CONF-781051

SNEAP 78

Proceedings of the Symposium of NORTHEASTERN ACCELERATOR PERSONNEL held at the Oak Ridge National Laboratory Oak Ridge, Tennessee

October 23—25, 1978

OAK RIDGE NATIONAL LABORATORY
OPERATED BY UNION CARBIDE CORPORATION • FOR THE DEPARTMENT OF ENERGY

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Compiled by

J. K. Bair and C. M. Jones

Edited by

G. D. Alton, J. K. Bair, J. A. Benjamin,
C. M. Jones, R. C. Juras, J. E. Mann,
W. T. Milner, C. D. Moak,
R. O. Sayer, and G. F. Wells

Date Published: June 1979

OAK RIDGE NATIONAL LABORATORY
Oak Ridge, Tennessee 37830
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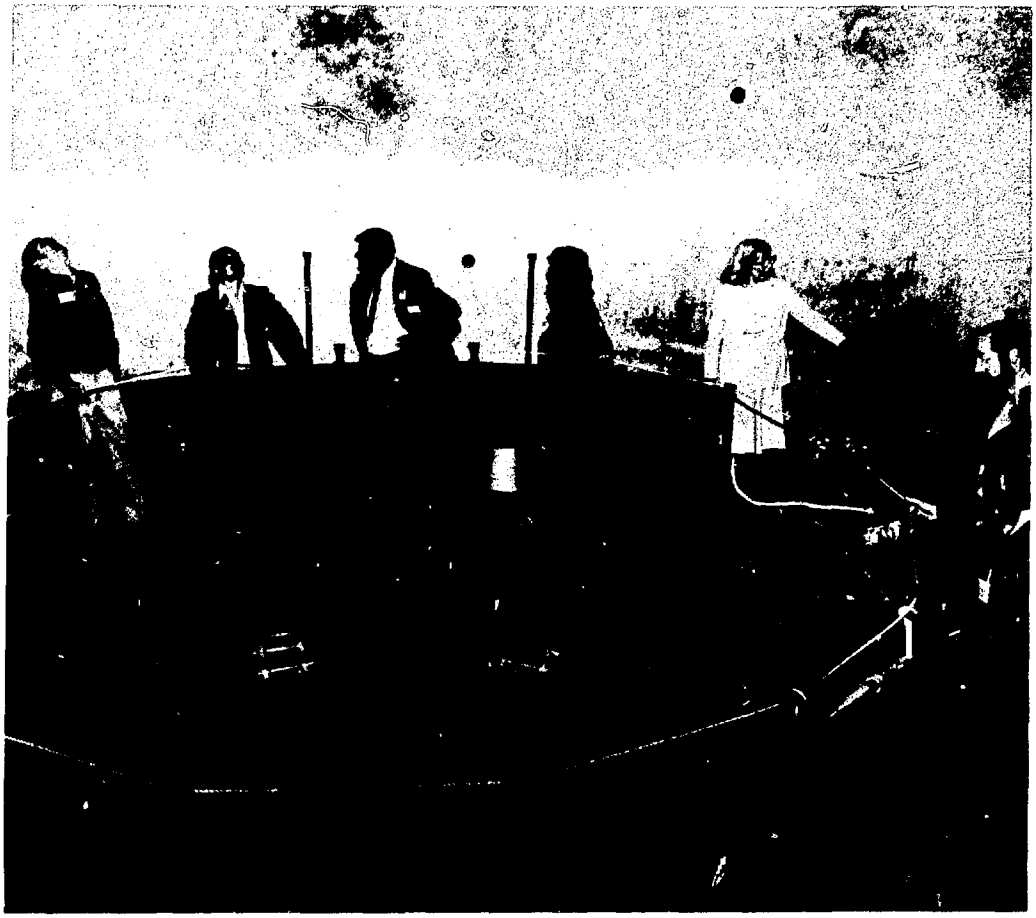
UNION CARBIDE CORPORATION
for the
DEPARTMENT OF ENERGY

1978 SNEAP Participants

Outside, Looking In



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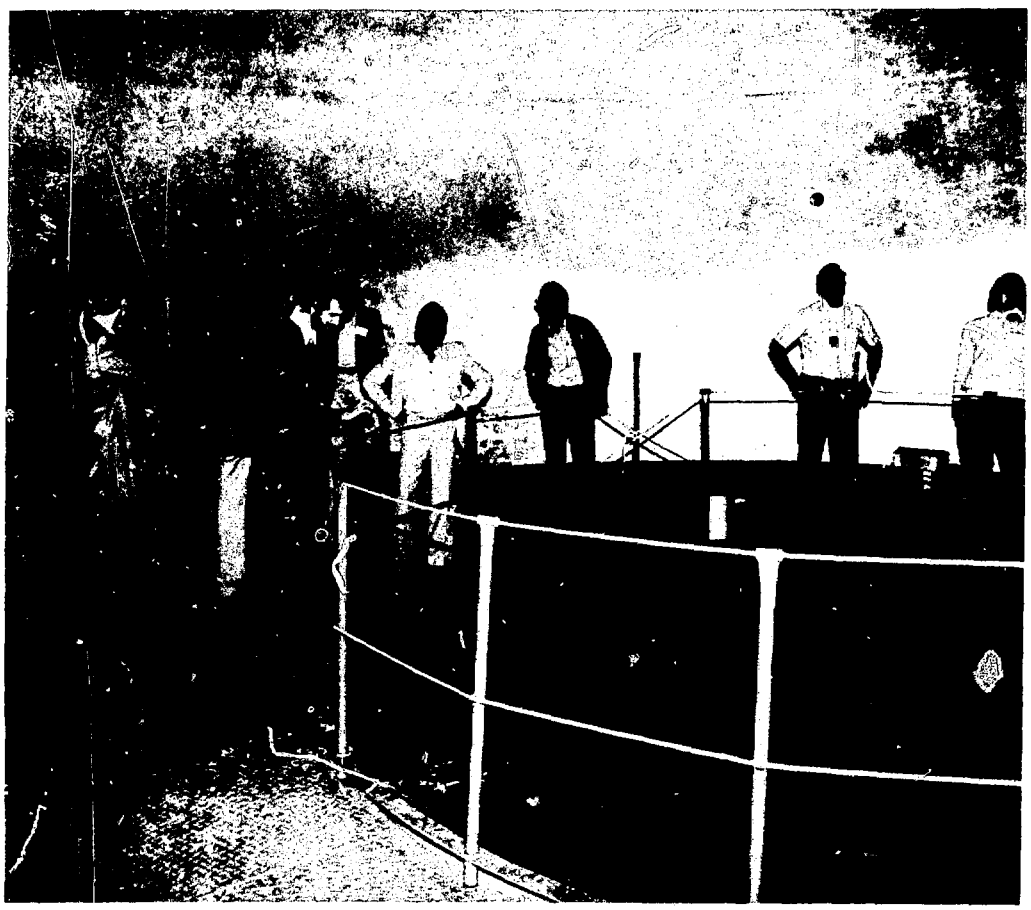
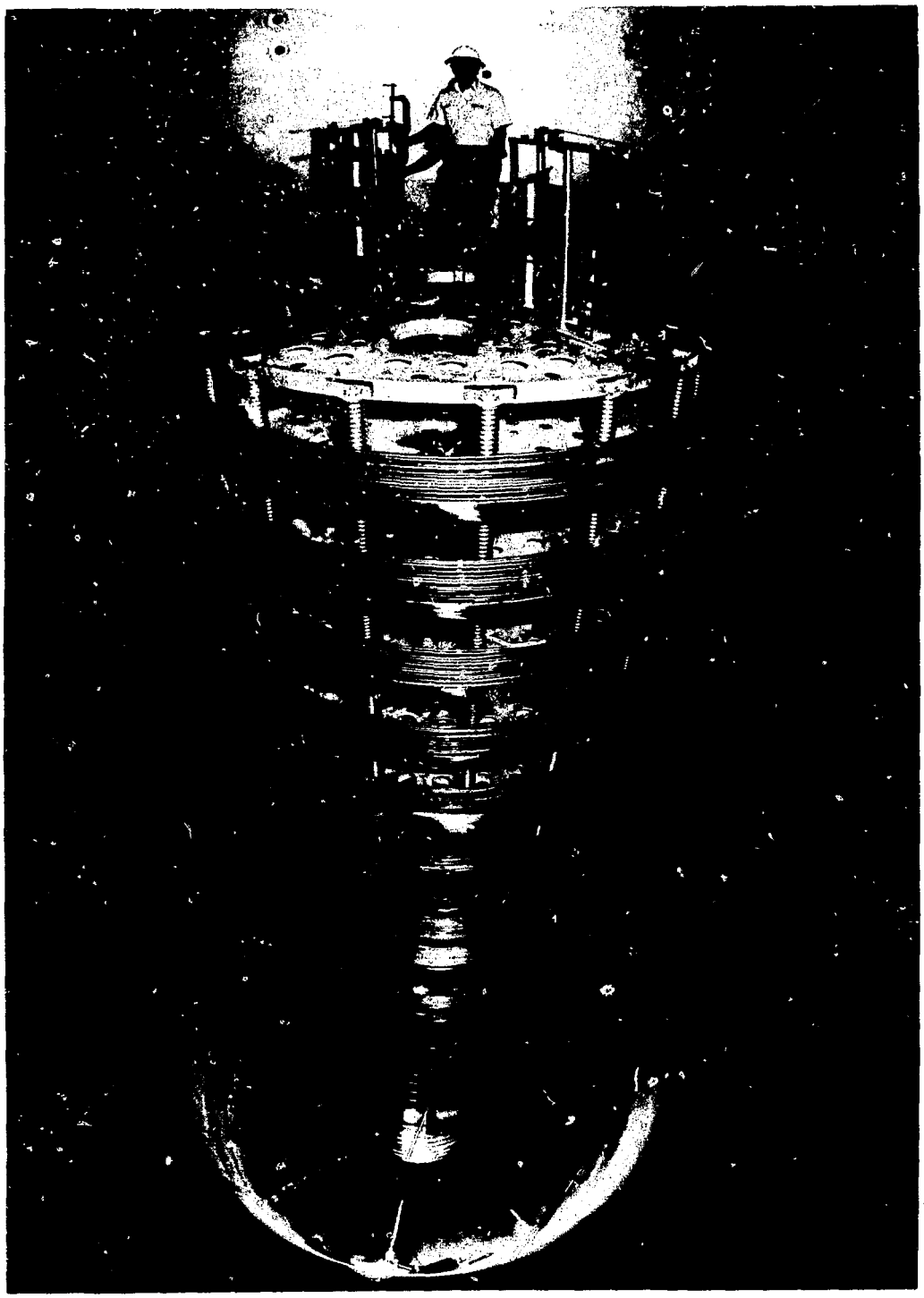


On Top, Looking

THE OAK RIDGE 25 URC COLUMN – FEBRUARY 1979



EDITORS' NOTE

These proceedings are a record of the presentations and discussion of the 1978 Symposium of Northeastern Accelerator Personnel held at Oak Ridge, Tennessee on October 23-25, 1978. In most cases, those speakers who gave presentations have transmitted written manuscripts of their talks. These are reproduced without change. A few presentations and the discussion in each session were transcribed from tape recordings and are presented here with only minimal editing.

We wish to thank the many speakers who provided manuscripts of their talks as well as Ms. J. K. Thacker for her assistance before, during, and after the symposium.

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WELCOMING ADDRESS

Alexander Zucker
Associate Director
Oak Ridge National Laboratory

It is a great pleasure to welcome this illustrious group of accelerator specialists to the Oak Ridge National Laboratory. We have turned out a special edition of our beautiful Fall colors for you, but I can tell you that to our eyes that big pile of concrete is more beautiful than any Fall colors. In fact, the accelerator tower is rapidly becoming the prime visible symbol of the Oak Ridge National Laboratory, and that is just as it should be.

I would like to say a few words about nuclear physics accelerators from the point of view of a large National Laboratory. In the past few years we have seen a move away from the small backyard accelerator to large, modern, new equipment in a few centers at universities or at National Laboratories. As funding for nuclear research remains nearly constant, we have seen accelerators shut down all across the country. We also see the conversion of accelerators from their traditional nuclear physics role to other tasks. Accelerators are now supplying various particles for studies in atomic physics. Accelerators are actively engaged in research on bulk materials and on the properties of surfaces and near surfaces. Electrostatic accelerators show a strong affinity for the Nation's fusion program: they are frequently just the right tools to measure atomic properties, important in plasma physics, and they provide the right kind of information in the study of radiation

damage, first-wall surface effects, as well as some measurements needed in the heavy ion inertial fusion program. From our point of view, then, we are observing a maturing accelerator discipline. While small accelerators are going out of style for nuclear physics, they are becoming sought after tools in other sciences. I personally see a bright future for accelerators, with one condition. It is no longer true that if an accelerator produces a proton or a deuteron at a certain energy, a customer will appear who will use that particle and that the customer will not only be able to do important research but will also have a purse of money with which to support the accelerator operation. This is a picture of the 1960's and no longer holds true. In fact, it is probably not even desirable. What is needed now is imaginative and aggressive exploitation of nuclear particles that can be produced by the imaginative varied arsenal of accelerators. With this attitude I believe accelerators will continue to find many new uses: isotope production, nuclear medicine, materials research, and, in a broader context, microanalysis, nuclear geology, nuclear archeology, etc.

To compound the situation, accelerator use is changing as well. No longer is an accelerator a captive device of its immediate physics community. The expense and size and power of any accelerator make it imperative that it be available to all qualified users. Whether we like it or not, large accelerators will only survive if they serve a wide user community and if they are perceived as serving scientists who propose the best research. The user mode of operation places special requirements on the accelerator designer and on the accelerator

operator. One requirement is efficiency. Accelerators must be designed and operated to run long hours reliably and stably. A second requirement is just emerging: the ability of an accelerator to serve more than one user at a time. The efficiency of accelerators can be much increased by providing several beams simultaneously. Some of these beams can be used for experiments and some for alignment and debugging of equipment. But the future clearly belongs to the accelerator that can serve more than one customer at a time.

A curious development of the last few years is the emergence of accelerator design specialists in the nuclear physics accelerator area. The operative mode used to be that when a laboratory completed an accelerator it had to propose a new one just to keep its engineers busy. What is happening now is that accelerators are becoming more complicated, requiring more specialized knowledge to develop. It is in fact becoming more and more difficult to find competent accelerator designers who approach the problems in new and imaginative ways. That I think is an exciting development. It is also a sign of the health of a field and a sign that the field is striving to achieve the utmost. I personally can hardly wait. I want to see what the next generation of accelerators will look like, what great new things will be achieved, and what interesting research will be done with them in nuclear physics as well as in other areas of science.

I wish you a good meeting and I look forward to many new and interesting things that will come out of it.

Session I, Chairman, R. Woods
Editor, C. D. Moak

Conditioning and Breakdown Phenomena in Accelerator Tubes

S.J. Skorka
University of Munich, Germany

All large tandems are limited by the voltage holding capability of their tubes. While without tubes many MP's and 14 UD's were demonstrated to hold 15-17 MV, they spark at 13 to 14 MV or less with tubes installed. In NEC-machines most sparks are tube sparks while most upgraded MP's generally break down by tank sparks apparently being triggered by the tube.

In spite of their importance only very little is understood how large breakdowns in electrostatic generators develop. On the other hand we have at our disposal a number of mechanisms explaining the discharges and breakdowns between individual electrodes with small or larger gap. Most of them have been demonstrated to in fact take place under certain conditions, but still many details are unknown, many effects unexplained in particular the transition effects leading from the local events to the total breakdown of the generator.

In the first part of this paper I shall review shortly some of the more important breakdown mechanisms well established since some time (for a detailed discussion see also Ref. 1).

The second part will be dealing with some discharge phenomena in NEC tubes deduced from looking at the electrodes and insulators of a used tube.

Surface breakdown

As shown schematically in fig. 1 a tube and column breakdown, starting somewhere along the tube, leads to high overvoltages due to the finite firing time (1-10 ns) of the protection spark gaps.

A weak, maybe the weakest point in the tube, is the insulator surface.

Under the condition of large overvoltages (e.g. 5 times the static voltage) field emission from the negative corner is greatly enhanced.

Condensed molecules, if desorbed from the insulator surface and ionized by electrons can within nanoseconds feed charge carriers into a discharge channel close to the surface, supporting a very fast flashover. Rapid development of surface breakdowns within 1 ns were indeed reported[2].

If the breakdown at the inside is faster than at the protecting spark gap on the outside, a large amount of energy can be deposited inside the tube and can damage the electrodes or the adhesive film (PVA or aluminum). Electrode material or material from the glue is evaporated or sputtered onto the insulator or electrode surfaces and cracked molecules eventually develop a fine track across the insulator growing by repeated breakdowns.

Grooved insulator surfaces impede the mechanism mentioned above to some extend, but are subject to mechanical damage under spark conditions. Also, the relatively rough crystalline finish of ceramic surfaces might slow down the development of a surface flashover.

Microdischarges

A microdischarge consists of an ion exchange avalanche maintained between electrodes by positive and negative ions.

If the regenerative process has a multiplication constant K exceeding unity the current increases exponentially until quenched by lack of adsorbed gas molecules on the surfaces or by breakdown of the voltage.

K depends on the voltage between the electrodes, not on the electric field, and on the surface density and composition of adsorbed gases as well as on the chemical composition of the surface. Chemicals can increase K catalytically by large factors.

Between parallel electrodes the microdischarge is more or less localized. In NEC-tubes with the inhomogenous field between the 1"-apertures the whole acceleration tube between ground and terminal can be involved. Fig. 2 shows an example of the propagation of a microdischarge in NEC tubes based on computer calculations of the trajectories and assuming a multiplication factor K = 1. The average propagation speed is about 8-10 gap crossings per 8" tube. Since the flight times from 1"-aperture to 1"-aperture are roughly 50-200 ns depending on the ion mass, the propagation speed can be expressed as 0.4-2 ms/8" tube near maximum voltage across the gap. At Munich we observed microdischarge durations of 2-6 ms, indicating that the microdischarge is confined to a few 8" submodules, at most to the 10 submodules between adjacent dead sections within the MP.

The microdischarge is accompanied by a γ-burst from electron-breamsstrahlung, by a vacuum rise due to the

released neutral gas, by a dip of the terminal voltage, and by a flash of visible light.

Each discharge reduces the amount of adsorbed gas molecules on the surface somewhat and might therefore allow the applied voltage to be increased. This is part of the conditioning of tubes.

In relatively clean tubes like the NEC-tubes the microdischarge might eventually be supported by the electrode materials itself, that is by Ti^+ ions and by O^- ions from the oxide layer on the titanium. This was first shown by Prichard[3], who registered the particle species involved in the discharge. Oxygen on the surface can enhance the negative ion yields.

With increasing residual gas pressure (of a "good" gas like N_2 etc.) the multiplication factor K is reduced due to charge exchange collisions (quenching gas).

Microdischarges of moderate intensity might be desirable to prevent the development of whiskers on the electrodes. The protrusions are kept small by sputtering effects of the bombarding ions.

Field emission

At very high fields electrons are extracted from the electrode surface via field emission.

On a clean surface at room temperature fields of 10^3 MV/m are necessary to get measurable emission. But oxidised surfaces or surfaces contaminated with metals (e.g. caesium) or organic molecules can emit large electron currents at fields as low as 0.1 - 10 MV/m.

Appreciable quantities of electrons are emitted from whiskers or aspersions. If the residual gas density is sufficiently high self-distroyment of the protrusions takes place from bombardment by ions produced by the field electrons.

Field emission is accompanied by more or less steady x-radiation and sometimes very little pressure rise.

At normal (low) residual gas pressure electrons produced by field emission or during microdischarges discussed above do only to a very little degree support cumulative processes, since their impact on surfaces is too small to liberate other than very loosely adsorbed molecules. This might be different at elevated gas pressures and/or very high electron itensities during the transition from a localized discharge to a tube spark.

Clumps (microparticles)

When a microparticle (typically r = 0,1 - 10 µm) hits a surface at very high velocity the electrode and/or clump material is melted and evaporated, a crater with rims and spikes is produced. The vapor cloud is partly ionized by field emitted electrons. Enough charge carriers may be produced during this processes that a complete breakdown of the gap is triggered. The necessary critical minimum velocity of a clump to penetrate into the surface is highest for titanium which owns its surface strength to its very resistent oxide layer.

Microparticles can arise as remainders from sandblasting or polishing, as dust particles or as less tightly bound parts of the electrode surface or the ceramic insulators.

They also may arise as droplets being formed from whiskers which under the influence of field emission become thermally unstable until they melt. The liquid metal is then torn away and accelerated across the electric field.

The removal (deposition at less critical locations) of the microparticles is part of the conditioning of tubes. At increased field strength more tightly bound particles can appear. They can also be shaken loose under spark conditions.

Vacuum arcs

The vacuum arc is the most violent form of a discharge between metal electrodes. Vaporized and ionized metal atoms from the electrodes serve as charge carriers. The discharge can carry currents of mA to Amperes, depending on the surface condition oxide layer etc.) and materials. Current contraction leads to relatively small diameters (1-100 µm). In general the arc is time limited by the available charge on nearby capacities and may last 10-100 ns. It might damage the surface by forming a crater.

Arcs can develop from field emitting thermally instable protrusions or by the impact of vaporizing microparticles.

Discharge phenomena observed in NEC-tubes operated in the Munich MP

The upgrading of the MP-Tandem at Munich turned out to be particularly difficult and troublesome due to the repeated contamination of the NEC tubes by excessive organic material introduced during the production and shipping procedure. It is likely that the tube damages described in the following section do not occur in clean tubes - at least not to the extend observed at Munich. +)

Due to the high energy ions can gain between the 1" apertures one would expect microdischarges to grow more easily between those, while the small gaps carry a larger surface field favouring field emission, vacuum arcs and insulator surface flash overs. It has turned out, that in the Munich

+) After five years of use a tube removed from the 14 UD at Canberra showed similar appearance[4] of the surfaces although not so pronounced.

tube the important and limiting part of the breakdown activity takes place between the small electrodes. This is rather unexpected and might be due to the existence of the organic impurities mentioned above.

During July 1978 a few of the submodules had to be replaced since they limited the voltage performance of the machine more than the other tubes and could not be conditioned.

The insulators of these submoduls display many dark looking clouds consisting of titanium and titanium oxide and maybe some aluminum. The structure of the dark areas on the ceramic is not particularly parallel to the field suggesting condensations or sputtering rather than tracking.

Fig. 3 shows an example of such a cloud being particularly dark. This cloud is, maybe, the reason for the breakdown of this tube. The coating of the insulator surface was so heavy that a conducting track developed. The evaporated material might have been originated from the shiny areas on the titanium. The aluminum fillet looks good.

Fig. 4 shows a titanium surface and the aluminum interface more closely. The shiny marks or stripes are connected either to the fillet or to the other edge of the ring.

Fig. 5 shows a typical pair of electrodes. One side cathode one anode. They were mounted looking at each other, unfortunately not orientated correctly on the photo. Many of the shiny areas are located opposite to each other. All of them are connected to one edge, suggesting that they originate somehow from a discharge through the whole submodule along the edges of the inserted electrode.

