaust pressure will reach the trip point of 5 psig because of restrictions from the smoke eliminator filter on the outside exhaust line.

By testing with the N_2 from storage, we found that at a flow determined by about 35 psi on the accelerator input gauge, the Kinney pump could almost keep up with the flow. For a higher flow, the tank pressure would rise and the Kinney would trip off eventually, as described above. For a lower flow, the Kinney would keep pumping and the storage would be evacuated. As described in the subsequent events section, the Kinney pump was off at 02:00 when the doors were leak checked. This means that the exhaust pressure did reach 5 psi sometime before then.

The conclusion is that the pneumatic ball valve was not closed and the flow into the tank was just slightly larger than the Kinney pump could handle so that it pumped away about 90% of the gas before being tripped off. The test measurements with N_2 seemed to indicate that at the maximum flow allowed by the written procedures perhaps 30-50% of the gas would have been pumped away before the Kinney pump tripped off. Clearly, the conservative setting of a lower flow into the tank resulted in a larger loss of SF_6 during this incident.

Conclusions

A number of factors contributed to this incident and they can be summarized in this list:

1. Inexperience of the technician with the gas handling system operation.

2. Pressure from the research groups (whether direct or indirect) to get the machine running on the weekend.

3. Inadequate manpower available (more than one person should be involved in the gas transfer operations).

4. Gas handling operations at odd hours or during extended continuous shifts (as due to (2) and (3) above).

5. Inadequate interlocks and procedures for the gas handling operations.

The laboratory operation has never been specifically funded for operation more than five 24 hour days a week. The early operation of the machine, of 5 days a week, has gradually been extended to essentially 7 days a week. Also, during recent years, because of inadequate funding, the number of accelerator technicians has been reduced to one half of earlier levels (three technicians at present vs. six technicians five or six years ago). In fact, at the time of the loss of SF_6, the laboratory only had two technicians on the staff because of the resignation of the

Chief Accelerator Technician a month earlier. The two remaining technicians did not have as much experience with the operation of the gas handling system as they should have had.

Procedural Changes

Since this incident happened, the laboratory has completely reviewed the formal tank pump-out procedures, and a number of steps are being taken to reduce the possibility of another such loss of insulating gas incident. These steps include the following, in addition to the review of the written procedures: (1) A mechanical indicator has been installed on the Kinney pump pneumatic valve to insure visually and with microswitches, that the valve has closed. (2) A microswitch on the pneumatic reducing valve, which lets gas into the tank from storage, will sound an alarm if that valve is operated without closing the Kinney pump ball valve. (3) Microswitches and indicators will be added to several of the important valves in the gas handling system to make it easier to monitor the status of the system. (4) Two people must be directly involved with any operation of the gas handling system. (5) A more thorough training program will be developed to insure that the accelerator technicians understand the operation of the system, and do not just try to remember steps of procedures.

Perhaps the most dangerous aspect of this whole incident is the realization of the potential for accidental suffication by oxygen starvation. The dangers of oxygen depletion are well known, and this laboratory is one of the few Van de Graaff accelerator laboratories that has always maintained two Survivair breathing sets for just this reason. What has become clear as a result of this incident is that the accelerator technician could have become overcome by the SF_6 if in fact it had somehow vented into the accelerator room and basement. People have got to be trained to consider the possibility of an SF_6 problem any time they go into the accelerator basement. Just looking at the tank pressure gauge at the console, will take care of this situation, if the insulating gas is supposed to be in the tank, and is not in storage. A system to continuously monitor the oxygen content in the accelerator basement is needed. The easiest (i.e., least expensive) system that we have thought of is to use a gas pilot light assembly from a water heater or furnace with a thermocouple to detect the flame. The assembly could be installed in the floor well under the housing for the terminal support jackscrew. A warning alarm will be sounded if the flame is extinguished for any reason. This system could be packaged like a miner's Davey lamp to avoid any explosion of flammable vapors as from a spill of solvent in the area.

Experience with the Rochester Belt Charging System

Dan Kelly
University of Rochester

We have used the designs and suggestions of M. Letournel of Strasbourg in the Rochester MP belt charging system. We have modified the designs only to the extent of putting two rollers inside the belt because of space limitations. In Fig. 1 you can see that we are charging opposite the roller after the drive motor for the upcharge I and opposite the alternator in the terminal for the downcharge I. We collect the charge with a 'coarse' screen at some nominal potential and clean up any residual charge with a 'fine' screen. This is done for both the upcharge and the downcharge. The rollers are insulated and there have been high-speed bearings installed. The rollers are of the Strasbourg design. The rollers in the HE and terminal of the machine are mainly to guide the belt onto the drive motor and alternator. The rollers in the 6-7 dead section corresponding to the middle of the belt length helps control the flapping of the belt. As a result of the belt stability we have adjusted the belt spacers to within an inch of the belt. The rollers are 'run-in' for a few hours and then installed to rotate in the same direction. We have noticed a great decrease in the amount of belt dust and the terminal ripple as well.