The dark areas are of yellow-brown color. γ-backscattering experiments showed that they carry a layer of about 500 Å

of Silicon-and/or Aluminum-oxide and Titanium oxide. The shiny patches are pure titanium metal. No other chemical coating was discovered, although it is difficult to detect an organic compound of only a few monolayers by this method.

Note that there is no apparent difference between cathode and anode side.

The following figures are microphotos of various parts of the electrode surface.Fig.6 shows one of the shiny areas on the bonded titanium only one mm apart from the aluminum fillet. X-ray diagnostic has indicated that the shiny surface contains some aluminum but not all the shiny structures consist of aluminum. Electron microscope photos rather indicate a landscape of melted titanium.

On Fig. 7 one can see magnifications of one of the bright, shiny patches on an anode surface of an inserted electrode.

These photo prove the shiny areas to display melted titanium rather than the effects of sputtering. One might speculate that the irregular pattern is the residue of many , many craters later on smoothed out by ion exchange sputtering. Still later in time a few new craters were superimposed on the old pattern.

The remaining 4 figures are photos from the cathode of an inserted ring.

Fig. 8 shows a normal (discolored) surface area. The appearance is completely different. The linear structures typically stem from the manufacturing of the rings.

Fig. 9 reminds very much of the figure 7: an old melted surface with new craters. This photo was taken at a shiny area in contrast to the former one.

Fig. 10 shows a very good example of microcratering, a landscape of very regular structure as if one crater is placed next to another one by some surface destructive mechanism.

Note again that there is no marked difference between cathode and anode. Multicratering seems to be polarity independent, or takes place during the ac-like conditions of a spark.

The enormously large number of craters suggests that they cannot each be connected to one tube speaks, not even to CPU voltage dips noticed at the control desk. It is rather likely that they are produced during localized breakdowns between the small electrodes in form of a short vacuum arc of possibly 10-100 ns duration.

The available electrostatic energy for such a discharge would be approximately $3 \cdot 10^{-3}$ Ws. Assuming a spot size of 10 µm radius and 5 µm thickness, the involved mass of crater material would be of the order of 10^{-11} kg. To heat this amount, to melt, to evaporate and to ionize it to the extend necessary for the development of the vacuum arc only 10^{-4} Ws are needed. So even a partial breakdown of a small gap is sufficient.

This quantitative comparison does not explain why such an extensive surface deterioration takes place.

Shiny patches on electrodes containing large numbers of craters with circular rims and a pit in the center very similar to fig's. 6, 7, 9 and 10 have been described by other authors[5] too. As compared to what was described above the surface damages appeared at considerably higher fields (although similar potential differences) and were observed on mild and stainless steel electrodes. The structures were different on anode and cathode, but identical if ac voltage was applied.

Other authors[6] have described the growth of whiskers (finally leading to discharges) by field polymerization[7], that is the growth of microneedles in the presence of organic substances. The sudden removal of such an organic protrusion by a major discharge could produce a crater. Cratering by microparticles was described by Menon and Srivastava[8]. At the low velocities gained between the small electrodes of NEC-tubes microparticles can still trigger discharges during their approach to the cathode. Very large numbers of microparticles ($10^3/cm^2$) being composed of the electrode material were observed.

From a more practical point of view one might ask the question: do all the surface damages to the insulators and electrodes limit the voltage performance of the tube?

It turns out that the originally conducting clouds of Ti metal on the insulators can be oxidized by letting the tube up to an oxygen pressure of 10^{-5} Torr for an hour or so. As TiO_2 they do not seem to be harmful.

The shiny patches on the electrode surfaces consist of bare metal. Compared to metal oxid their tensile strength is greatly reduced. Protrusions can grow more easily. Their work function is increased, consequently microdischarges are somewhat less likely, which tend to sputter the protrusions away before larger breakdowns occur. It is, therefore, possible that the shiny areas are less stable than surfaces protected by an oxid layer.

To understand the origin of the surface destructions and their influence on tube performance or to avoid such destructions is certainly very important for future developments.

Acknowledgements: The microphotos were prepared by R. Berisch and H. Kukral, Max-Planck Institute for Plasmaphysics. Their collaboration and valuable comments are greatfully acknowledged. I am also indebted to H.R. McK. Hyder for stimulating discussions.

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Discussion:

Purser:

Dr. Skorka has mentioned the number of very interesting observations. One which surprised me was the fact that in the analysis, there were layers of titanium which were mixed in with aluminum and oxygen to a depth of 500Å, I think he said. It's curious because the normal depth that you would expect diffusion of such things to be, is very much less and that says that probably some type of implantation has taken place with these foils and one has to ask the question, is there always a lot of aluminum and oxygen around, even though the vacuums are very good? Its not obvious where that comes from but clearly it could come from the walls. I think the other interesting comment, or the comment which I found very intriguing was the fact that the areas where there was a lot of this cancerous activity from the tubes apparently was clear of these materials, and in fact, was pure titanium. Its an interesting fact, too, that if you have a pure material on the surface then the lattice sites are very degenerate so you can imagine that under those conditions, it's much easier for atoms to move from one site to the other on very small electric forces. What this in turn leads to is the question whether, in fact with these stable surfaces that one would like to have, probably would have to have something other than pure metals on the surface. And once you've made that assumption, one has to ask, is it, in fact, ideal to have the very best vacuums that you can have in such an acceleration tube and does one, in fact, want to have some type of impurity in the vacuum acceleration tube that will make the right material on the surface. Finally, in this very convoluted question, one has to ask the question, whether things like carbides, which are very well-known to be stable, and which may, in fact, be formed by implantation of, say, carbon from methane or from the carbon that is present in polyvinyl acetates may, in fact, be a very important consideration in the behavior of such tubes?

Skorka:

Its very well known that titanium oxide has a much higher tensile strength, and also surface migration forming protrusions is less. In addition, the work function of the surface is decreased by oxide and much higher on the bare surface. The microdischarges between oxygen and titanium are quenched if you have a very clean bare titanium surface. The microdischarges might be an important part to stabilize the surface as I indicated before. Maybe its good to look at a surface as being, sort of moving all the time, that protrusions tend to build by surface migration but microdischarges keep them down at the time and so one

has, sort of, good surfaces as long as the gas pressure is not too low and as long as the surface has enough tensile strength as it would be in titanium oxide. Some oxygen in the tube is also important to oxidize evaporated titanium once it came to the insulator.

Wegner:

Is there any evidence that you have any acceleration of either alumina or silica dust particles possibly from the fabrication processes of the tube that are left over in sections? In other words, is there a migration of any microparticles of any kind and could they contribute to any pitting that you find?

Skorka:

Yes, there is evidence. We have in our first set of tubes seen a lot of dust particles but NEC has improved the cleaning method. And our new process tubes show very little, but there are still some and we have discovered in electron microscope photos some such particles which are very very small. But as I said, they, in general, need a longer path to develop enough energy to really damage the surface. Only, in cases where charges are very high by some reason, can they gain enough energy in a small gap like this so it is hard to believe microparticles play a very important role.

Foster:

Is there any indication that electropolishing would help on the electrodes or may they already do this? Would you address that, please?

Skorka:

As far as I know, NEC does not do very much to these surfaces. They just have their titanium oxide layer and are produced just from that material. Maybe you can make a comment on polished surfaces?

Rathmell:

You indicate that you think some of these discharges along the surface and the positive-ion negative-ion exchange can take place within, well, some number of nanoseconds or up to 50 nanoseconds. How do you arrive at these times?

Skorka:

Just by calculating the acceleration the particle in the tube in the electric field by a rough estimate of the acceleration of the particle starting from rest.

Rathmell :

But the discharge along the surface requires several smaller preceding discharges.

Skorka:

Yes. In the case of the microdischarge, I just can calculate the time that is necessary. For the flashover across the surface, the main point was that in this case, the charge carriers or the molecules which were produced by ionization from the charge carriers need not come from the electrodes. They come from the side and can feed flashovers by only moving a few micrometers, for example. One micrometer per nanosecond is the speed of gas molecules under normal temperature conditions. Thats the advantage (or disadvantage) of a surface flashover that can be very fast in reaction to a steep rise in the voltage across the gap.

Rathmell:

Now there were certain bright spots on the electrodes and your micrographs showed that these were very structured with many pits. Would I conclude then that that's not the result of a single large spark but the result of many discharges?

Skorka:

Yes. If you make an estimate about the number of craters assuming that one is produced after the other, and this seems the case from the overlap of the rims if you look closer. We never had so many millions of sparks. There are many many millions of these little craters. They cannot even be produced by events which you see on the CPU, that is dips of the voltage. They are also not enough, if you add them up for months, so they must be produced by very small events which do not show up and are localized and do not spread across the larger part of the tube to produce a voltage dip.

I should mention, because representatives of NEC are here, it is, of course, not proven that any of these changes or damages to the tube are harmful to the performance of the tube. At least at this time it is not known to me whether they are really harmful. We learned that these clouds on the surface of the insulators can be conditioned just by putting in 10^{-5} Torr oxygen for one hour into the tube. Then they are all oxidized and work as good insulators again. We do not know because we do not push our machine too hard at the moment whether these white areas prevent the tube from going up to rated voltage.

Burn :

My question is almost the same as Bob's. You stated that you had measured microdischarge duration times in the order of 2 to 5 milliseconds?

Skorka:

Yes.

Burn :

Was that also measured by calculation or did you observe them?

Skorka:

No. We observed the length of the gamma flash with sodium oxide crystals and we saw the length to be between 2 and 5 milliseconds. And if you divide this by the time necessary for one crossover per particle of 50 nanoseconds or so you get about 50-100 exchange processes necessary to produce such a length.

Chapman:

Could you say just a few words about what is the present state of the voltage holding capacity of your accelerator with the new tubes? Now you've had some appreciable conditioning times since the Ebeltoft Conference.

Skorka:

Yes. We stopped doing too much to the machine because experimentalists wanted the machine. The machine is running since the new tubes were installed and from time-to-time we try to condition. During the first few months after the new tubes were installed we saw deconditioning effects very similar to ones in contaminated tubes. After a spark the conditioning limit was down and after running the machine below the conditioned limit after a while the spikes on the CPU came up etc. This is typical of the migration of an organic or some such dirt in the system. Also after we had conditioned individual sections (sets of ten in the MP) to a certain voltage, say 3.2 MV, after a few weeks when we looked again only at this section with shorting rods the conditioning limit was down. So we had deconditioning. This disappeared recently. We now notice that a conditioning limit, once reached, stays for weeks. So the status is now that we have conditioned the tubes up to about 11 MV and we are running with only one tube spark per day at 10.6 - 10.7 and we want to go very slowly in increasing this voltage by

conditioning every two weeks or so a little further. This conditioning must be done section-wise. Otherwise, one gets too many tube sparks. This is a surprising effect. With one, two or even three sets of ten connected together the whole system behaves linearly and one gets many little CPU dips when one is very close to or at the conditioning limit and one get x-ray flashes etc. But with all four together the probability of a spark is much larger. One can say that the ratio of sparks to the little dips increases with the length of the system connected. With four sections connected, sparks come without any notice (without dips) when one is near the conditioning limit and with one set one has many little dips but no sparks.

Adams:

We have the HVC titanium tubes in our machine. And one phenomenon we observed is the conditioning goes up quite rapidly and we can condition very quickly. Say we condition to 8½ million volts. As soon as we put the beam in the machine through the tubes we see conditioning set in again. This works itself out within two hours or so depending on the energy. Do you observe the same phenomenon?

Skorka:

No. No. There is no difference with and without beam, but I must say we have apertures in the dead sections which prevent part of the beam hitting the electrodes. Rehovoth has reported in Ebeltoft that they have a lower operating voltage with beam than without beam. They are now running at 13 MV without beam and at 12.6 MV with beam with about one tube spark per day.

Larson:

I would like to resolve different phenomena that you have spoken about. The figure that you showed with beam trajectories was between the 8 inch sections.

Skorka:

That's right.

Larson:

And you have correlated that with the microdischarges lasting milliseconds. On the other hand, the pitting and the phenomena you showed in the photomicrographs are a different beam transport. Do you make any correlations between these?

Skorka:

No. But looking at the tube one comes to the opinion that most of the activity in the tube takes place between the small electrodes. The microdischarges which one would expect between the one inch apertures (because there the voltage is much higher) play a role during conditioning and maybe partially during a tube spark near the conditioning limit but I do not see any connections to these vacuum arcs between the small electrodes.

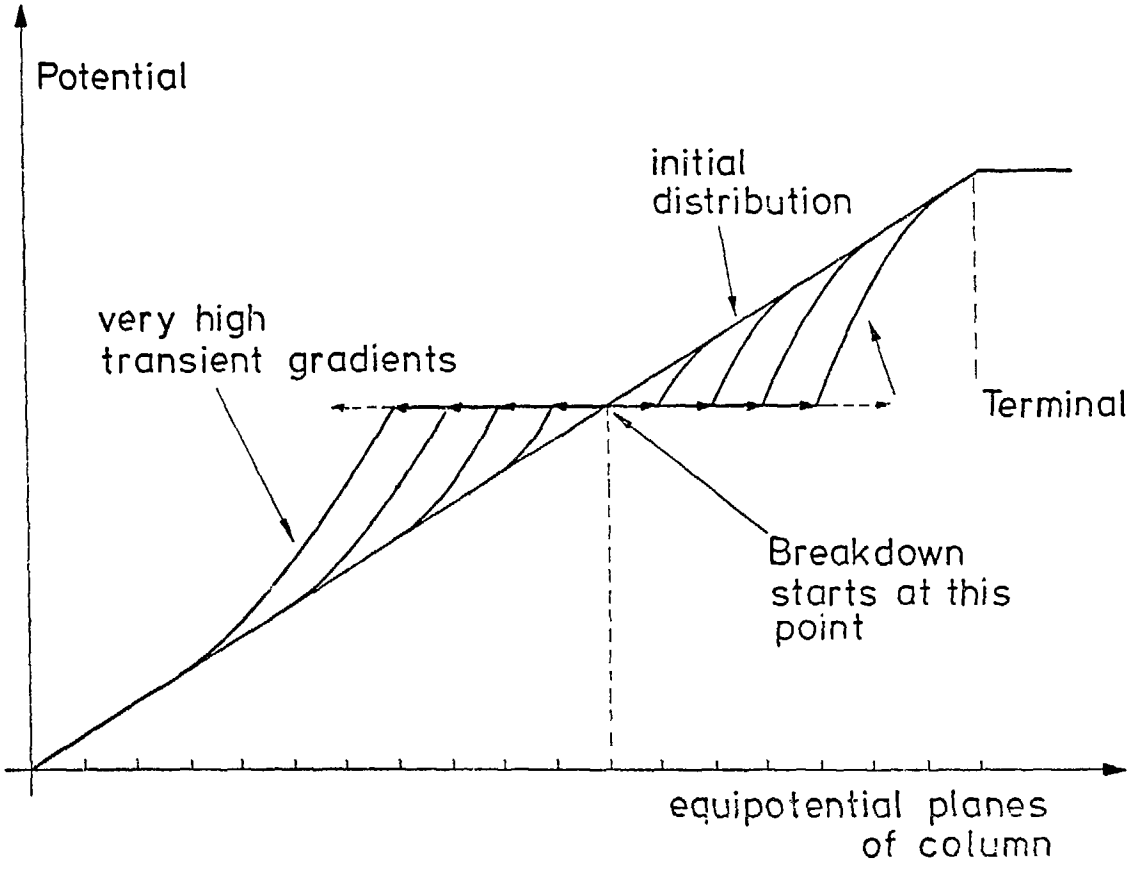

Fig. 1 Development of the potential distribution along the column during breakdown, schematically.

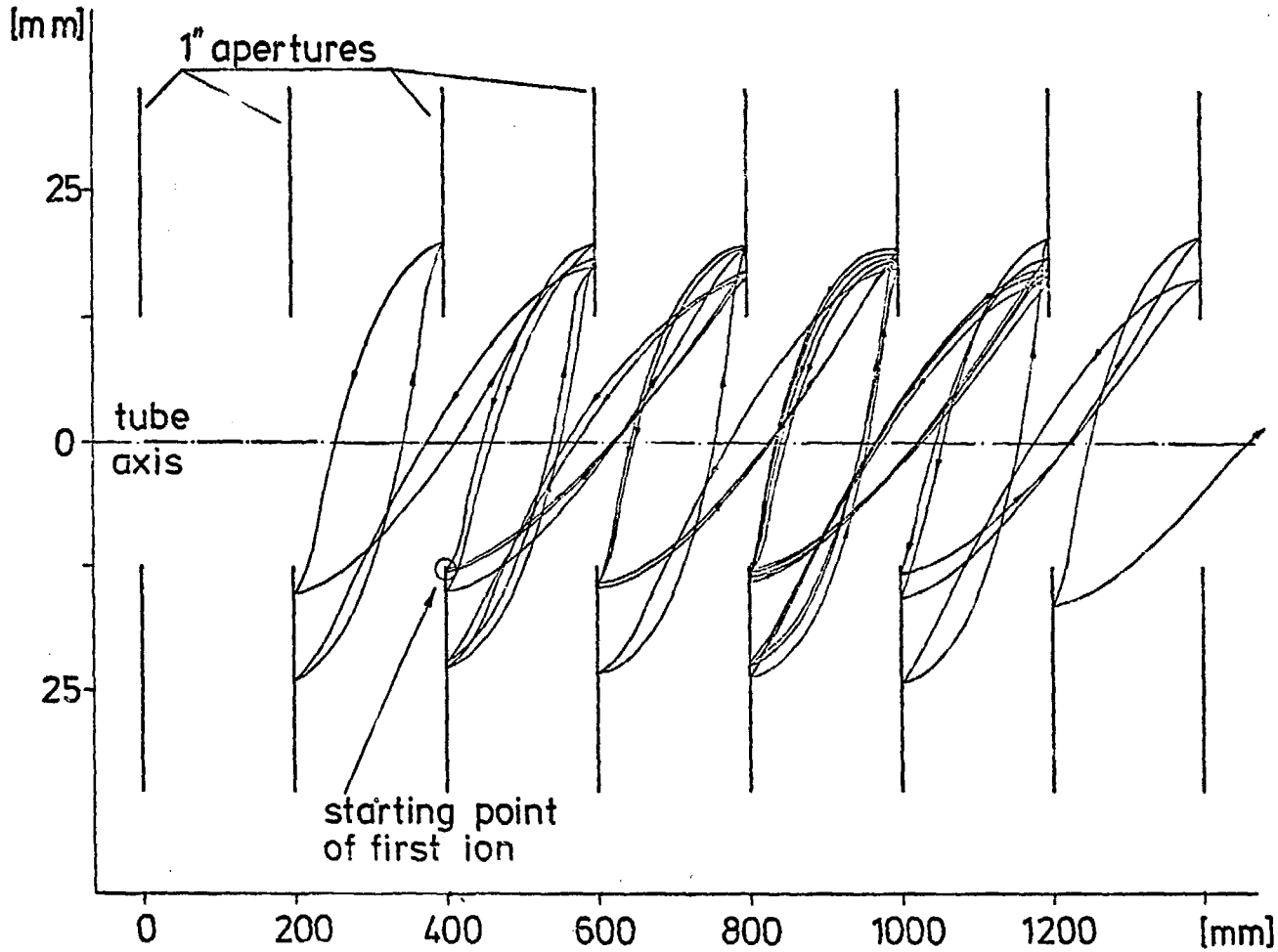

Fig. 2 Propagation of ion exchange discharge in a set of NEC-tubes. Points of start and arrival on apertures calculated by computer, trajectories sketched.

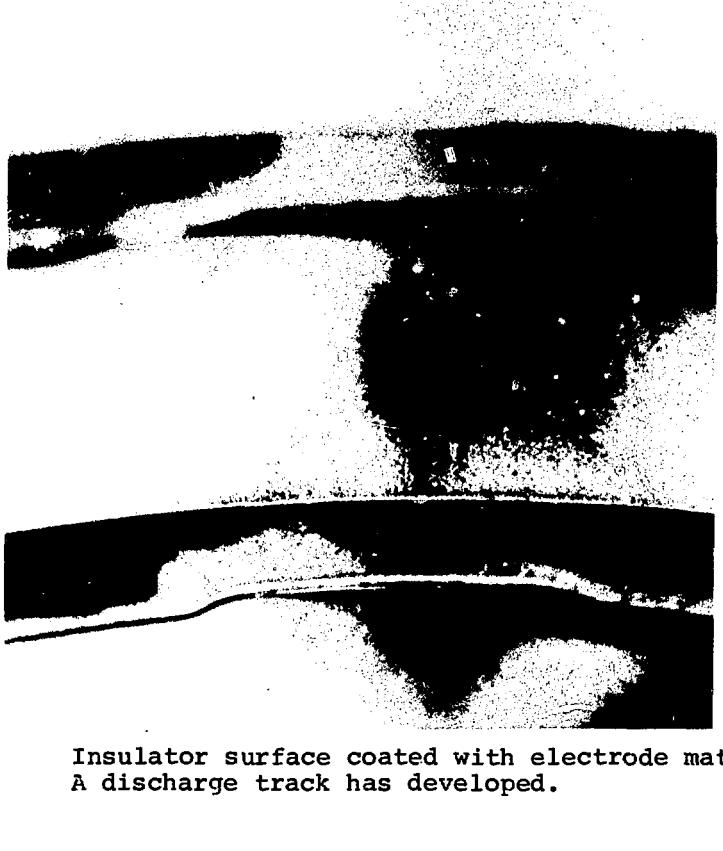

Fig. 3 Insulator surface coated with electrode material. A discharge track has developed.

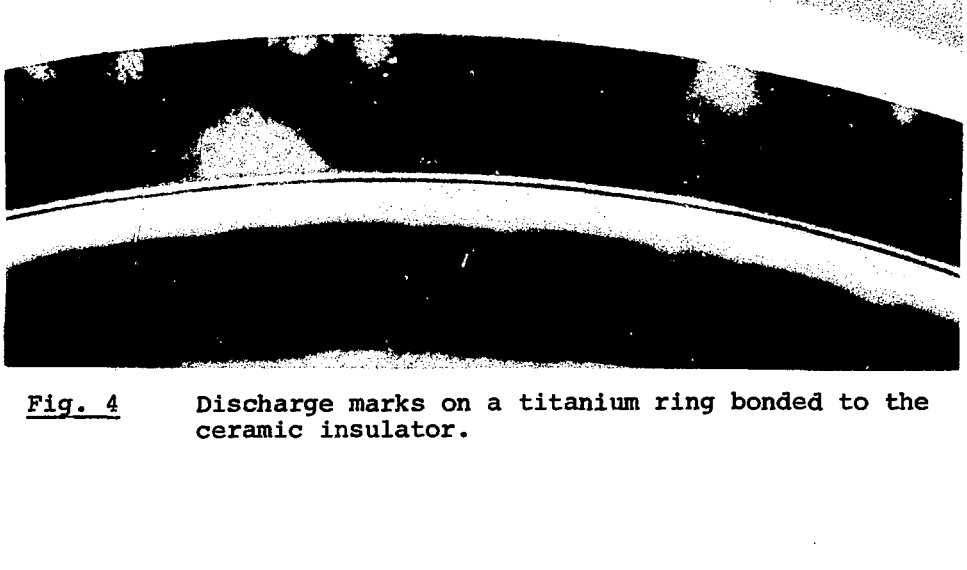

Fig. 4 Discharge marks on a titanium ring bonded to the ceramic insulator.

Fig. 5 Pair of inserted electrode rings removed from used tube.

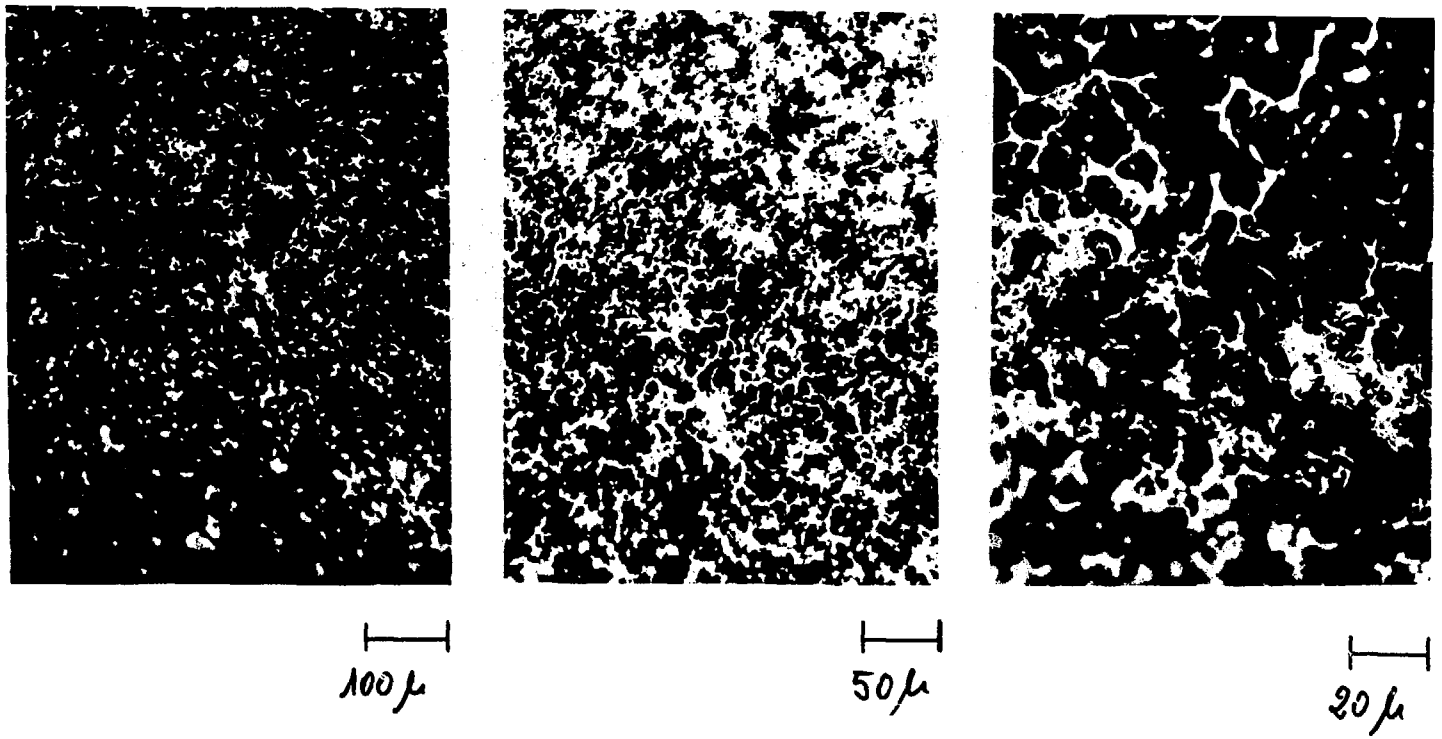

Fig. 6 Microphoto of part of a bright area on a bonded Titanium ring (like on fig. 4).

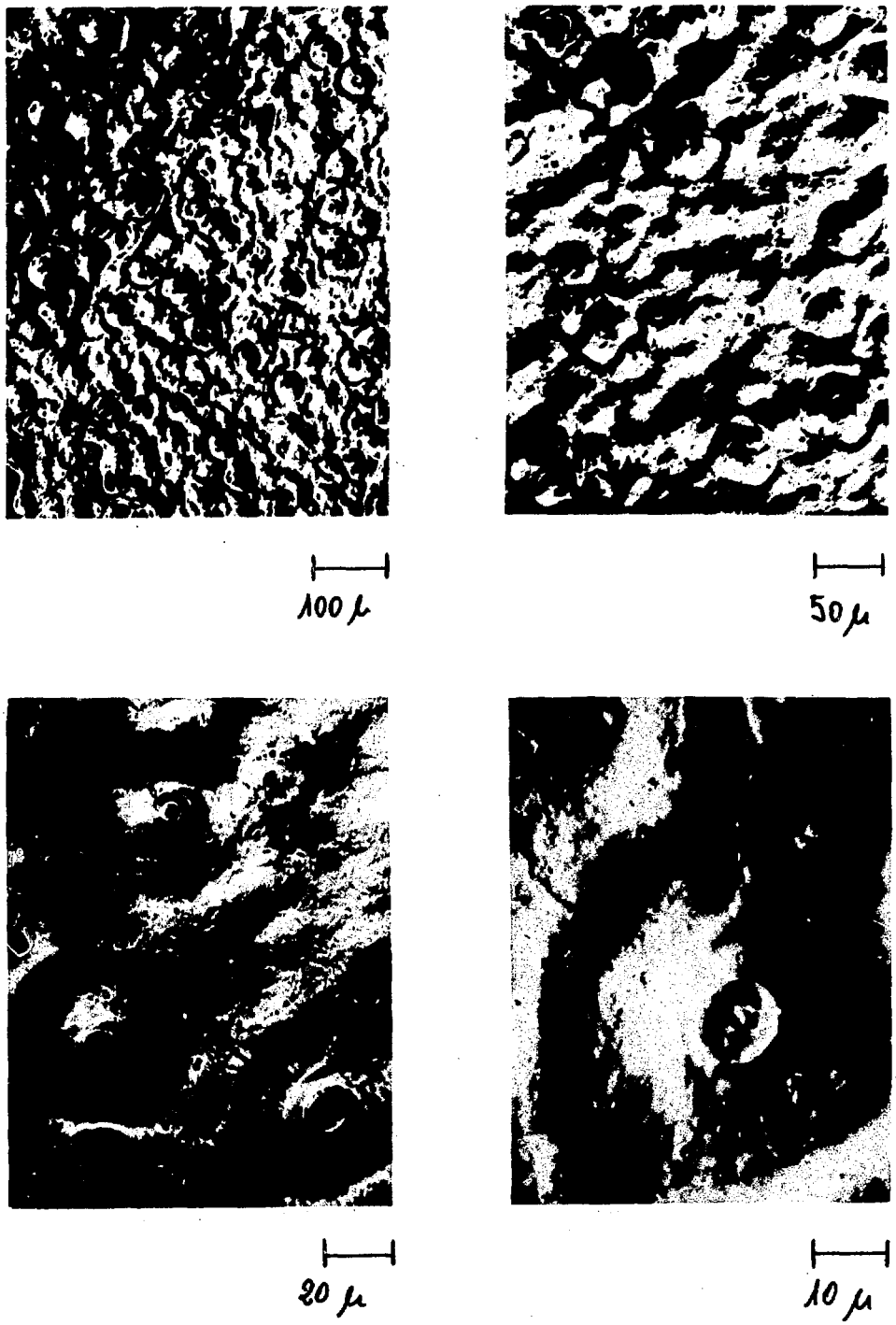

Fig. 7 Microphoto of a bright area on the anode side of an inserted electrode.

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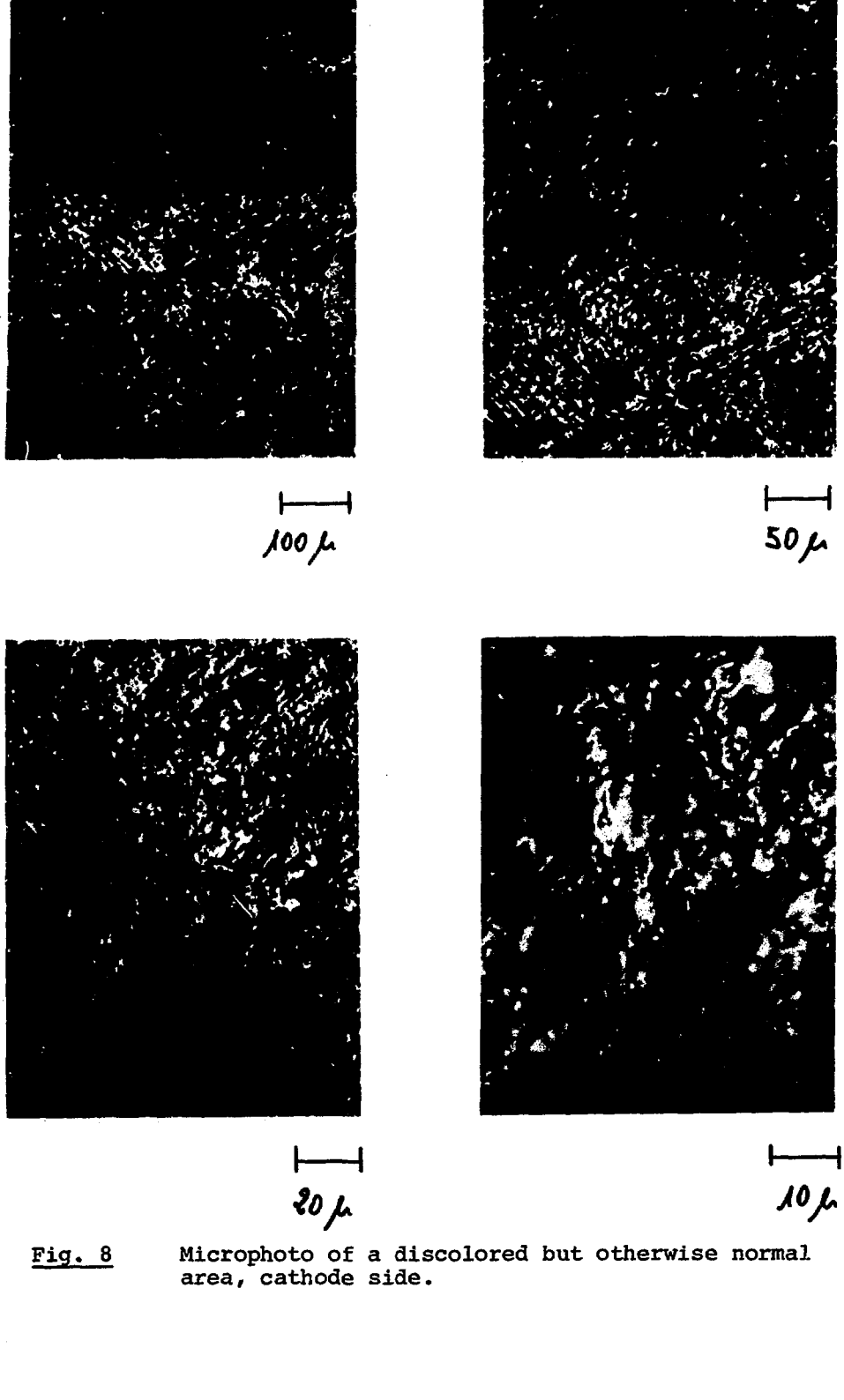


Fig. 8 Microphoto of a discolored but otherwise normal area, cathode side.

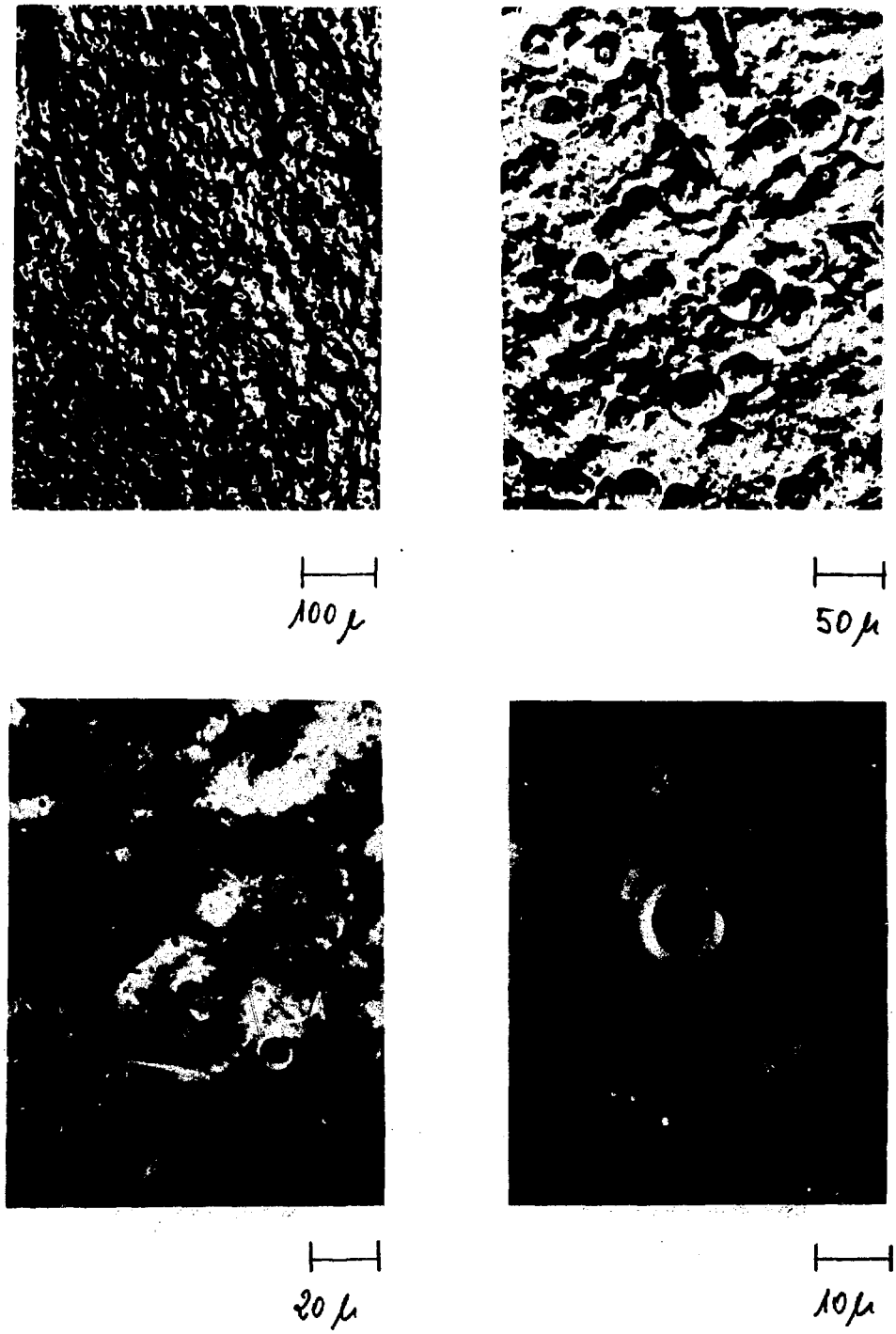
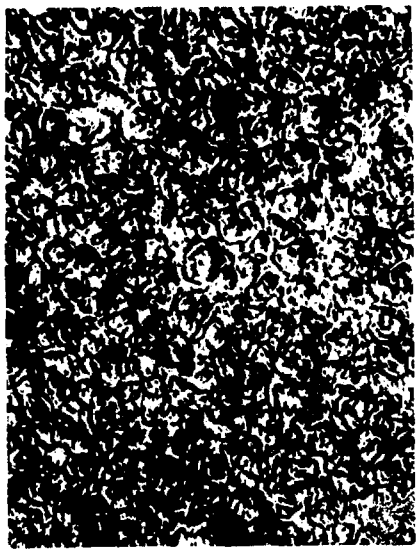


Fig. 9 Microphoto of a bright area on the cathode side of an inserted electrode.



140:1 ⊢⊣ 0.1 mm

260:1 ⊢⊣ 0.05 mm

650:1 ⊢⊣ 0.02 mm

1400:1 ⊢⊣ 0.01 mm

Fig. 10 Crater pattern on cathode side of an inserted electrode, bright area similar to fig's. 7 and 9.

CONDITIONING OF NEC ACCELERATOR TUBES

J. L. Yntema

Argonne National Laboratory
Argonne, Illinois 60439

The conditioning of NEC tubes was investigated by recording of the fluctuations observed with a residual gas analyzer for a fixed mass and a simultaneous recording of the current fluctuations in Ultek DI pump at the low energy base of the accelerator. In the conditioning process one can distinguish a part where the terminal voltage fluctuations are small and the occasional burst where the terminal voltage decreases by 50% or more.

For the small voltage fluctuations there is a good correlation between the DI pump current fluctuations and the RGA fluctuation for mass 2 shown in Fig. 1, masses 15 and 16 shown in Fig. 2, and mass 28 shown in Fig. 3. A good correlation was also observed for mass 14 as well as 27 and 30. It should be noted that the relative intensities of mass 14 and mass 28 make it clear that only a small fraction of the mass 28 partial pressure is due to N_2. At least 90% is presumably due to carbon compounds. In contrast with the other masses, mass 18 as shown in Fig. 4 shows no participation in the conditioning process.

However, in the case of large voltage fluctuations it appears that the major effect in the RGA spectrum is correlated with a large increase in the mass 18 partial pressure.

After such a large terminal voltage excursion the vacuum often takes a fairly long time to recover and this appears to be correlated with the H_2O pressure and pumping speed. Our experience indicates that the voltage holding capability of the NEC tubes is adversily affected by water vapor and therefore we intend to avoid venting of the accelerator tubes as much as possible.

Discussion:

Clegg:

Now after running 3,000 hours, what's your routine good voltage operation? Can you run above 9 on the terminal very easily, or you said up to 9.6 where you condition it, how can you do that? I would like a little information about your experience.

Yntema:

Well, first at 9.6 we have all these mechanical things happen to us, we have not gone back there since that time. We have run the machine at 9, there is a fair deal of sparking at 9, we don't particularly like to run there. For our particular application, what we need is extremely stable operation at about 8 or 8.5 MV. We have achieved that to the point that we have run for 5 or 6 days without any tube sparks and this for our operation is what is needed.

Clegg:

We've been able to run with our standard FN, routinely at 8 for days without any tank sparks and run at 8.5 with several tanks sparks a day with the old belt system. I guess I wonder whether for someone like us who might be considering whether we should upgrade, whether its really worth the effort. I just don't know.

Yntema:

I am not sure either but maybe you'll have a better idea after I describe what we did and why we did it on Wednesday morning and we operated with standard FN tubes at 9.3 regularly until we made this modification. That has really nothing to do with the terminal voltage, nothing to do with our modification; in fact, we are willing to sacrifice terminal voltage, that's pretty irrelevant to the Argonne system, as long as we get to about 8.5, we are satisfied.

Wegner:

What pressure insulating gas do you use? And do you use pure SF_6?

Yntema:

Just relatively pure SF_6. It was pure when it was put in with about 8 percent oxygen-nitrogen and it depends, in our system you have to be a little bit careful. If we want to run at high terminal voltage, we have to run about 95 lbs. If we want really a lot of up charge, we have to run about 115 lbs. We normally run about 90 lbs.

Ball:

If you're running and get a good healthy tube spark, how long before you're operating again?

Yntema:

What ever time it takes for the voltage to get back up. We have an automatic switch which cuts off the charging system and we go down. So, we wait until the pressure drops down to a reasonable number, of say, like $7\text{-}8 \times 10^{-8}$ both at the high energy end and low energy end and say maybe half a minute or so, then we flip a switch, and we're back on.

Berners:

Could I take a minute to describe some recent experience with our FN tandem at this point? Just from here. We have a FN tandem that's ten years old, has its original aluminum tubes still in the machine. We use nitrogen and CO_2 tank gas and we consider it to be approximately an 8½ million volt accelerator when we're running it as delivered from the factory with its charge exchange ion source or running with a polarized ion source. That means we can usually count on running at that voltage. Sometimes we can run it at 9 million volts but we know that there will be many days when 9 million volt operation is just not possible because we have very frequent sparks, like more than one spark per hour. Also, sometimes we find that 7½ million volts is difficult. About a year ago we begin injecting a heavy ion beam from a single ended Van de Graaff machine at energies typically between 2 and 2½ MeV. We've run lithium, boron, carbon, oxygen, etc. and we have found more and more as we run more and more with this megavolt injector that the ability of the tandem to run at much higher voltages is much, I should say that the tandem is able to run stably at much higher voltages with this megavolt beam than it can run when we're injecting 40-50 or 80 kV ions from the charge exchange ion source. We have done experiments at 9.4 million volts with the beam from the injector. Last week the machine was running at 9.6 million volts. This was with almost no sparks at all, less than one spark per day. The characteristics of the beam from the injector, of course, are much better than the beam from the charge exchange ion source. The emittance, the absolute emittance is much lower. The beam is convergent as it enters the acceleration tubes of

the FN having come from an electrostatic quadrupole some distance before the tank. We tried to imagine what we could do to make it better. We think of, somehow, squeezing the beam down to zero emittance, having just a single ray of charge going through the accelerating tube. It would seem that that would be the ideal condition. But, if you think of going one step further and squeezing the beam down to nothing at all, in other words, having no beam, we do that by putting in the low energy cup. And then, the behavior of the machine is much worse than under any other condition. It is more difficult to achieve stable high voltage operation with no beam at all than with this high quality, say 2½ MeV beam coming from the injector. I wish that I could begin to understand what is going on in the machine in order to make it run that way, but while we're thinking about it, we're just enjoying it.