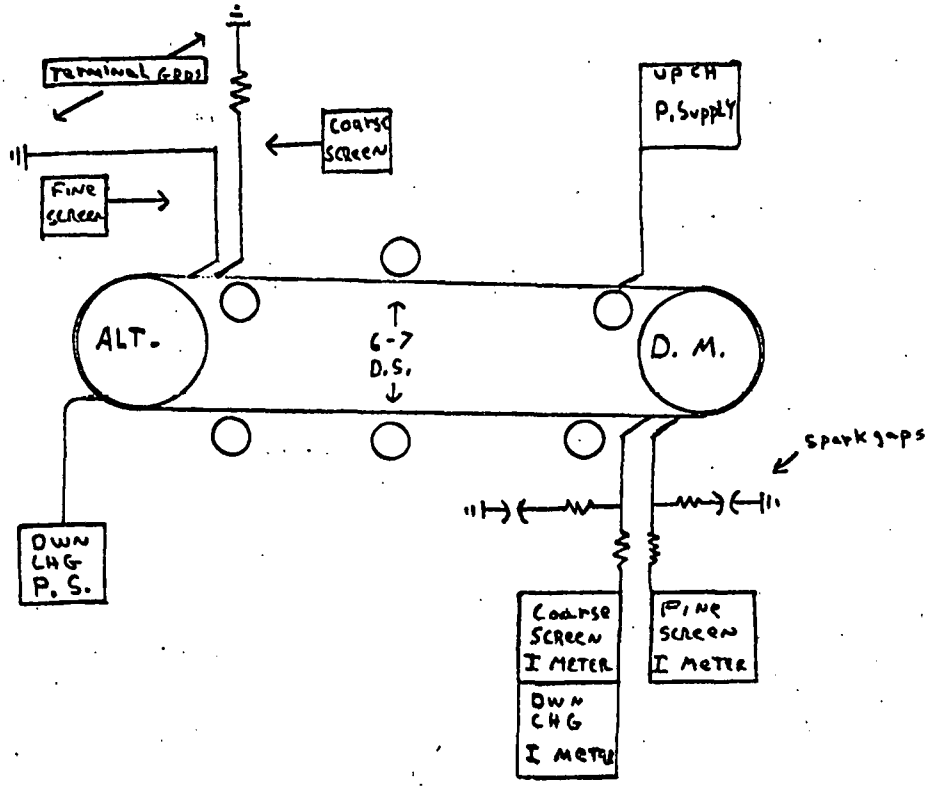

Fig. 1
Rochester Belt Charging System

Weitkamp: How much charge comes off on the fine screen?

Kelly: It's in the microamp range.

Weitkamp: Do you know what the fraction of the total charge it is?

Lund: About 10% comes off on the second screen.

Saylor: Do you have resistors in series with the screens?

Lund: The first screen is at ground. The second screen has about a hundred megohm (I'm not sure if that's exactly right) to induce high voltage on the screen.

Gingerich: Did you remove the spacer bars?

Kelly: We're running with guides on the outside only.

Gingerich: So it's not the exact system that Letournel describes?

Kelly: No, the spacer bars are 2 inches from the belt.

Lund: They are not the standard HVEC spacer bars.

Letournel: We have screens for both the up charge and the down charge. The first main screen has 0.020-inch diameter wire and is connected through 10 MΩ to the terminal. The second one is thinner (0.012 inch) and is connected directly to the terminal. The purpose is to sweep up all the residual charge not collected on the first screen. With a 400-μA current on the belt the second screen lowers the ripple from 5 kV to 1 kV. I have a question about one of the photographs that you showed. Why is there so much dust in the machine?

Kelly: The old belt had a tear in it and we have not had a chance to clean it all out yet.

Letournel: Our system is very very clean. The belt touches only the rollers.

Weitkamp: How much better is the ripple on your system compared to what you had before?

Lund: The ripple is 2 or 3 times smaller.

Berners: I must have misunderstood something. You are not suggesting that increasing the number of screens lowers the amount of dust produced, are you?

Letournel: No. The screens have nothing to do with the dust. That was a separate question I asked about the amount of dust shown in the slide. We took the belt guides out of our machine so the belt only is in contact with the rollers. That helps keep down the amount of dust.