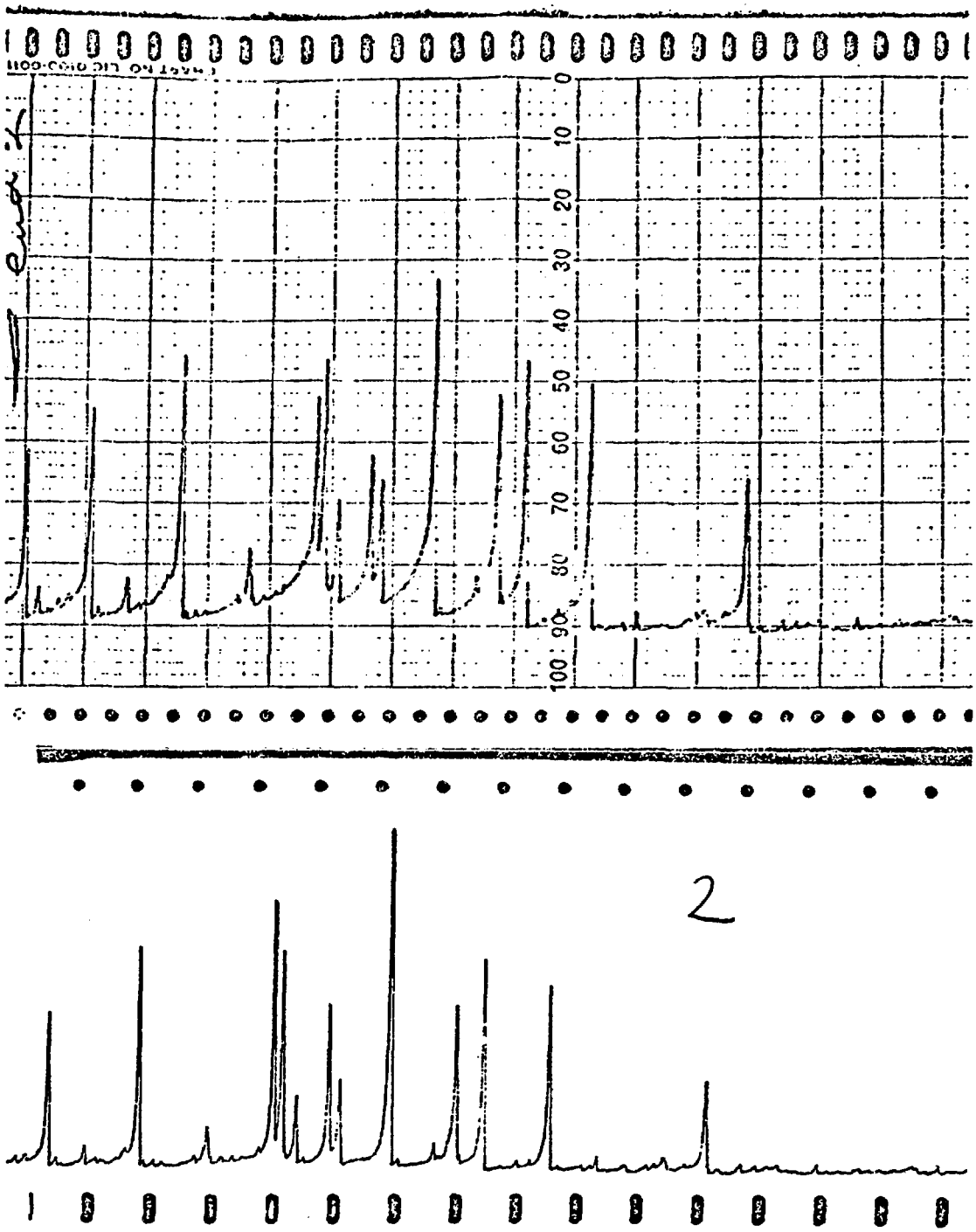


Figure 1

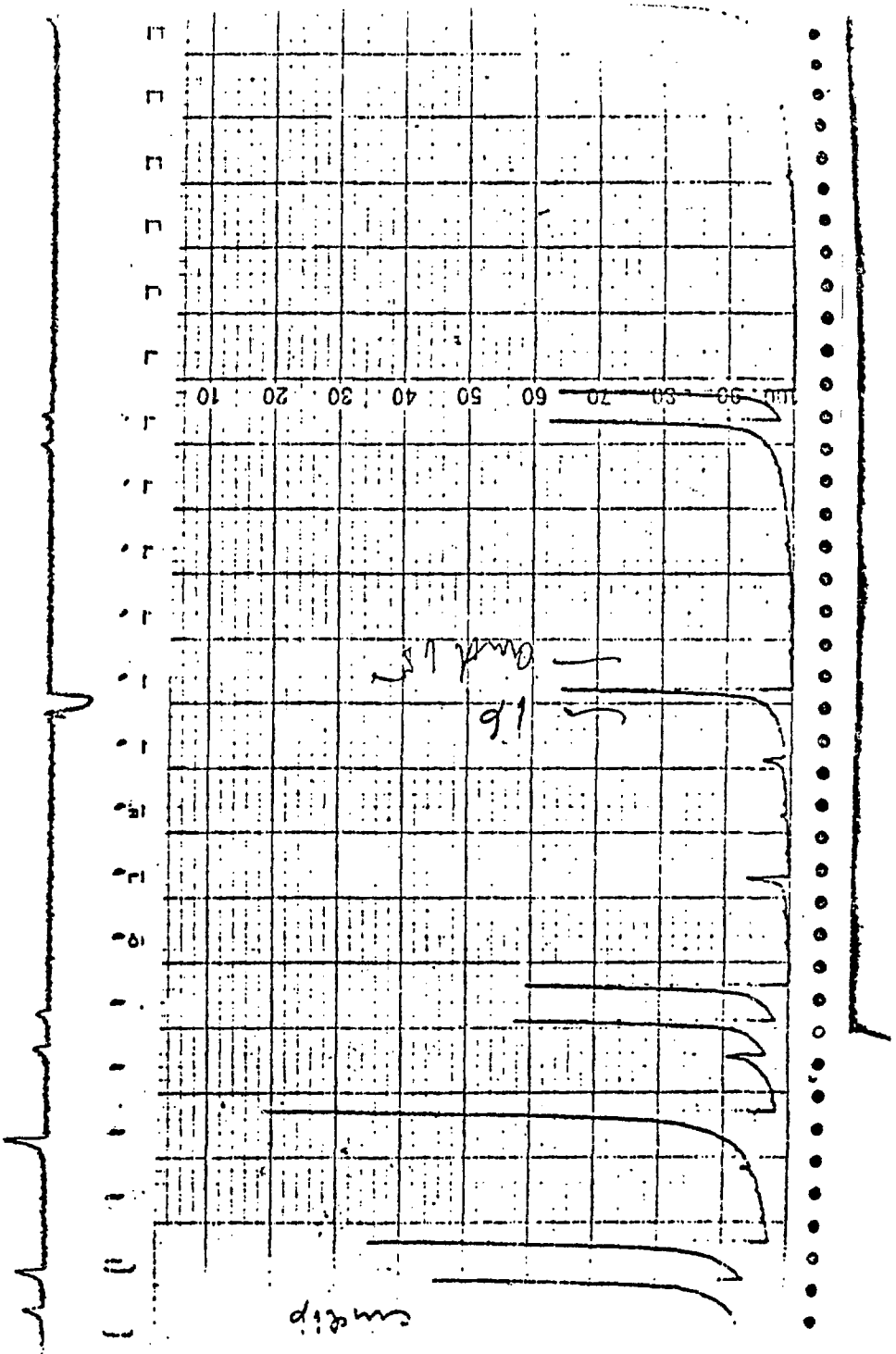


Figure 2

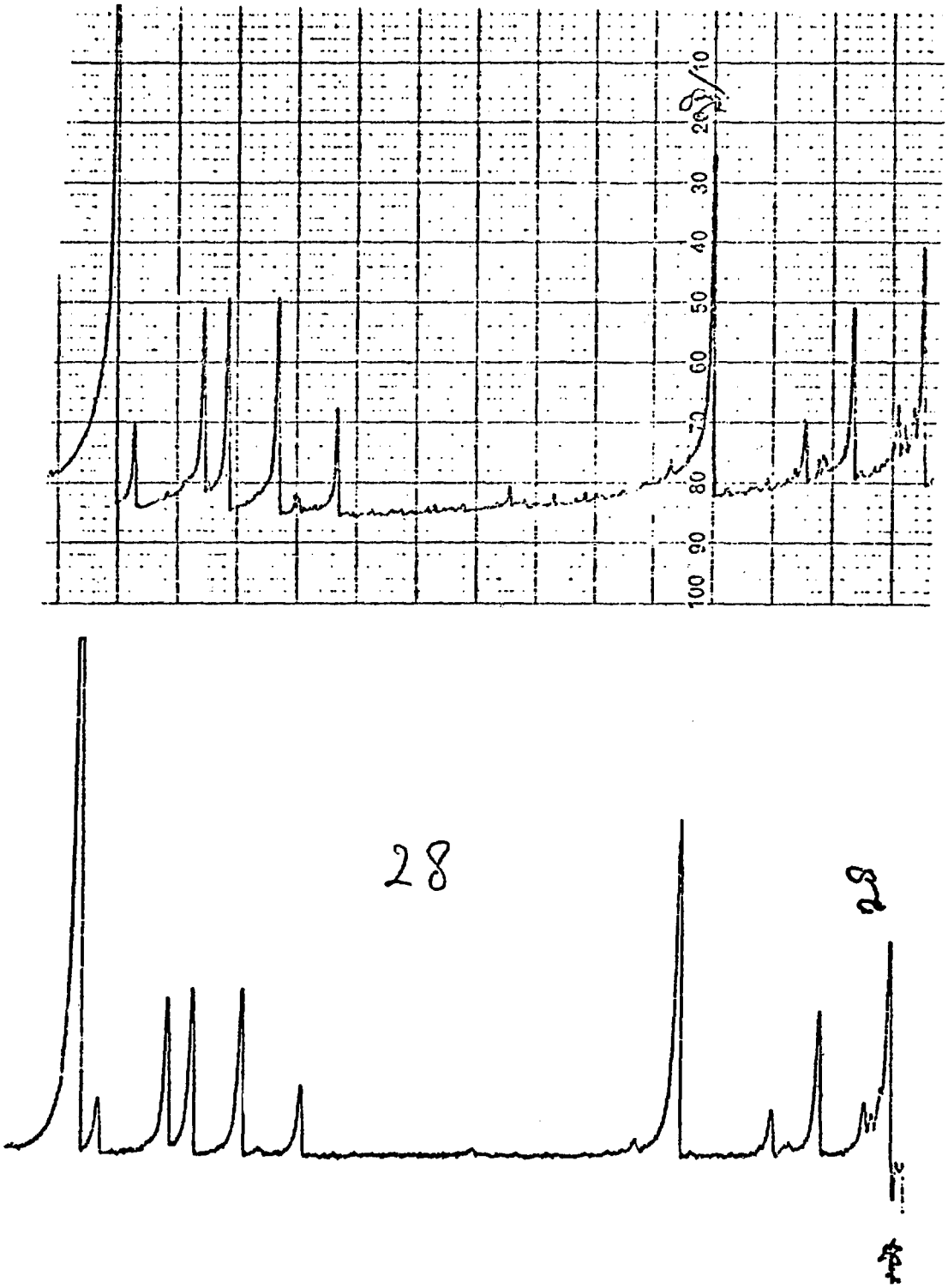


Figure 3

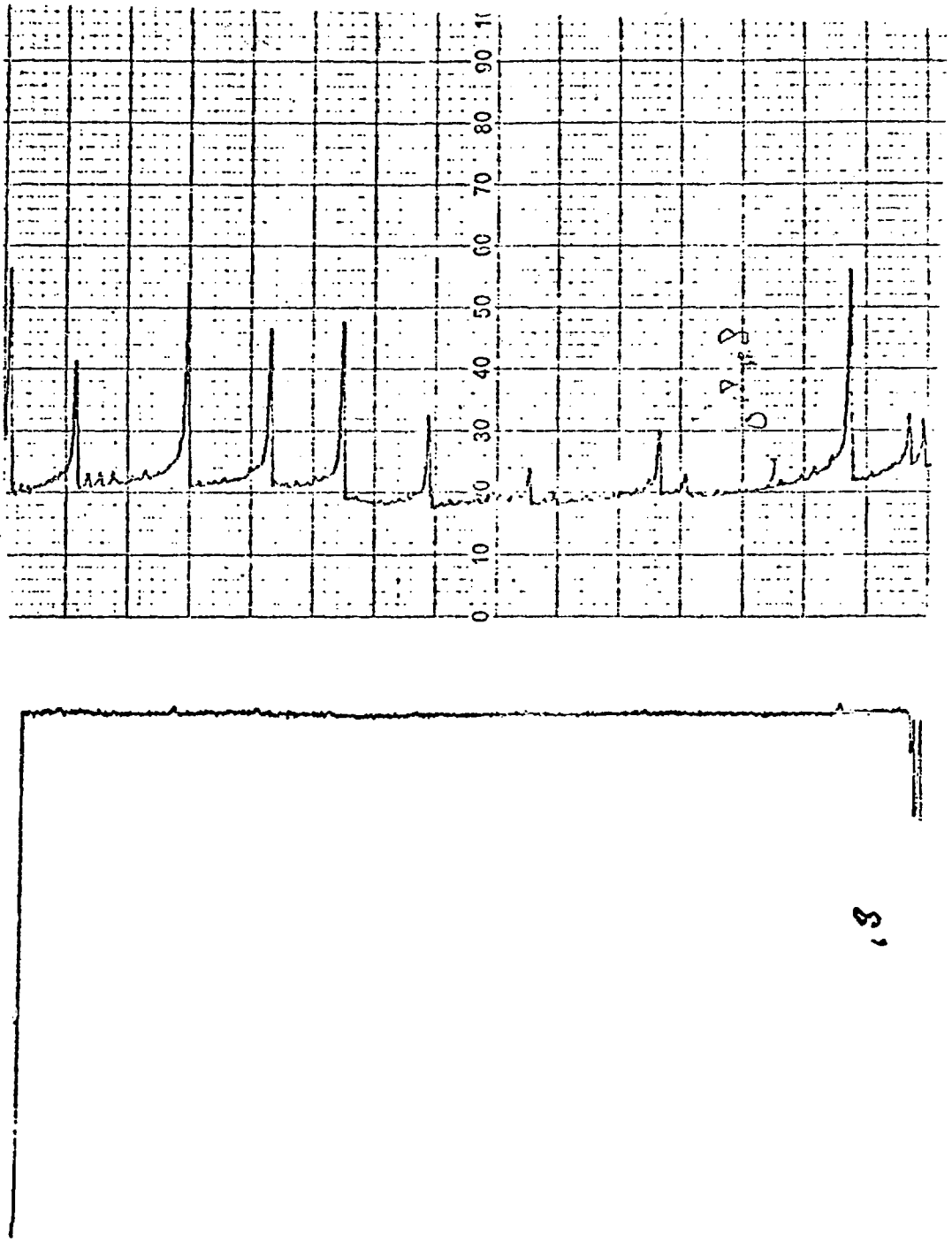


Figure 4

Session II, Chairman, E. D. Berners
Editors, J. K. Bair and J. E. Mann

AN EXAMINATION OF THE BREAKDOWN PROPERTIES OF PURE AND MIXED ACCELERATOR TANK GASES

John H. Broadhurst
University of Minnesota

In the 1930s the first van de Graaff accelerators were constructed using pressure vessels to improve the maximum voltage capability of their terminals. Initially air under pressure was used, due to its availability, the first discovery of insulating gases of better performance made by Professor R. Herb of the University of Wisconsin. He found that adding carbon tetrachloride vapor to the tank air increased the terminal potential that could be sustained by his van de Graaff. A second experiment using acetone vapor was more spectacular, if of somewhat short duration, but more seriously pointed to the necessity of the insulating gas being inert and nontoxic as well as having good insulating properties.

Very soon after this period Dr. van de Graaff and others adopted a mixture of nitrogen and carbon dioxide as an insulating gas and this material remained the standard mixture until about 10 years ago, when the properties of sulphur-hexafluoride, a material which had been investigated by manufacturers of high power circuit breakers, were recognized. This material promised to improve the performance in existing accelerators, and enable future machines to be constructed with cheaper pressure vessels.

Since the adoption of sulphur hexafluoride as an insulating gas, several problems have remained unsolved, for example are its insulating properties best when used alone or in combination with other gases; why do installations find performance changes with different deliveries of insulating gas, why do the insulating properties change with use; and other allied questions.

It is obvious that full scale testing of gas mixtures and preparation conditions, would not only be prohibitively expensive, but would be very time consuming. On the other hand the problem of extrapolating scale model tests has proved difficult. In fact scale model tests usually indicate that insulating gases perform orders of magnitude better than can be realized in full scale applications.

The work reported here represents an attempt to provide a basis for extrapolation of scale tests by introducing the concept of the probability of a spark occurring per unit time under a given set of conditions. If the sparking probability can be determined as a function of electric field strength to which the insulating gas is subjected, (or terminal potential of the scale model) then extrapolation can reasonably be made to the probability of a spark occurring in a full size installation.

To make measurements of this type the equipment of figure 1 was assembled. An annular spark gap inner diameter d outer diameter 3d was constructued, of scale approximately 1:1000 of the M.P. Tandem, the outer electrode having a Ragousky profile (suitably modified because of the cylindrical geometry) on its inner surface in order to prevent the enhancement of potential gradient that would otherwise occur at its ends. The correctness of this profile was tested initially by generating 1000 sparks in the gap and visually

verifying that their spatial distribution was random. The equipment operated as follows; the programmable power supply provided a steadily increasing potential difference to the gap until a spark occurred. At this time the current flow due to the spark set the spark detecting bistable, which then applied a crowbar to the programmable supply both to prevent repetition of the spark, and to minimise the energy dissiapted in the spark gap. After a fixed time interval the program generator released the crowbar by resetting the spark detector, and started the linear rise of the potential difference of the gap again. The bias supply both provided a clearing field and enabled different gas mixtures to be examined without exceeding the range of the programmable supply. The cycle time chosen was 100 seconds with a rate of rise of potential of 1 Kv/second. These parameters were selected by shortening the cycle time until the data first indicated that recovery from the previous spark was incomplete, and then adopting a cycle time 10 times longer; an arbitrary but simple approach. The computer continuously digitized the potential of the programmable supply, and examined the state of the spark detector. When it found a spark had been detected it retained the previous potential and stored it as a histogram of the number of sparks which occurred in each kilovolt interval. As the loop time of the digitization cycle was the order of a millisecond the uncertainty of the potential as recorded was about 1 volt, and hence negligible.

Attention was given to the possibility of "conditioning" effects as the gap was repeatedly sparked. After initial assembly the gap was allowed to spark 1000 times before any data was recorded, after which the gap was not disturbed in any manner. After each change in gas type or pressure at least 100 sparks were permitted to occur before data was recorded. The data set shown in figure 3 in which is plotted the average potential of sets of 20 sequential sparks demonstrate that over a typical run the amount of conditioning observed was negligible.

Figure 2 shows a plot of a typical set of data for 10^3 sparks, the horizontal scale showing the gap potential difference, while the vertical scale shows the logarithim of the probability of a spark occurring in any one second that the gap was within ± 500 volts of that potential. It will be noted that the data in this example was adequately fitted by a straight line over a spark probability range of seven decaves. This indicates that the spark probability - terminal potential relation can be expressed as an equation of the form

$$\log p = k_1 V + k_2$$

where k_1 and K_1 are constants, p is the spark probability per unit time, and V is the potential difference applied to the gap. As this relation was found to hold (with different constants k_1, k_2) over the initial sets of data taken at different pressures, the work was expanded to the following examinations. The following pure materials were investigated as a function of pressure, sulphur hexafluoride, dichlorodifluoro methane, (Freon 12) and monochlorodifluoro methane (Freon 22). In addition two tank gas mixtures, Minnesota tank gas, an economy nitrogen carbon dixiode mixture with the addition of 20% sulphur hexafluoride, and Yale tank gas, to a constitution kindly provided by Mr. K. Sato, were investigated at a pressure of 75 p.s.i.g. The effects of the contaminant carbon tetrafluoride commonly present in sulphur hexafluoride was investigated, over the contaminant range 0-2%.

Figures 4 and 5 show the results of these measurements, each data point being defined by a set of 1000 sparks. From these spark data the constants k_1 and k_2 were determined and the potential at which the probability of one spark occurring per 1000 seconds is calculated. Note Figure 3 indicates that Freon 22 would be an insulating medium superior to sulphur hexafluoride under these conditions.

If however an extrapolation is made to the dimensions of a full size accelerator, this prediction is no longer valid. In order to perform the extrapolation it was heuristically assumed that each spark was originated by some event occurring on the surfaces of the spark gap. In this case the probability of such an event occurring within a given time would be proportional to the area of the gap electrodes. Hence in scaling this model gap to represent the dimensions of the M.P. Tandem the sparking probability was assumed to have increased by 10^6 (1:1000 scale model). The results of this extrapolation are presented in Figures 6, 7 and 8 which are now plotted so as to indicate the mean time interval between sparks for an MP Tandem operated at terminal potentials of 12 and 15 M.V. (the currently difficult area for these machines). The double point on Fig. 4 indicates the reproducibility of different runs. Note that now the Freon 22 (Fig. 7) is now markedly inferior to sulphur hexafluoride and in fact not useable as an insulating gas in this application. The sulphur hexafluoride data shows characteristics similar to the results obtained by actual accelerators currently in operation. Very interesting also is the performance of the two mixed gases. (Minn. and Yale tank gas) which indicate an interval between sparks much longer than would be obtainable by using pure sulphur hexafluoride at a pressure equal to its partial pressure in these mixtures.

It is intended to extend these studies both to gases at higher pressures and to gas mixtures, some of which appear on first examination to have properties superior to sulphur hexafluoride.

Discussion:

Wegner: Is there any special reason why you didn't investigate much higher pressures of SF_6, like maybe 15 - 18 atmospheres or something like that, or is it the limit of your test vessel?

Broadhurst: Yes, the test vessel is designed for 75 psi and at 90 psi I got nervous. I intend now to try to repeat up to 180, with a new vessel.

Jones: You showed us two data sets with CF_4, and the latter was plotted in terms of an extrapolation to the larger machine, there are two data sets? Is that true?

Broadhurst: Yes, one was the probability of one spark per 1,000 seconds on the scale model, the other was the effect on an MP sized machine by extrapolation.

McKay: It seems to me, looking at some figures from a long time ago, that if you do increase the pressure of SF_6 in a machine it should curve over at about 120 pounds. Now I can't recall where I saw that, but I remember that is one of the pieces of mythology of which I remember quite a bit. The other question which I had, in the curves of machine performances the best mixture was still just slightly above that of pure sulfur-hex. Is that correct?

Broadhurst: Yes. But the best mixture was tested at the same pressure which was a much smaller partial pressure of SF_6.

McKay: But the advantage would not be in performance but only in cost.

Broadhurst: Yes. But I'm inhibited as yet in taking high pressure measurements by the lack of a suitable pressure vessel.

Burn: I'd just like to comment, John, that at Chalk River we have been using pure SF_6 for a number of years and the calibration constant we use is approximately 6 psig per megavolt and that means that before we upgraded the machine we ran at 10 megavolts and used approximately 45 psig. At the moment we use 75 psig which during our upgrading tests was shown to be good to about 15 megavolts.

Broadhurst: Yes, I believe it falls very well on the curve, actually.

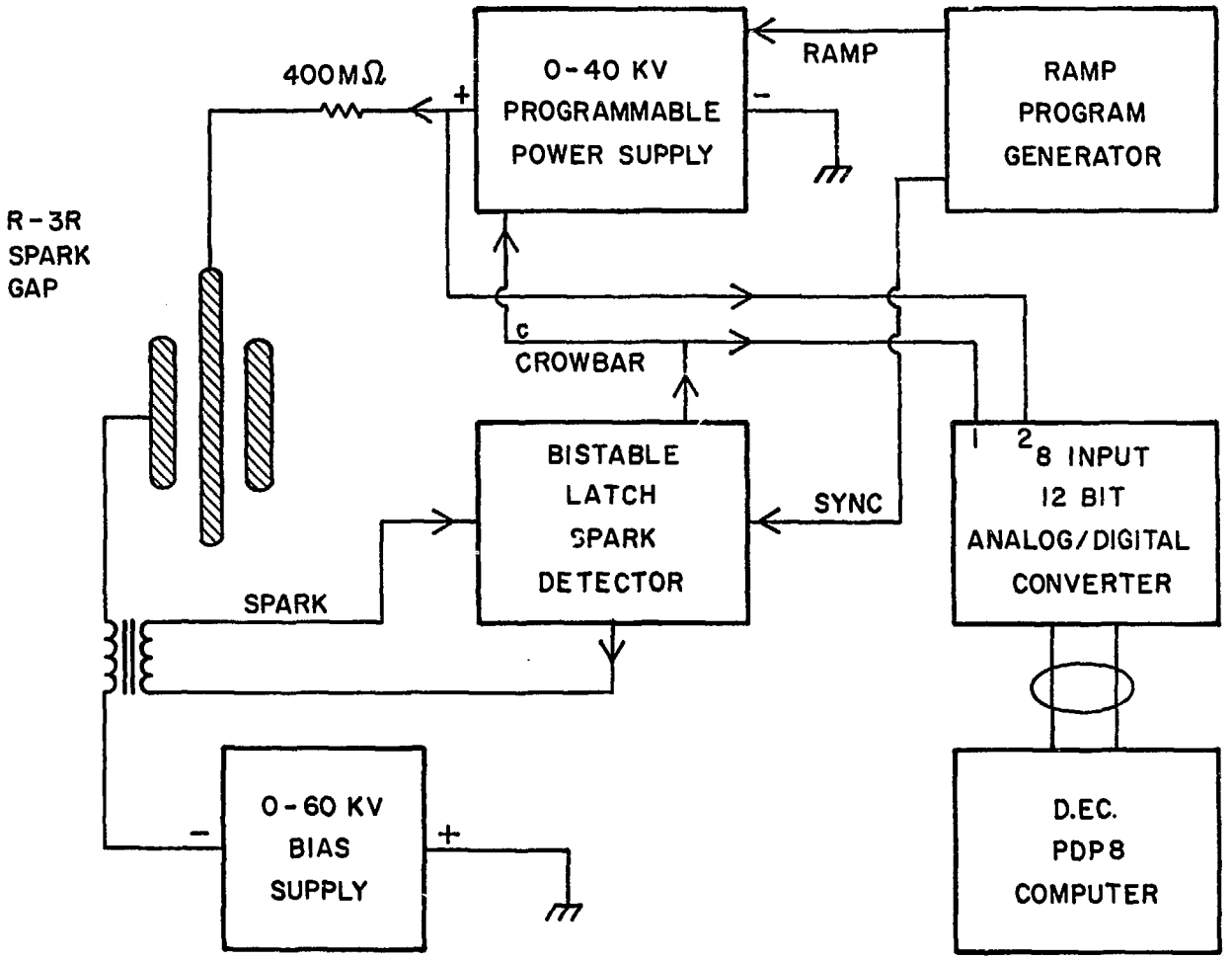


FIG. 1. CIRCUITRY

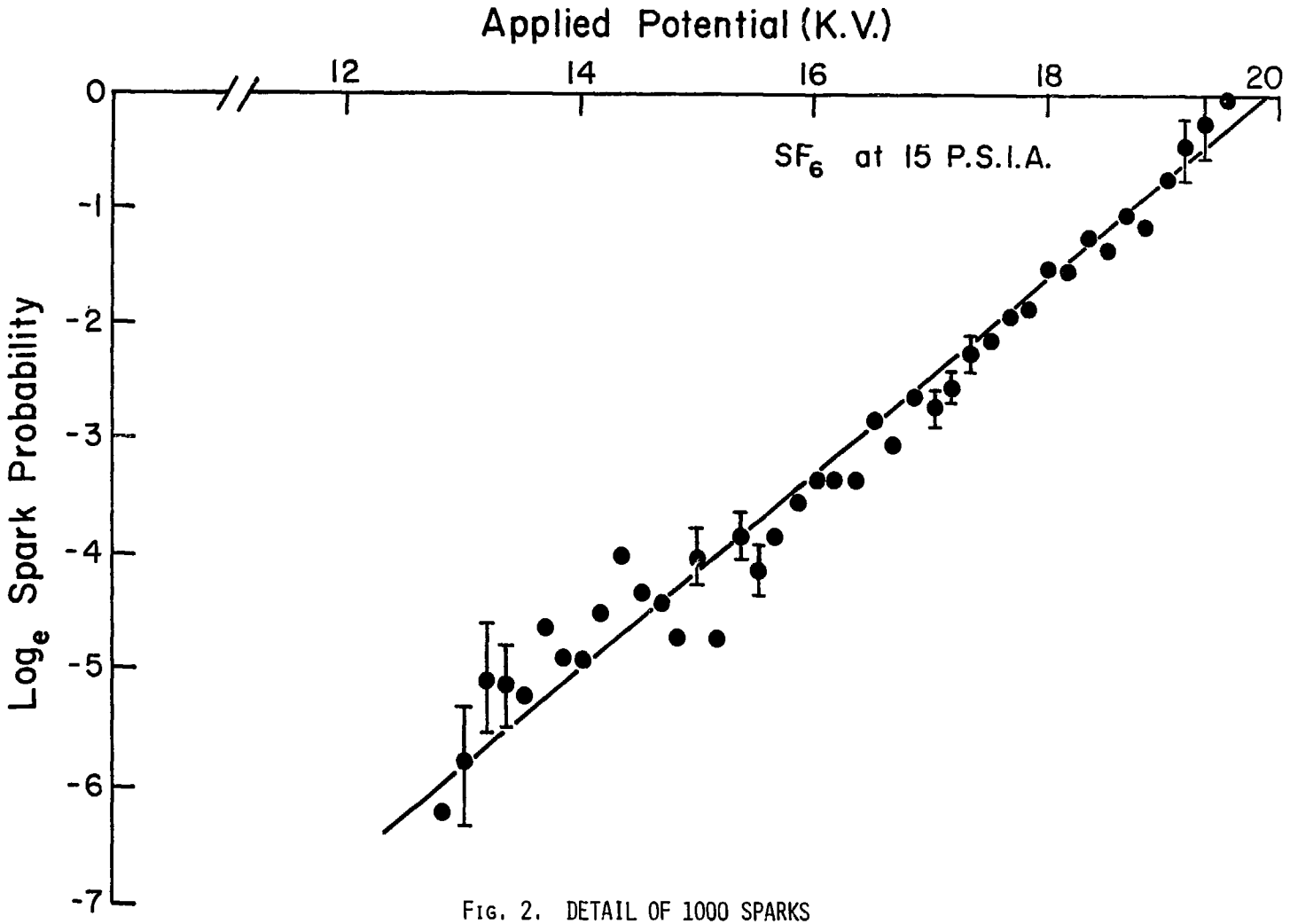
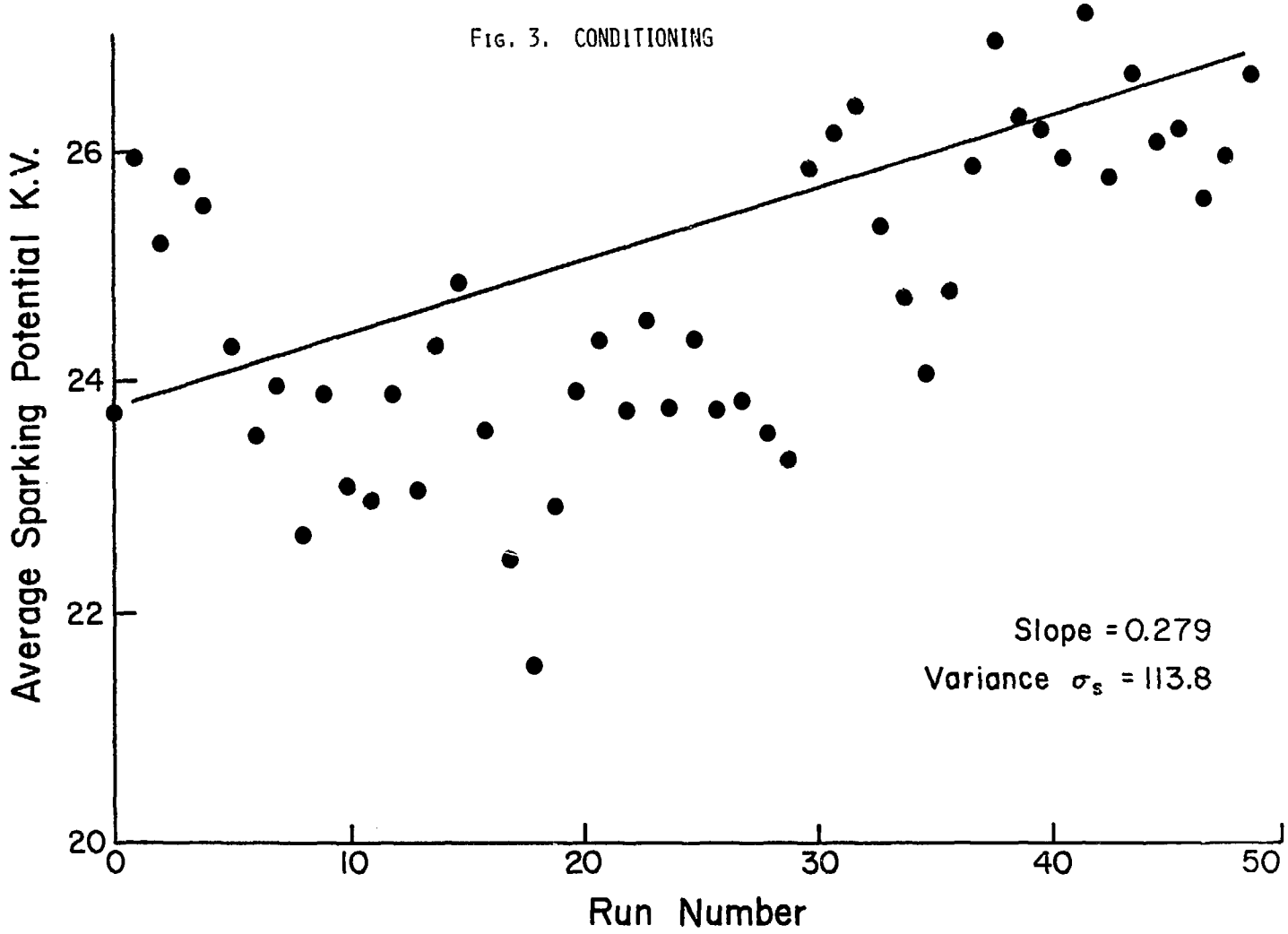


FIG. 2. DETAIL OF 1000 SPARKS

FIG. 3. CONDITIONING



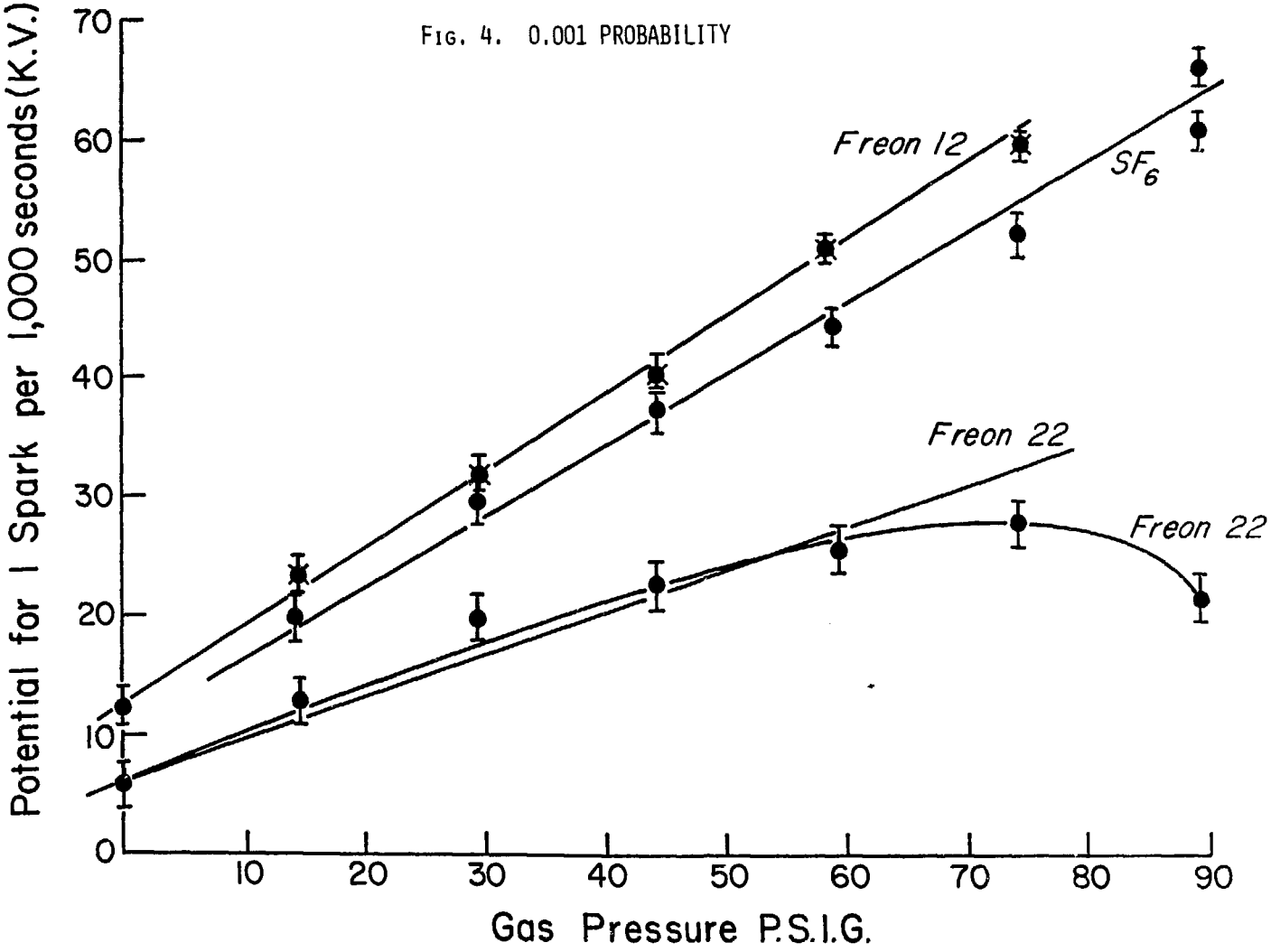
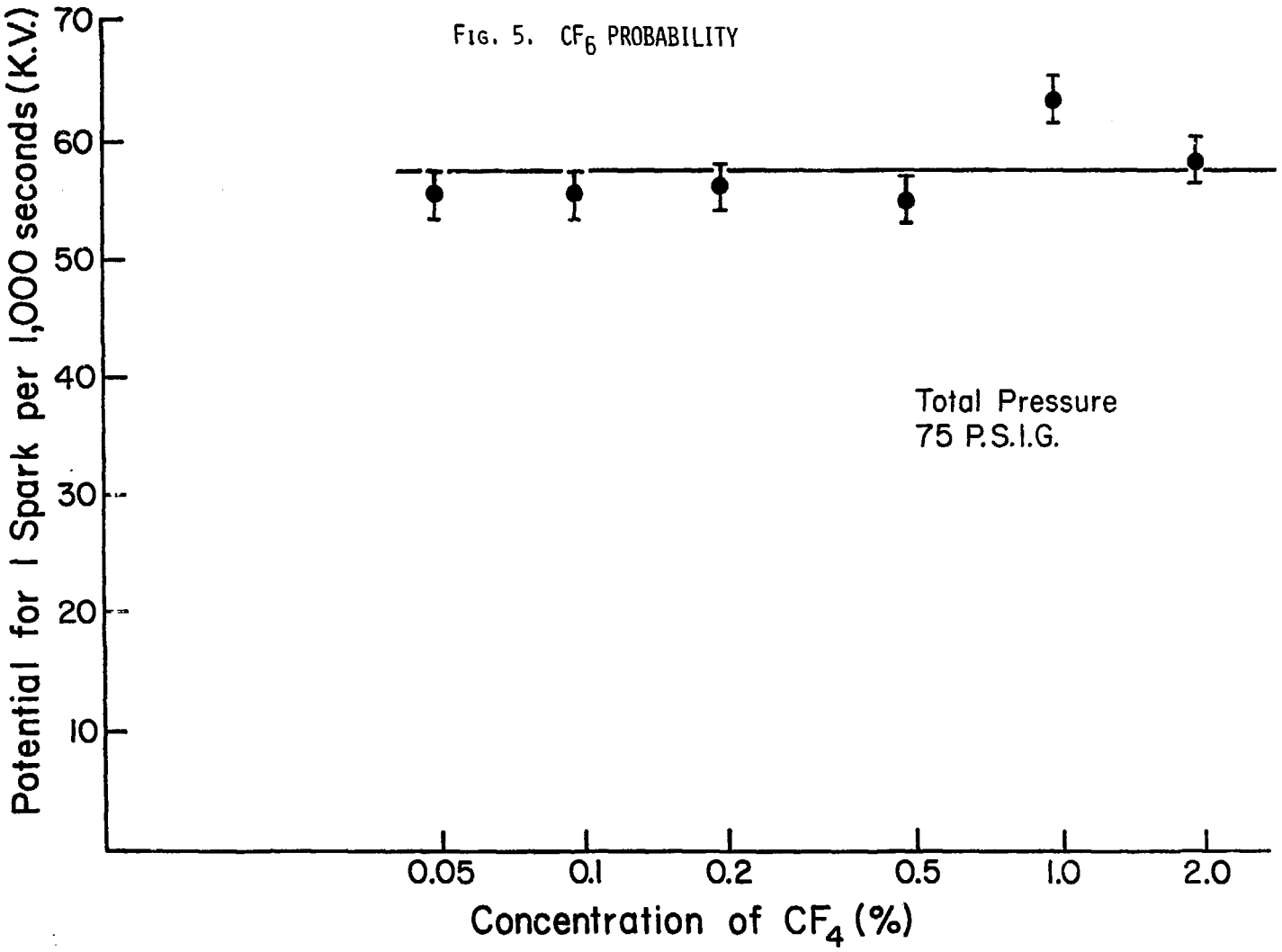


FIG. 4. 0.001 PROBABILITY

FIG. 5. CF_6 PROBABILITY



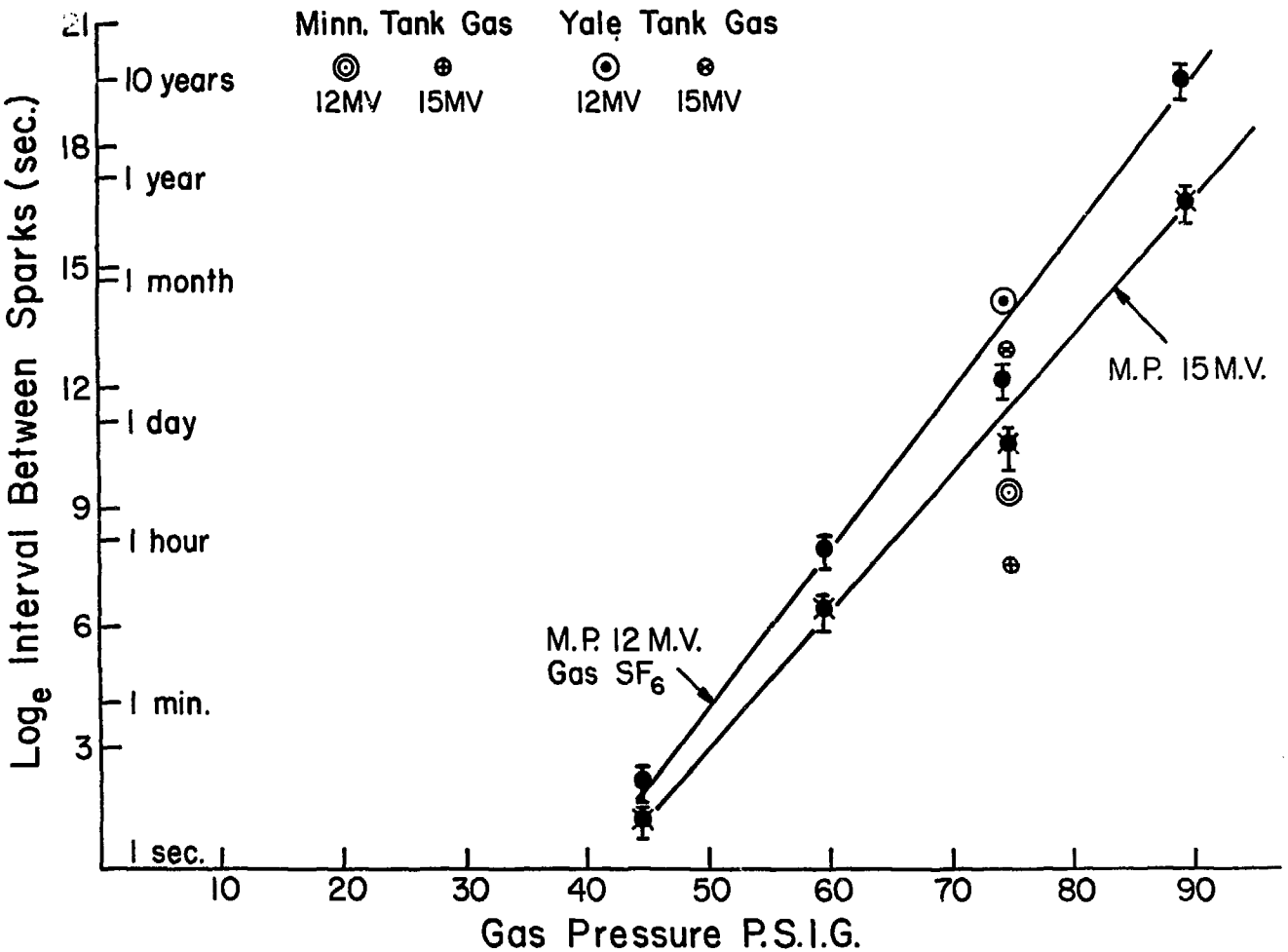


FIG. 6. FULL SCALE SF_6

FIG. 7. FREON 22, FULL SCALE

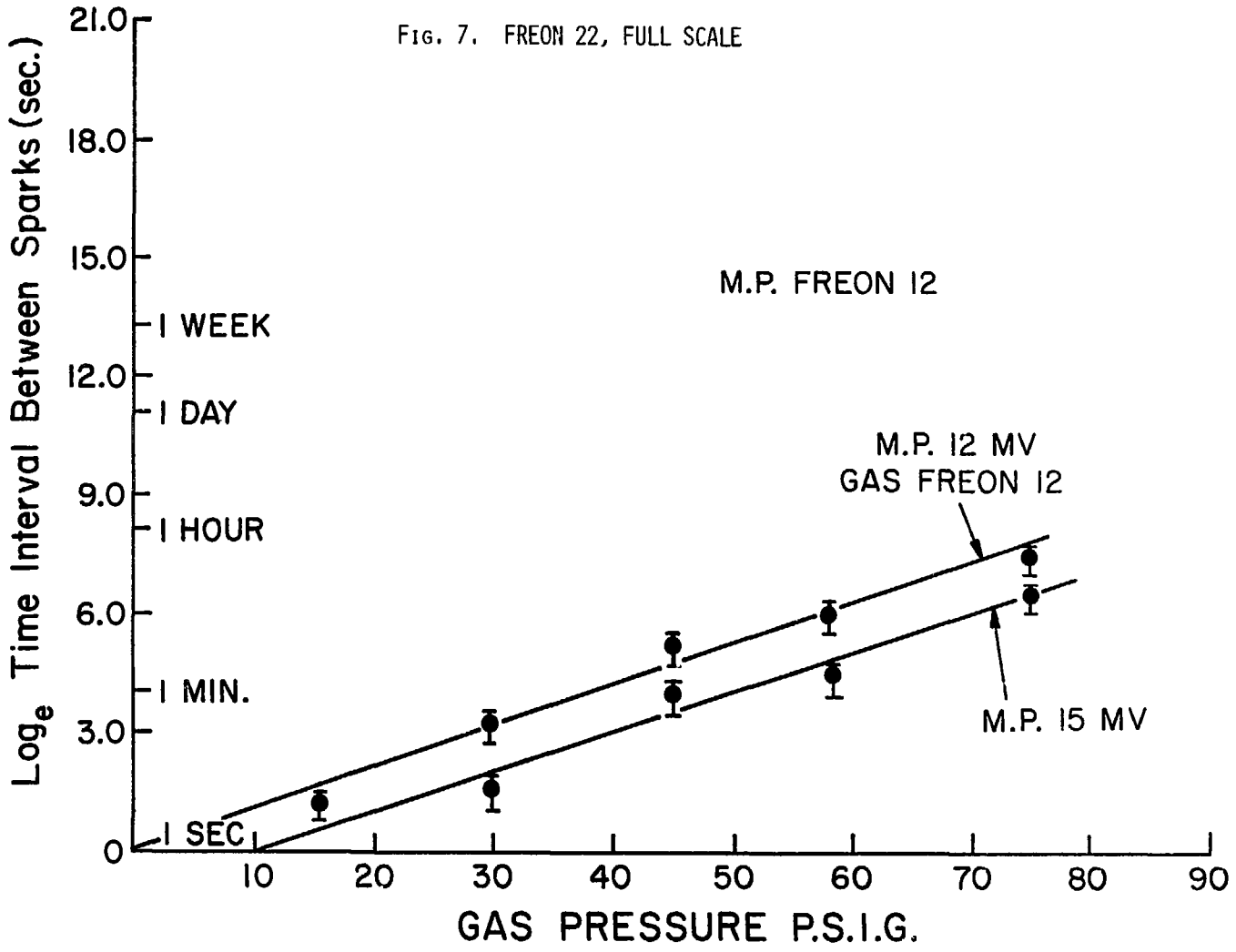
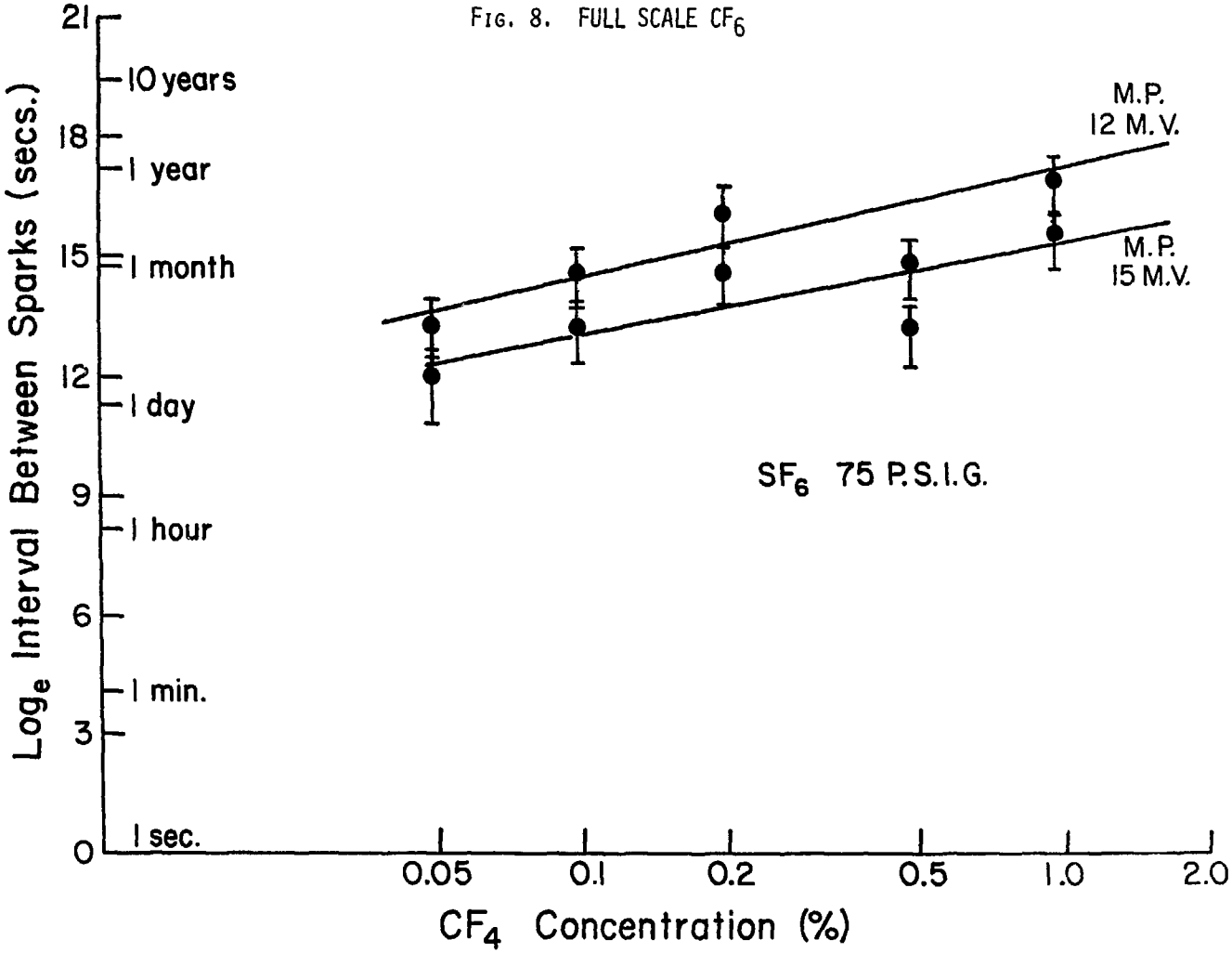


FIG. 8. FULL SCALE CF_6



SULFUR HEXAFLUORIDE PURIFICATION FROM MIXTURES WITH AIR -- A PROCESS FEASIBILITY STUDY FOR THE HOLIFIELD HEAVY ION RESEARCH FACILITY*

J. J. Perona
Oak Ridge National Laboratory

SUMMARY

A preliminary feasibility study was made for the purification of SF_6 vapor contaminated with air for application at the Holifield Heavy Ion Research Facility. Liquefaction appears to be a good way to recover about 90% of the SF_6 if it is badly contaminated (15% air), and even greater recovery will be possible for mixtures containing less air.

Where liquefaction is insufficient alone, adsorption of SF_6 on activated carbon at -50°F looks promising. Two carbon beds each containing about 500 lb of carbon should be sufficient. The refrigeration system for liquefaction and adsorption would have a capacity of about 2 tons.

As alternatives, the use of molecular sieves to trap out the air was investigated, but such a bed would require at least 15,000 lb of molecular sieves and very long cycle times. A large-scale desublimer was investigated and appears workable but would require some development work to permit design work to proceed with confidence.

INTRODUCTION

The Holifield Heavy Ion Research Facility at Oak Ridge, now under construction, will use SF_6 as an insulating gas. A description of the Facility is given by Jones (1977). The accelerator is contained in a pressure vessel filled with approximately 240,000 lb of SF_6 gas at a pressure of around 100 psia. When entry into the pressure vessel is required, the SF_6 must be transferred to storage. The SF_6 will be stored as a liquid at ambient temperature in three 2000-ft^3 tanks at about 350 psia.

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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.

The accelerator vessel will be evacuated before filling with SF_6 from liquid storage, but of course it cannot be evacuated completely. Therefore, the concentration of air in the SF_6 will gradually rise as the SF_6 is transferred back and forth, and eventually the SF_6 will require purification. Mistakes in operating procedures could also bring about contamination of the SF_6 with air. Therefore, the need for a purification process for the SF_6 is virtually certain during the many years of operation projected for the accelerator. The need for purification arises not from a deterioration of insulating properties but rather from the necessity of liquefaction at ambient temperature for storage.

Purification of SF_6 has been carried out on a small scale at other accelerator laboratories. Separation from air by freezing out the SF_6 with liquid nitrogen has been done at Chalk River (SNEAP-74). The use of an activated charcoal adsorption column for purification from air and helium was described by Brassard (SNEAP-76) in which the contaminated gas was introduced to the column in short bursts and the column was operated as a chromatograph. With the column at -20°C (-4°F), a 650-gm burst of gas was admitted and the effluent for the following 1 min. was vented as primarily contaminant gases. The SF_6 was then collected for the next 15-to 20-min period. The column was 8 in. in diam by 33 in. long and had a production rate of 100 lb/day. Rowton (SNEAP-76) reported some studies of air adsorption into Linde 5A molecular sieves. Use of these methods was investigated for application to the present system.

SELECTION OF BASE CASES

As a worst case, let us consider the following incident: SF_6 is pumped from liquid storage into the accelerator, but the accelerator was not first evacuated. The accelerator filling is stopped at the normal operating pressure of 85 psig, but it now contains 1 atm partial pressure of air. The composition of the accelerator tank is 207 lb-moles of air and 1200 lb-moles of SF_6, giving a mole fraction of air of 0.15.

This incident badly contaminates most of the SF_6 inventory and, therefore, serves as a "worst" case. The purification process must be capable of restoring the SF_6 inventory to a purity in the neighborhood of 99 percent in a period of two to three weeks. It should also be capable of routinely purifying a small recycle stream containing approximately 1% air. These two cases are denoted as "Case 1," the worst case, and "Case 2," the routine purification of slightly contaminated SF_6.

PURIFICATION BY LIQUEFACTION

One of the simplest operations which can be performed with the contaminated gas is to cool it and produce a liquid phase. Two questions naturally arise: (1) how much of the SF_6 can be recovered by liquefaction, and (2) how pure will the liquid be? The distribution of gases and liquids obtained from an initial gas mixture can be obtained by the application of Dalton's and Raoult's laws:

$$y_{SF_6} P_T = X_{SF_6} P_V, \tag{1}$$

where

y_{SF_6} = mole fraction of SF_6 in vapor,

P_T = total pressure,

X_{SF_6} = mole fraction of SF_6 in liquid,

P_V = vapor pressure of pure SF_6.

Since we are not interested in conditions that result in air solubilities greater than 1 or 2%, we can take $X_{SF_6} \sim 1$ with the understanding that results are not accurate where this assumption does not hold. Therefore, to a good approximation the composition of the vapor phase is fixed when the total pressure and temperature are specified.

A property of importance is the solubility of O_2 and N_2 in liquid SF_6. Despite an extended search in the library and personal contacts with SF_6 manufacturers, no published data on solubilities were found. Therefore, solubilities were estimated using the regular solution theory of Hildebrand, Prausnitz, and Scott (1970). The relationship takes the form

$$-\ln X_i = \ln \frac{\lambda_1}{y_i P} + \lambda_2, \tag{2}$$

where X_i = equilibrium solubility of gas i (mole fraction),

$y_i P$ = partial pressure of gas i (atm),

λ_1, λ_2 = constants which depend on gas and liquid properties and temperature.

Values of λ_1 and λ_2 for O_2 and N_2 in liquid SF_6 were estimated. If temperature and total pressure are chosen, the

gas-phase composition is fixed by the vapor pressure of SF_6 and the ratio of O_2 and N_2 in air. By choosing a range of pressures for several temperatures of interest, the solubilities shown in Fig. 1 were calculated.

As the total pressure decreases to approach the vapor pressure of SF_6, the partial pressure of air must approach zero and the solubility must approach zero. Hence the solubility curve for -50°F approaches an asymptote of 40 psia as the solubility becomes very small.

The amount of SF_6 liquefied at a given total pressure and temperature is calculated by obtaining the composition of the vapor phase from Eq. (1) and using the fact that essentially all of the 207 moles of air remains in the vapor phase. The amount of SF_6 condensed is the difference between the original 1200 moles and the amount in the vapor. Figure 2 was constructed by this procedure and shows that in Case 1 over 95% of the SF_6 can be liquefied at moderate pressures if the mixture is cooled to 0°F or below. The solubility estimates in Fig. 1, however, indicate that the amount which can be liquefied is less than 90% at temperatures down to -50°F if the liquid-phase composition is to be limited to 1% air. A flow sheet for purification for Case 1 utilizing liquefaction is given in Fig. 3, which is based on a 20-day cleanup period for this worst-case incident. After liquefaction, a vapor phase remains which is 40% SF_6 and amounts to 1050 lb SF_6/day. This stream requires further purification by some operation labeled "separator" in Fig. 3.

For Case 2, starting with a vapor containing 1% air, liquefaction appears to be a more efficient way to purify SF_6. At -50°F and a total pressure of 50 psia, over 96% of the SF_6 is liquefied, and from Fig. 3 the composition of the liquid phase is only 0.0018 mole fraction air.

PURIFICATION WITH MOLECULAR SIEVES

Molecular sieves, with very uniform pore sizes, are available commercially with pores too small for the SF_6 molecule to enter. A separation process can be devised in which the mixture of air and SF_6 passes through a bed of molecular sieves, and the air is trapped out while the SF_6 passes on through. Consider the "separator" in Fig. 3 to be a bed of molecular sieve. As the gas stream passes through the bed and loses air, it becomes saturated and liquefies. The liquid might cause problems by blinding the pore openings, thus preventing the gaseous air from entering. After the bed is saturated with air, it would be taken off stream, depressurized, and warmed to release the air in preparation for another cycle.

The required size of a bed of molecular sieves for this application was discussed with engineers at Linde. The bed cannot be operated below approximately -50°F because the SF_6 would freeze and plug the bed. Equilibrium loadings for specially dehydrated (activated) molecular sieves at -50°F are 2.5 lb of oxygen and 6 lb of nitrogen per 100 lb of molecular sieves. Practical design values recommended by Linde are 1 lb oxygen/100 lb and 4 lb nitrogen/100 lb. For the conditions presented in Fig. 3, a minimum bed size of 15,000 lb is required. Molecular sieves, being ceramic oxides, are very poor conductors of heat, and temperature cycling of such a large bed would require prohibitively long periods of time. Therefore, molecular sieves do not appear to be a good solution to the problem.

PURIFICATION BY DESUBLIMATION

Freezing out SF_6 from a mixture with air is practiced on small batches of gas at a few accelerator facilities. The feasibility of desublimation on a large continuous scale was investigated for the present application. Desubliming is not a widely practiced operation, and the engineering of desublimers is not well established in comparison with most other unit operations. However, a great deal of the expertise existing is at Oak Ridge, arising from the routine desublimation of UF_6. A design procedure including a computer program was written by D. Dunthorn (1968) and was applied to the desublimation of krypton by R. Eby (1978). Hence the design procedure which is available has been tested on two different chemical systems and has been verified experimentally on both of them.

The desublimer would be the separator in Fig. 3. To recover 90% of the SF_6 entering the desublimer (99% of the SF_6 leaving the accelerator), the partial pressure must be reduced to 4 psia, which requires a temperature of -115°F. Allowing for heat transfer resistance, a cooling fluid temperature of -130°F or below is needed.

The desublimer can have many geometrical configurations, but one of the simplest and most effective is the finned tube (Fig. 4). As the superheated gas flows through the tube, the gas near the cold surface is cooled to the point where the partial pressure of SF_6 exceeds the vapor pressure, and solids are deposited on the cold surfaces. The loss of SF_6 from the gas near the surfaces gives rise to a transverse mass transfer driving force, and the rate of mass transfer becomes one of the most important rate phenomena occurring in the desublimer. At the same time, the bulk gas is cooled as it flows along the tube and may generate SF_6 "snow" when

it becomes subcooled. Both phenomena must be taken into account in the design. The greatest uncertainties in the calculations lie in the values of density and thermal conductivity of the deposited solids.

The design program of Dunthorn assumes that the snow is deposited at the same axial position where it is formed. High gas velocities cause the snow to be carried further along the tube. UF_6 desublimers are fitted with filters at the gas outlets to prevent loss of snow. A rule of thumb from UF_6 experience is that the accumulation of snow on the filter is small if the superficial gas velocity is kept below 0.5 ft/sec. Eby (1978) made one run with a gas velocity about twice this value and did not find significant snow accumulation at the gas outlet.

Guidelines for estimating the density of the deposited solids are very rough. Dunthorn (1968) reported a frost-to-liquid density ratio of 0.69. This does not check very well with the generally accepted meteorological ratio of 10 inches of snow per inch of rain. Eby assumed a ratio of 0.5 for krypton but did not confirm it directly through his experiments. If we assume a ratio of 0.5 for SF_6, we obtain a solids density of 57 lb/ft^3.

The volume of a desublimer 12 inches in diameter and 15 ft long is about 12 ft^3. If the solids density is assumed to be 57 lb/ft^3, such a desublimer would hold at maximum 670 lb of SF_6 solid. Since a desublimer cannot be filled completely, let us assume that a desublimer of this size could be loaded with 525 lb of SF_6. Comparing with stream 3 in Fig. 3, we find that two cycles of loading per day would handle the flow.

The operation of a 12-inch-diam desublimer 15 ft long with 16 radial fins was investigated for a coolant fluid temperature of -150°F. The heat transfer coefficient (h) between the gas and the wall was calculated with the following equation (McAdams, 1950):

$$\frac{h}{LG} = 0.67 \left(\frac{LG}{\mu}\right)^{-1/2} Pr^{-2/3},$$

where L is the axial length of the fin. High coefficients can be obtained with small values of L, which keep the boundary layer from becoming well developed. Even though our exchange was 15 ft long, L could be made small by specifying that the fins were cut and rotated every 6 inches or foot of length.

The results of the computer study show that the desublimer would reach a loading of 525 lb in about 2.5 hours. The gas velocity would remain well below the 0.5-ft/sec limit. The effluent gas composition (mole fraction-SF_6) during the loading period ranged from 0.007 after 15 min to 0.048 after 2.5 hr and resulted in an SF_6 recovery for the desublimer of well over 90%. Our calculations indicate that this is a workable design.

Since the thermal conductivity and density of the deposited solids are strictly guesses, the sensitivity of the design to these variables was investigated. A thermal conductivity of 0.1 Btu/hr-ft-°F was used in the previous calculations, which is the same value which seems to work for UF_6 desublimers. Computer runs were made in which the thermal conductivity was decreased by a factor of 10 to 0.01 Btu/hr-ft-°F and in which the density of the deposited solids was decreased by a factor of 2 to 30 lb/ft^3. The effects of these changes were similar. The design as it stands would not work because the effluent gas concentration of SF_6 would become excessive after about 1.5 hr. The hardware could be made to work by using three cycles per day, collecting 350 lb/cycle in about 1.5 hr. If the low values of both density and thermal conductivity are applied at the same time, a larger desublimer would be required.

Based on the design calculations, the use of a desublimer appears feasible for the purification of SF_6. But, the uncertainties of solids properties would require that a small-scale desublimer be built and tested against design calculations before design of a full-scale desublimer could proceed.

PURIFICATION BY ADSORPTION ONTO ACTIVATED CARBON

The affinity of SF_6 for activated carbon has already been noted. A conventional process arrangement would be to pass the SF_6 air mixture through the bed, allowing the SF_6 to be adsorbed and the air to flow on through. When the bed is saturated and SF_6 begins to break through, the bed would be taken off stream and warmed up to release the SF_6. A search of the literature turned up no data on equilibrium loadings of SF_6 onto activated carbon.

A simple apparatus for measuring the adsorption capacity of activated carbon for SF_6 was assembled, as shown in Fig. 5. Dry air and SF_6 gases were metered into a cooling coil and passed into a column packed with activated charcoal. The column was 2 in. in diam, 2 ft long, and contained 430 g of activated charcoal. The gases and column were cooled to -50°F to -70°F before the start of a run and maintained

there. The effluent gases from the column passed through a gas density analyzer (manufactured by GOW-MAC) which provided a continuous analysis of the gas composition.

Ten runs were made with gas-phase compositions ranging from 0.068 to 0.58 mole fraction SF_6 (Table 1). Breakthrough curves were very sharp for all cases, with essentially zero percent SF_6 in the effluent gas for the first 30 min, then the effluent composition suddenly increased from 0 to 100% SF_6 within a minute or two. This behavior is very desirable in an adsorption system, permitting high utilization of the bed. Bed loadings are also encouragingly high, being greater than 1.0 gm SF_6 absorbed per gm of carbon for all runs and ranging up to 1.48 gm SF_6 per gm of carbon.

A large heat of adsorption effect was noted. During a run, the temperature near the center of the bed would suddenly rise above 0°F as the mass transfer zone reached that part of the bed, and then slowly fell to nearly its original temperature at breakthrough. Thus, the loadings cannot be taken as isothermal data.

The loading data in Table 1 do not correlate with partial pressures of SF_6, as might be expected. The loadings are so high that nearly all of the void volume of the charcoal bed is filled with SF_6 in these runs, and thus the expected correlation was not found.

Preliminary feasibility calculations for a purification system as shown in Fig. 3 using charcoal adsorption for the separator are favorable. The separator could consist of two charcoal beds, one onstream and loading for 12 hr while SF_6 is recovered from the other by a temperature cycle. Each bed requires a capacity of 525 lb of SF_6, which means the bed must contain 525 lb of activated charcoal for a loading ratio of 1.0. Using a charcoal density of 35 lb/ft^3, each bed would have a volume of only 15 ft^3. These volumes are very reasonable.

A refrigeration system to cool 8750 lb SF_6/day and 300 lb air/day to -50°F also is reasonable. The enthalpy of SF_6 gas at 70°F is 53 Btu/lb, of SF_6 gas at -50°F is 43 Btu/lb, and of SF_6 liquid at -50°F is -2 Btu/lb (basis: saturated liquid at -40°F). The total refrigeration requirements are less than 20,000 Btu/hr for cooling and liquefying gases. Cooling requirements for temperature cycling the carbon bed would be only about 20% of this. Thus, 2 tons of refrigeration should be adequate. A rough estimate of the cost of such a system can be obtained from the Union Carbide Investment Estimating Manual, which when corrected for 1978 prices, indicates a cost of around $15,000.

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ACKNOWLEDGMENTS

The contributions of R. R. Brunson, S. D. Clinton, J. S. Watson, and R. E. Barker to the experimental work are gratefully acknowledged. The probing questions and careful review of the manuscript by C. Jones were very helpful.

Discussion:

Wegner: You didn't mention anything about the recovery of the SF_6 from the charcoal bed. Presumably you heat this bed up and pump the SF_6 out of it. Do you get pure SF_6, or as the pressure goes down and down do you gradually start pulling air and other things back, some residual hold up of air and things in the bed, how does that work out on recovery?

Perona: There should be no air in the bed. The critical temperature of oxygen and nitrogen is down around about -80 or -90°F, so the bed is being operated above the critical temperature which means that there should be no absorption of air. Charles Jones has worried about this a bit and has asked us to get a data point, which we are in the process of doing, but I feel confident that we will get essentially pure SF_6.

Chapman: This activated carbon system, if I'm not mistaken, is effectively the same as the Montreal System that Brassard has described in the past, is it not?

Perona: It's operated in a different way. That was operated essentially as a chromatography-type device. He put a short burst of oxygen and SF_6 into one end and then let that burst travel through and, let's see, I think the air came through first and then he captured the sulfur hexafluoride. It's a very different kind of operation, one which is not used in chemical processing engineering. We think that this is a more effective way to use such a thing.

Rowton: Joe, what temperatures and pressures did you handle with this activated carbon? I guess another way to ask the question is: did you have refrigeration in the activated charcoal bed or did the precooled sulfur-hex carry all the heat?

Perona: This was sort of a quick and dirty thing, Larry. We packed the bed in dry ice. We did precool the gas mixture with a coil as we mentioned. In the flow sheet we just arbitrarily said we will leave the separator device operating at a hundred psi, which is what we used in our liquefaction. In the experiments which I showed you, we were actually operating the thing at about 15 to 20 psi. Of course, the higher the pressure, the better the deposition is going to be. So the pressure is not critical. What we have done is something very conservative. If you operate at a higher pressure it should work better. And, incidentally, there is a good possibility that if we operated at a higher temperature than -50, the thing would still work pretty well. Those loadings essentially fill all the void spaces with sulfur hexafluoride, those loadings above one pound per pound. So we think you could operate at a higher temperature and it would still work well. And we are going to get a bit more data in that range.

Table 1. Adsorption of SF_6 onto activated carbon

Run	Feed comp. mole fract. SF_6	Inlet pressure (psia)	Inlet temp. (°F)	Time to breakthrough (min)	Partial pressure SF_6	Loading gm SF_6/gm C
2	0.251	28.4	-63	31.5	7.14	1.48
3	0.336	21.7	-63	34.8	7.29	1.43
4	0.423	20.7	-62	30.0	8.77	1.20
5	0.780	17.7	-62	36.5	10.27	1.32
6	0.370	28.7	-51	25.8	10.80	1.03
7	0.205	24.2	-76	29.4	4.97	1.09
8	0.165	20.7	-65	50.3	3.41	1.32
9	0.118	22.7	-73	57.1	2.69	1.07
10	0.068	27.2	-55	113.3	1.86	1.12

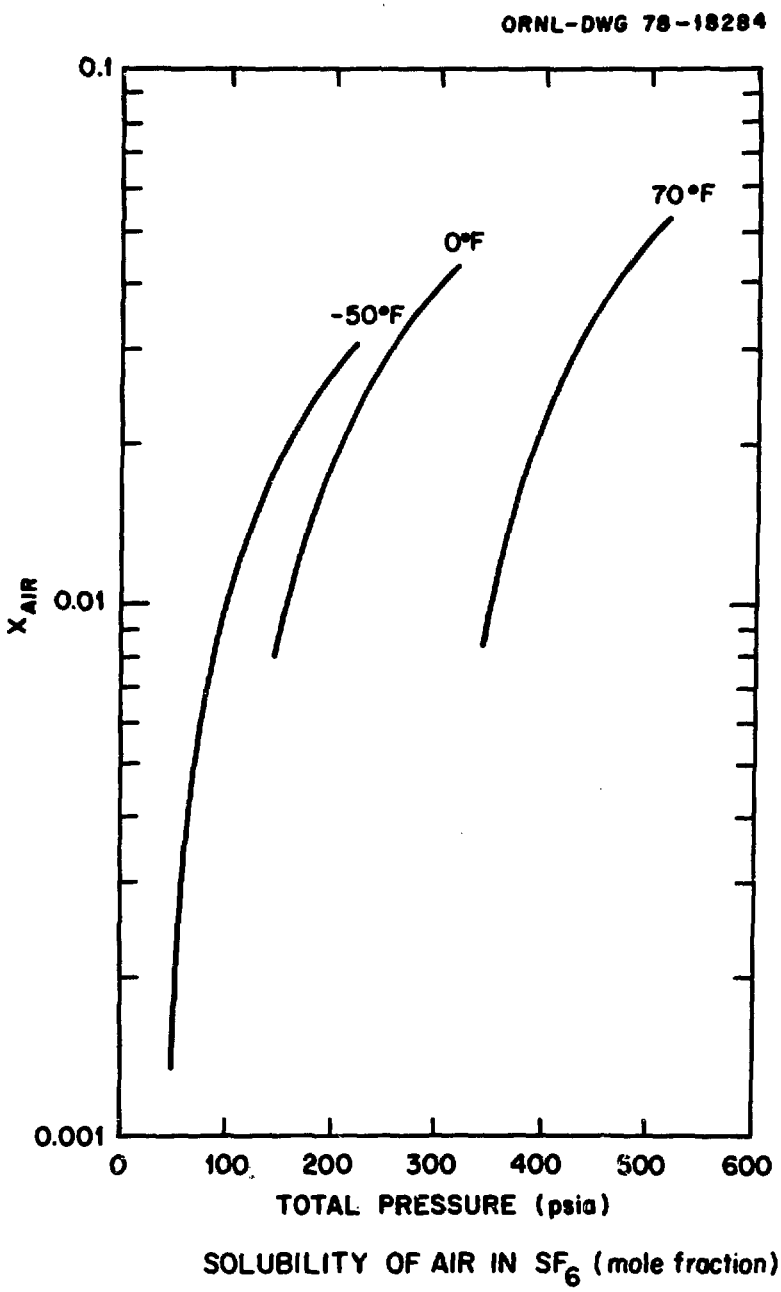
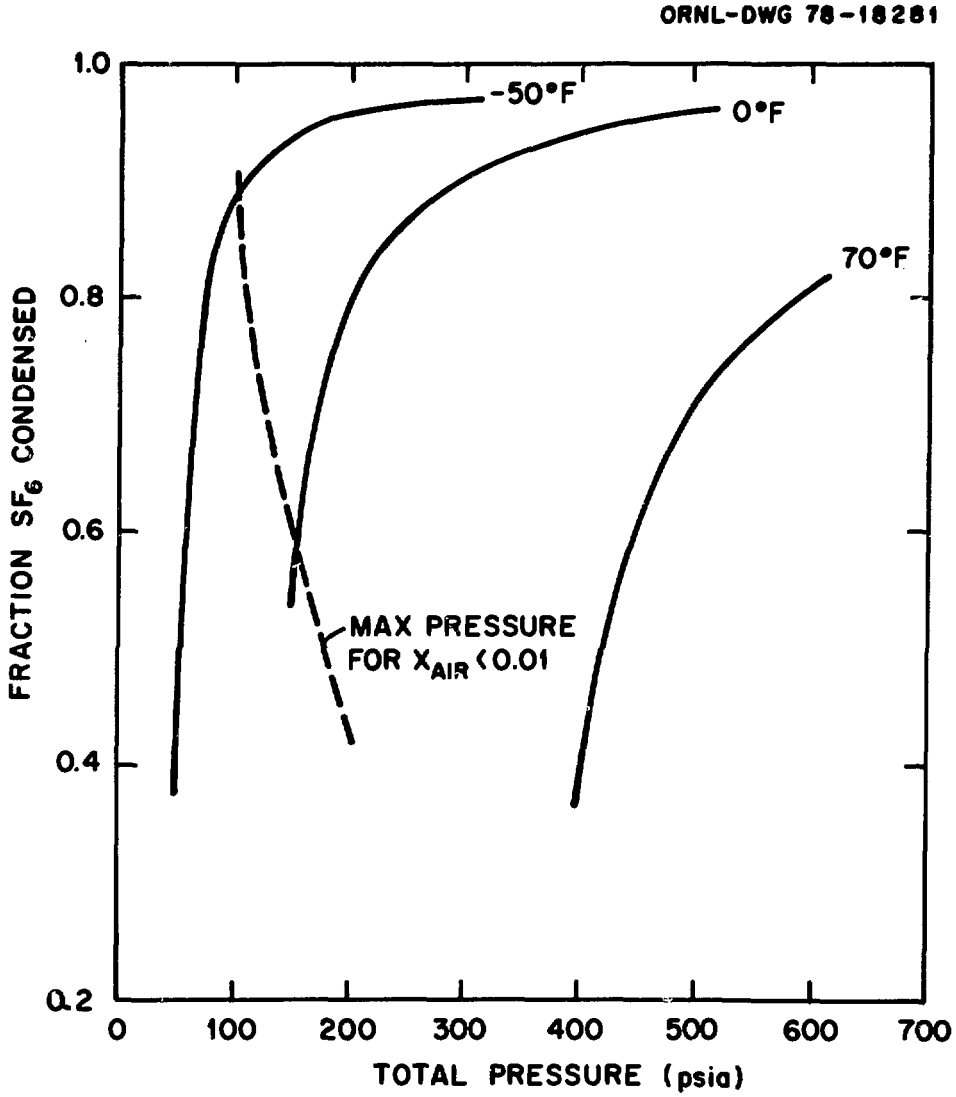

ORNL-DWG 78-18284
0.1
0.01
0.001
X_{AIR}
-50°F
0°F
70°F
0
100
200
300
400
500
600
TOTAL PRESSURE (psia)
SOLUBILITY OF AIR IN SF_6 (mole fraction)

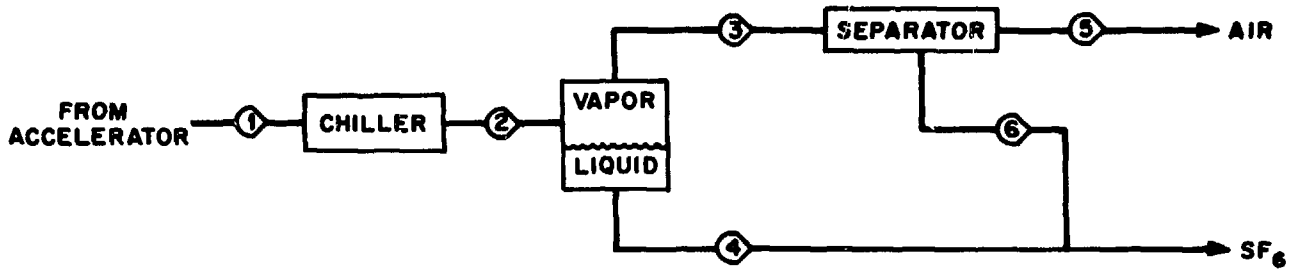
FIG. 1



FRACTION SF_6 CONDENSED STARTING WITH Y_{AIR}=0.15

FIG. 2

ORNL-DWG 78-18285

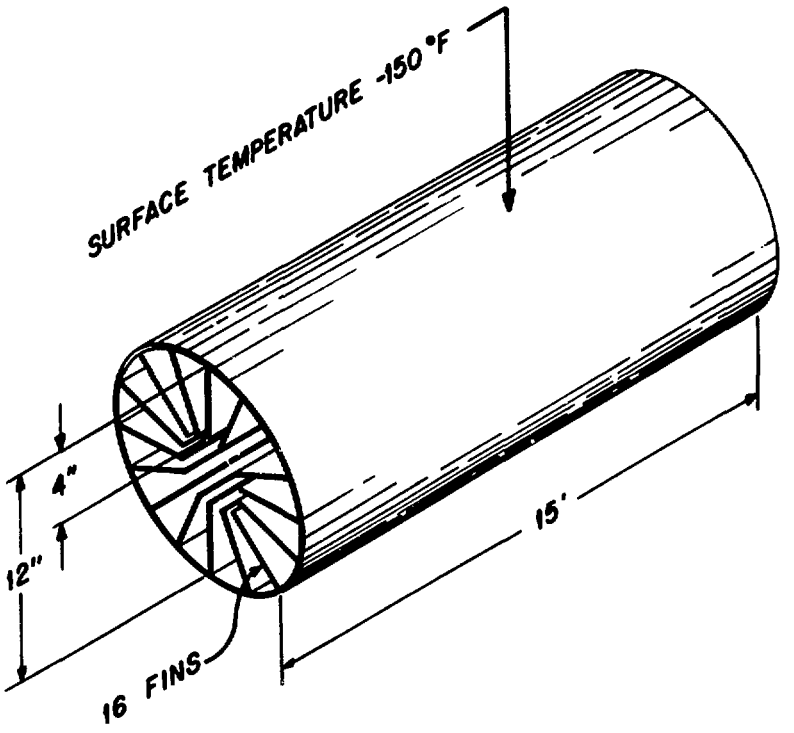


	1	2	3	4	5	6
TEMPERATURE (°F)	70	-50	-50	-50	-	70
PRESSURE (psig)	85	85	85	85	85	0
FRACTION LIQUID	0	0.90	0	1.0	0	0
AIR IN VAPOR (mole fraction)	0.15	0.60	0.60	-	0.96	0
AIR IN LIQUID (mole fraction)	-	0.01	-	0.01	-	-
FLOW RATE* (lb/day)						
SF_6	8750	8750	1050	7700	50	1000
AIR	300	300	285	15	285	0

* 20 DAY CLEANUP

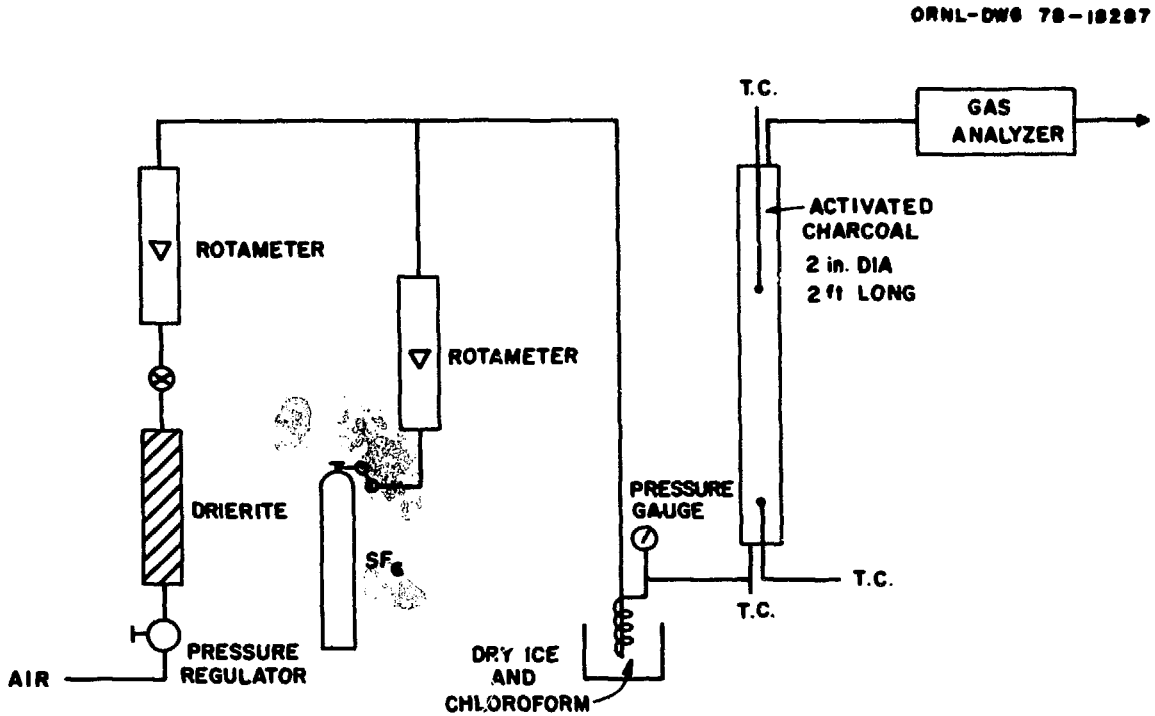
FIG. 3. FLOW SHEET

ORNL-DWG 78-18286



FINNED-TUBE DESUBLIMER

FIG. 4



APPARATUS FOR ACTIVATED CARBON ADSORPTION OF SF_6

FIG. 5

Dup

A Preliminary Comparison of MP Sparking Characteristics for SF_6 Insulating Gas Mixtures and Pure SF_6*

R. Lindgren and H.E. Wegner

Brookhaven National Laboratory, Upton, NY 11973

The second of the two MP accelerators (MP-7) that comprise the three-stage Van de Graaff facility at Brookhaven National Laboratory was operated for a period of approximately 600 hours with pure sulfurhexafluoride (SF_6) insulating gas at 115 PSIA, followed by 600 hours operating with a mixture of 50% SF_6, 40% N_2, 6% CO_2, and 4% O_2 at a pressure of 175 PSIA. The insulating gas mixture has been in use for several years; however, we were anxious to evaluate the performance with pure SF_6 as soon as the necessary amount of gas became available in our gas-storage system and the machine was in suitable and reliable operating condition for such a test. Most other MP accelerators operating in the 12.5 to 13 MV region all use pure SF_6 insulating gas in the region of 100-115 PSIA. We expected to find superior operation of MP-7 with the pure gas as compared with the gas mixture customarily used because the partial pressure of SF_6 in the mixture corresponds to 87.5 PSIA as compared to 115 PSIA in the case of the pure SF_6. We were surprised to find that the machine was considerably more difficult to operate in the 12-13 MV region with the pure SF_6 than with the mixed gas and could not be conditioned to 13 MV. This short note is an attempt to characterize the obvious

*Research supported by the Department of Energy under Contract No. EY-76-C-02-0016.

(to an operator) operational difference between the pure and mixed SF_6 insulating gas by means of a statistical analysis of sparks that occurred during comparable running periods.

Before the test run with pure SF_6, the machine was opened and all the resistors were checked and the machine generally adjusted for the best possible mechanical and electrical operating condition. It was then pressurized with pure SF_6 (115 PSIA) and the research program was continued over a period of approximately 600 hours. In each test run the gas was dried too. During this time, all major voltage breakdowns or sparks were recorded and subsequently analyzed. Approximately one half the sparks were terminal-to-tank and the others were due to chain or column breakdown. Tube sparks with correlated vacuum activity were extremely rare. The only parameter convenient for this preliminary analysis and limited statistics was the length of time after a given spark until the next spark while operating at a particular voltage. In some cases the voltage would be changed to accommodate the experimental program without any sparks occurring and a summation of those periods of spark-free running was also included. The machine was operated mostly between the terminal voltages of 10 and 13 megavolts (MV) with most of the sparking occurring between 11.5 and 13 MV. The number of sparks occurring within one to ten minutes after a spark at a particular voltage was comparable to the number occurring between 10 and 100 minutes and 100 and 1,000 minutes.

It was found that the machine could not be conditioned to 13 MV during the test period with pure SF_6 and was limited to a maximum of 12.8 MV. It was also soon discovered that when a terminal spark occurred it was important to hold the voltage down on the machine for at least 15 or 20 minutes or in some cases even longer before coming back up to the previous operating voltage. If the voltage was returned too soon, there appeared to be a higher probability of resparking than was customarily encountered with the gas mixture where the voltage was always brought back up in 5-8 minutes.

The operation with pure SF_6 was terminated when the research program required 13 MV in the next scheduled running period. The SF_6 was then removed from the machine without opening it and repressurized with the SF_6 mixture customarily used for our normal operation. The subsequent data was analyzed for a period of approximately 600 hours with the SF_6 mixture similar to that analyzed for the previous operation with pure SF_6.

At the beginning of the test run with the SF_6 mixture, the machine was quickly conditioned to 13.2 MV and a relatively long running period for the research program was carried out successfully at 13 MV, a voltage that was not obtainable with the pure SF_6. The research program subsequently required lower voltages and over the 600-hour running period the machine averaged a lower overall voltage than the previous test period with pure SF_6. In general, the spark distributions in terms of the different time ranges of

sparks as a function of voltage was very similar to that encountered with pure SF_6 and a comparison of these two preliminary spark analyses are shown in Figs. 1a and 1b. Figure 1a shows a comparison of terminal sparks per hour in half MV intervals as a function of terminal voltage for the pure and the mixed SF_6. The two curves show that within the statistical uncertainties of this preliminary analysis, there would appear to be little or no difference in the performance for the mixed or pure SF_6.

A different kind of analysis of the spark frequency was made in Fig 1b where the number of terminal sparks per hour occurring in the 10 to 100 minute interval of time was compared for the two machines in the limited region of 12 to 13 MV. A spark occurring in the 10 to 100 minute period interferes severely with the research operation and is the kind of spark that cannot be tolerated in the course of carrying out an experimental run with the machine. The number of minutes that the machine was operated at a specific voltage is also indicated to give a visual indication of the kind of spark frequency encountered at the different voltages. With the exception of the anomalous high point at 12.6 MV, it appears that the gas mixture shows little or no change in sparking frequency from 12 to 13 MV while the pure SF_6 appears to show a rapidly increasing tendency to spark above 12.6 MV and actually was not operable at 13 MV. This trend in the data for pure SF_6 is consistent with other MP accelerators operating on pure SF_6.

The main conclusion of this preliminary test and analysis of the performance of MP-7 with the mixed and pure SF_6 is that the operation is certainly different; however, the differences cannot be quantitatively characterized with such meager statistics amounting to a total of approximately 200 sparks, 600 hours, and a limited voltage region for each of the two comparisons. The re-sparking characteristics after an initial spark are quite different between the two gases; however, it was not possible to quantitatively characterize this phenomenon.

Further experiments of this kind are planned in the future at much higher pressures of pure SF_6. Similar tests are also planned for a small 1 MV test machine to determine if any of the general operating characteristics observed with the MP can be duplicated with the smaller machine where tests can be made much more rapidly and under conditions that do not have to be compatible with carrying out a research program.

Discussion:

Jones: Presumably the pressure of SF_6 in the mixtures was the same.

Wegner: No, it turns out that the pressure was slightly higher. The partial pressure of SF_6 was 80 psi gauge equivalent in the mixture compared to 90 in the pure. So, in effect, the mixture has less SF_6 in it than the pure SF_6 present in the machine.

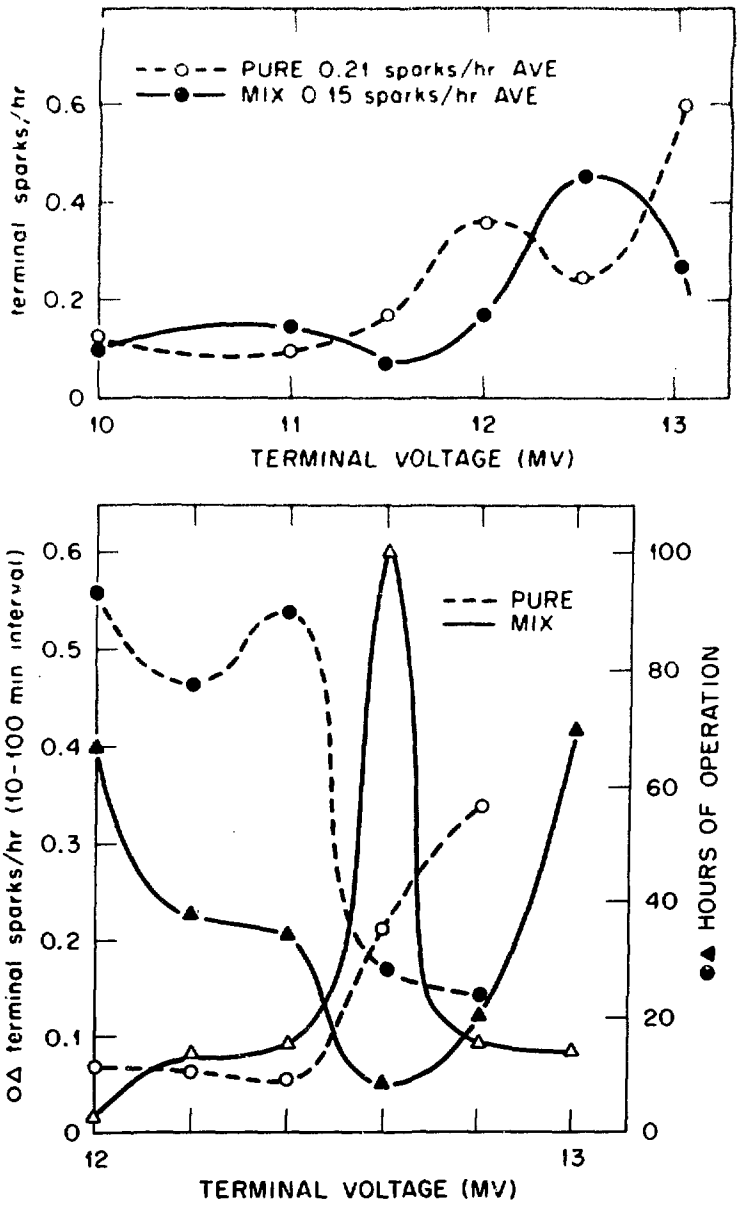


Fig. 1. Comparison of terminal sparks/hr vs. voltage in MP-7 under different conditions (see text).

THE SEPARATION OF AIR FROM SF_6 USING 13X MOLECULAR SIEVE AT ROOM TEMPERATURE

P. I. Hurley
Chalk River Nuclear Laboratory

Operators of MP Tandem accelerators will appreciate the need to keep the percentage of air in SF_6 as low as practicable. Air has been separated from SF_6 at Chalk River by using liquid nitrogen to freeze the SF_6 and then venting the remaining gas to atmosphere. Another separation process was developed by C. Brassard, University of Montreal, which used a column of chilled charcoal through which the SF_6 diffused more slowly than lighter gases. The MP Tandem at Heidelberg had the nitrogen and carbon dioxide removed from its SF_6 by Linde using a commercial process, part of which was reported to be freezing the SF_6 with liquid argon. L. Rowton, Florida State University, used chilled 5A molecular sieve in a pilot process to successfully remove air from SF_6.

Considering the large difference in molecular weight and size between normal air components and SF_6 it should be possible to separate air and SF_6 by a very simple, energy efficient flow process in a sorber column. At Chalk River an attempt is being made to produce a once through system with an output SF_6 purity of 98% or better which would operate at close to room temperature. The process consists of putting a charge of SF_6 and air mixture into a column loaded with 13X molecular sieve pellets; then pumping the initial outlet pressure rise (air) to atmosphere and retaining the second rise (SF_6) to be returned to the accelerator tank.

During the past summer, S. P. McGarry, an undergraduate from the University of Waterloo, working at CRNL on absorption materials, selected molecular sieve 13X as the preferred material. Selection consisted of measuring the ability of the material to absorb, at ambient temperature, SF_6, from a gas mixture consisting of 70% SF_6 and 30% air.

Selection testing was accomplished using the test assembly illustrated in Figure 1. The ballast vessel in Figure 1 was filled with 70% SF_6 and 30% air mixture to one atmosphere of pressure. It was then connected to the evacuated test cylinder. V1 and V2 were opened, and the pressure was allowed to equalize. The final pressure of the system was recorded.

A flow system was developed from materials available in the laboratory. After a few tries the column structure described in Figure 2 evolved. The column was a five-foot piece of 2-inch copper pipe. Unfortunately the 13X molecular sieve, which was on order, had not arrived; so the column was filled with a mixture of 13X sieve of various sizes and some glass beads for fill.

Column operation consisted of: pumping the column down to approximately 150 microns; charging the input reservoir, a 3.2 litre cylinder, with the 70% SF_6 and 30% air gas mixture; closing valves V2 to V12 and V14; releasing the gas from the reservoir into the column by opening V1; pumping the first pressure rise (air) to atmosphere through V13. When the pressure as indicated by the thermocouple gauge at the column outlet bottomed and started to rise, V13 was closed and V2 to V12 and V15 were opened; the second pressure rise due to SF_6 was pumped to a collection cylinder. An analysis of the purified SF_6 from the initial flow tests was 92.9% SF_6, 4.3% oxygen and 2% nitrogen. An analysis of the gas discharged to atmosphere was 1.3% SF_6, 26.7% oxygen and 70.6% nitrogen.

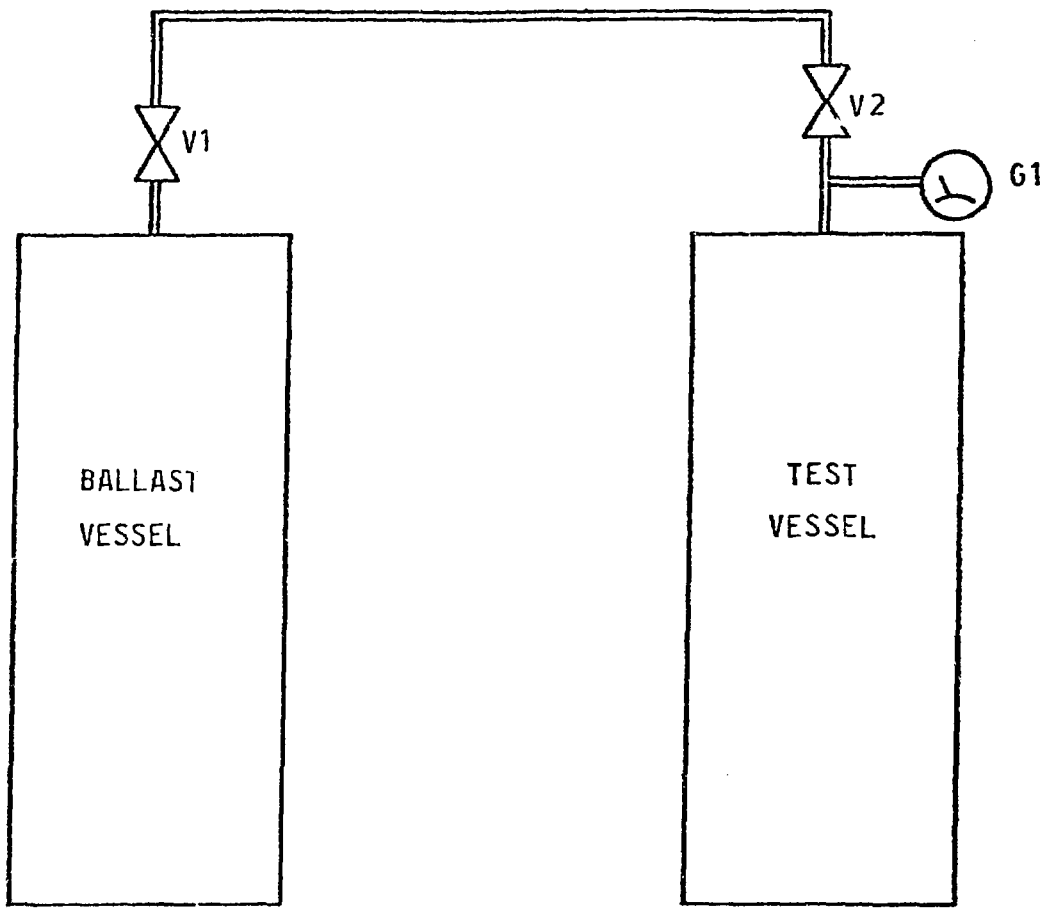
Since the components used in the column were normal plumbing components rather than vacuum components the results were encouraging. However, the oxygen content in the "purified" SF_6 and in the discharge to atmosphere was not in the proportions relative to the other gases that was expected.

We do not have on-line gas analysis to give immediate results from changes in the flow process. Equipment for on-line gas analysis is on order but is not expected to be in operation during 1978. Work on the flow process will continue by using pressure variations as an indication of separation.

When the column was heated to approximately 20°C above room temperature, the pressure dip between the air and SF_6 practically disappeared. Also, depending on the quantity of gas mixture injected into the column, the heat of absorption would raise the first half of the column a few °C. Since the process is obviously temperature sensitive, temperature stabilization would have to be included in a permanent installation. However, the objective of a nominally room temperature process will be pursued.

Nitrogen and/or oxygen have been cycled through the column with no build-up of base pressure. The time for a nitrogen and/or oxygen cycle was similar to the "air" pressure portion of the SF_6 and air cycle. Also, pure SF_6 cycled through the column behaved similarly to the SF_6 portion of the SF_6 and air cycle. Since 90% of the cycle time is required to pump out the SF_6, the present column valve configuration must be modified.

SF_6 ADSORPTION TEST ASSEMBLY



V1, 2 - HELIUM LEAK TESTED ¼" HOKE VALVES

G1 - 30" VAC. TO 30 psi PRESSURE GAUGE

FIGURE 1

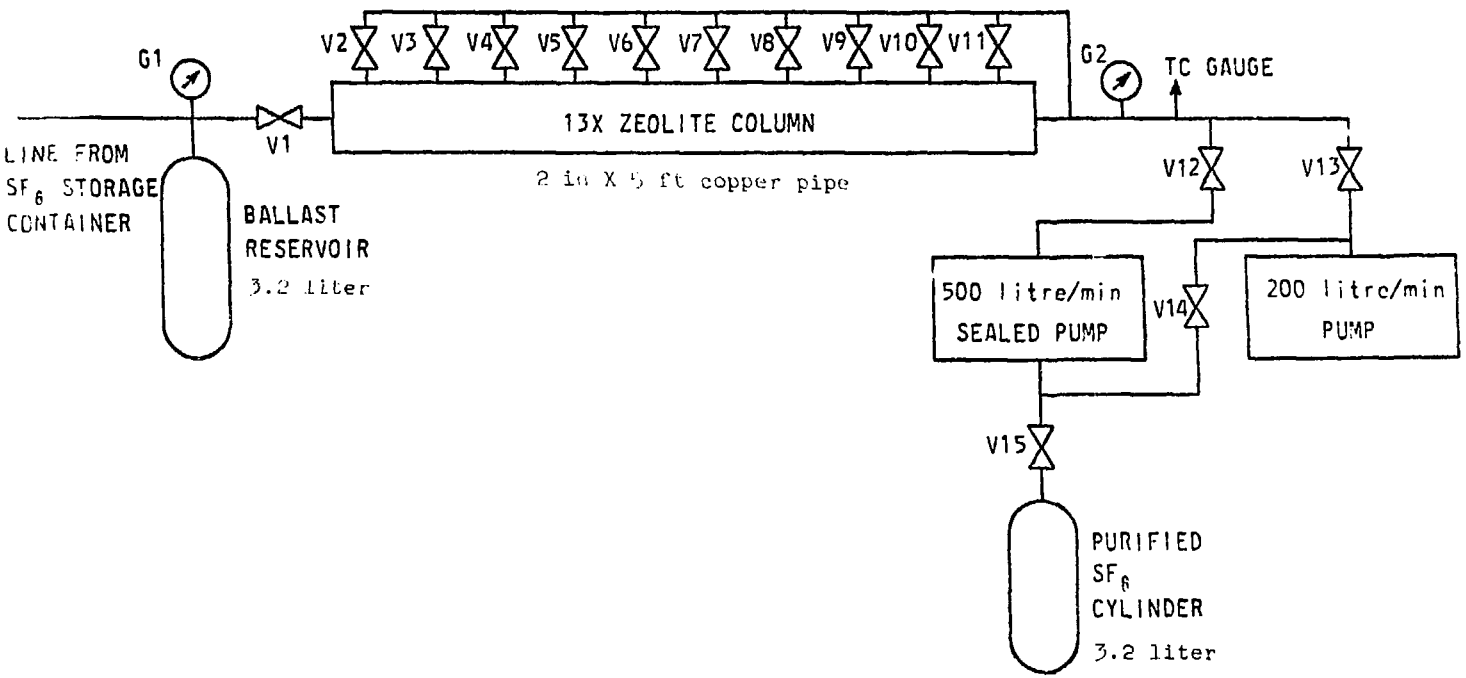


FIGURE 2 - GAS HANDLING SYSTEM

Session III, Chairman, J. W. McKay
Editor, J. A. Benjamin

REPORT ON THE FOURTH TANDEM CONFERENCE AT EBELTOFT

(June 5-8, 1978)

K. R. Chapman
Florida State University

This will be just a very brief report on the "Ebeltoft" Meeting to acquaint you with this series of conferences and to mention some of the topics discussed. These meetings are the European counterpart of our SNEAP and are distinct from the Daresbury, Strassburg Conferences. The meeting was held this year in Ebeltoft, Denmark and was organized by the Scandinavian Laboratories. They are normally held bi-annually, each in a different region -- the preceeding was held in Israel. This year's location was in a pleasant resort area and was convenient to Aarhus where tours of the laboratory were organized.

It should be pointed out that this report is of information now four months old and more recent data may now be available but, as no proceedings are produced, it is hoped that this talk will at least serve to inform you of where this work is in progress.

Reports were presented of the progress and status from many of the larger installations. At Rehovot they have been running for six months with a self-imposed limit of 12.5 MV but are now prepared to run to 14 MV although sparking still occurs above 12.5 MV with beam. The new corona tubes have not been installed long enough to enable an assessment of them to be made. Evidence of the long tube effect is seen. Running time between the EN and the UD 14 has to date been divided approximately equal.

In Munich, tube problems continue inspite of extreme care with the new tubes. Long tube effect is also seen. I shall not report further on this as Dr. Skorka is at this meeting and will present a paper on results with their tubes.

At Daresbury, the new accelerator is progressing well, in spite of building construction problems. This group, bringing a fresh approach to electrostatic accelerator technology, is continuing to perform extensive testing in order to reduce much of the art to a science as in circular accelerators and the results of this work will benefit us all in the years to come. The acceleration tube is now going into production although testing will continue. Work proceeding with slack foils offers considerable promise. Some idea of the magnitude of this project is perhaps given by consideration of the 35 meter counting room that is completely copper screened and the 6-8 tons of SF_6 required to just fill the pipes of the gas handling system.

In Oxford the folded tandem is nearing completion (reported now at Oak Ridge as operational) and is of special note on account of its budget of only £100,000 (~$200,000) for the conversion from a single ended accelerator.

At Padua the accelerator is a 16 MV XTU with Laddertron charging and has a projected operation date of Spring 1980. The tank has been ready since November 1977 and the building is partially completed. A super conducting cyclotron is proposed as a booster. The 1:6 scale model has completed testing successfully and the full sized one will perhaps be funded in 1980.

In Catania, a modified MP with a new tank design is guaranteed to 13 MV with 5-10 μA of protons by HVEC. It is expected that the building will start in a few months.

At Niel Bohr they have proposed a 25 MV Tandem.

At Helsinki they have ordered a EGP-10-2 from the Soviet Union. Delivery is scheduled for the end of 1980. The voltage will be modest 3-10 MeV protons at 3 μA. The tank is in 6 sections and will be a vertical one similar in size to an FN. The accelerator will have a 60 KV injector and will be equipped with only gas stripping. The installation will be rather unusual in that the ion source room will be at ground level and the target rooms in two underground vaults.

At the EGP-10-1, a technical report was presented on experience of some 5 years running on this accelerator. Belts have been a problem and 28 belts have been installed since 1973. Grading resistances have also given trouble, 40% having to be changed in 1974 and 80% in 1975. The maximum terminal potential has been held at 4 MV since 1975. But additional pumping is being installed including a separate pumping tube from the terminal to the base and it is expected that higher terminal potentials will then be used. The insulating gas is ~80% nitrogen + ~20% carbon dioxide with 3-5% of freon added. Beams of heavy ions up to bromine are available.

Several papers were presented dealing with accelerator components. Dr. Trump reported on the laddertron development at HVEC. The test laddertron has now been run for 2,400 hours on the test rig with up to 13 MV on the terminal during the day and a lower potential at night. Intervals of up to 8.8 hours have been recorded between sparks at 13 MV. The laddertron runs on 20 inch diameter pulleys and uses 10 inch diameter idlers and was run at speeds of from 8 to 20 meter per second. In the accelerator at Padua it will run at 12.5 meters per second. Initially, lateral

run out was a problem but by selective assembly in sections of 7 to 8 links this was corrected and has now been reduced to 1 mm. After these tests signigicant link loosening was observed but this is expected to be corrected by tighter tolerances. The chain will carry a charging current of 500 μA and requires 8 KW to drive it, this increasing to 12 KW when full charging current is drawn. HVEC are now prepared to offer a laddertron charging system for smaller accelerators such as the FN (they have in fact accepted an order from Nigeria to supply an FN with a laddertron charging system).

Several laboratories presented accounts of problems with grading resistors especially the HVEC 'blue resistors'. Saclay and Cologne have changed back to the earlier 'yellow resistors' with reproducible success. Copenhagen is also experiencing some resistance problems.

Dr. Skorka described a shorting rod assembly that enables sections of the column to be shorted out without opening the tank and a velocity filter that is useful in sorting the abundance of mass, charge state products frequently obtained when heavy ion beams are run.

Dr. Greenway currently with Aarhus presented a paper reminding one of the point so easily forgotten that if slits are used in the low energy beam line to control accelerator loading their position must be determined with attention to beam optical data. Plots were presented showing that by incorrect positioning of the slits along the beam line they may cut just those rays which travel nearest to the axis in the tube region of the accelerator.

Ion source work was also well represented in the sessions. Dr. Middleton presented details of his work on a new type of sputter source but this information will be presented later in this meeting.

Dr. Bland of Bochum described his work with a reflected sputter source and his results with beams of readily sputtered matterials were of special interest. These are usually the least easily handled in a conventional sputter source but by recessing the small 5 mm diameter target pill so as to produce a cylindrical hole above the surface of this pill the beam outputs obtained were increased by 10 to 25 times. This is presumably because the cesium collecting on this wall enhances the cesium supply to the sputtering surface.

Dr. Tykesson presented a most informative review of ion sources and pointed out once again that matching of an ion source emittance to the accelerator acceptance is of prime importance. The acceptance of a typical tandem is 10-20 πMM mrad $MeV^{1/2}$ while the emittance of typical ion sources are approximately as follows (in the same units)

direct extraction	2
penning	4
charge exchange	2
sputter source	10-20
reflected source	4-7
inverted source	$\simeq$6
Aarhus source	9

Thus it is clearly seen that unless the phase space figures are closely matched transmission will be lost. It is perhaps significant that in a survey of the labs reporting the following transmissions were quoted.

mass of accelerated particle	1-4	6-18	>18
transmission	60-80%	40-60%	20-45%

It was also pointed out that the phase space diagram of source emittance may change with time. For example, for a sputter source with the cesium incident normally on the sputtering surface, the initial emittance may be represented by a horizontal elipse. Later when the surface becomes ridged or pitted the angular spread of the beam is restricted and the elipse will become more vertical. This will change the matching and may explain part of the beam increase with time observed.

Several papers were presented on Post Accelerator Booster Projects, one in operation, some well advanced and some proposed. It appeared to be the general feeling of the meeting that direct scaling of electrostatic accelerators is approaching its logical limit at this time and that higher energies will be more readily and economically achieved by use of a booster. While several installations are under construction, few are currently operational but it was felt that a linear accelerator, probably operated cryogenically, appears to be the equipment of choice at this time.

In Berlin, the installation of their Van de Graaff cyclotron combination is completed and operation is expected later this year. The electrostatic accelerator is a CN with extended terminal, a ring being used to give the necessary tank extension. The four sector cyclotron passed acceptance tests earlier this year. The use of emittance measuring equipment at the entrance to the cyclotron allows the beam parameters to be set at this point for cyclotron injection.

In Heidleburg, the MP, linear accelerator combination is now operational with 10 non-crygenic resonators running. It is noteworthy that only a 7-8 week tandem shut down was necessary and the combination first operated successfully on December 22, 1977 with

7 resonators. By Christmas, 1978, it is expected to have the complete installation with 30 resonators operational.

In Zürich, they have proposed a 'Stonybrook' system with cryogenic resonators using lead as the super conductor.

In Saclay, they hope to have a booster shortly probably a linear accelerator.

A survey of a number of labs reveals the following system: pre chopper-retrace eliminator-buncher-acceleration-post acceleration chopper. This survey also shows an average over 10 labs of ≃10% of the DC beam in the final accelerated pulse.

This meeting was very productive and our hosts are to be congratulated on it's success. I would strongly recommend that we all make a point of keeping ourselves appraised of these meetings and the information they yield.

Discussion:

Berners: With regard to your comments on scraper slits, I believe the proper place for these slits is close to or within a lens. This is the place where the large divergent components of the beam have their maximum displacement from the axis. The worst place to put scraper slits is at a waist in the beam. Do you agree with those comments?

Chapman: I believe you are correct. I don't have the trajectory plots with me.

McKay: Thank you very much, Ken. Now all that remains is to persuade the sponsoring bodies in our own laboratories to send us to Europe once a year.

IMPROVEMENTS TO THE UNIVERSITY OF WASHINGTON FN BELT CHARGE AND GVM CONTROL SYSTEMS

Harold Fauska

Nuclear Physics Laboratory, University of Washington
Seattle, Washington 98195

The original Corona Control System on the University of Washington FN Tandem was the conventional unit supplied by High Voltage Engineering Company. The system required much adjustment and care but if one did the proper care and feeding it would regulate reasonably well.

The generating voltmeter as supplied was almost identical to the Trump and Van de Graaff paper in RSI February 1940. Lori Miller tells me Lord Kelvin invented the generating voltmeter, I did not research it that far back.

We tried many times to update and improve the system of regulation. Each new try yielded some measure of improvement.

The system we now use got a big plus from the RSI paper by L. G. Smith of May 1954. The break through is from the technique of adding the signals from the two sets of stators. The signals are 180° out of phase. The waveform of each is triangular, and since the sum of the areas of the stators exposed to the terminal potential is a constant independent of time, the high frequency response should be as good as the response from the so-called mushroom capacity pickup. The D.C. comes right along for free.

There are two problems with this system. First the input impedance must be very high (tens of megohms), and second the signal is capacity coupled. Therefore it is A.C. and as such really doesn't know where the ground reference is. This problem will be discussed below.

We tested the two stator signals into about one hundred megohm input system. The signals were indeed triangular. Slide one shows how they appeared. Note

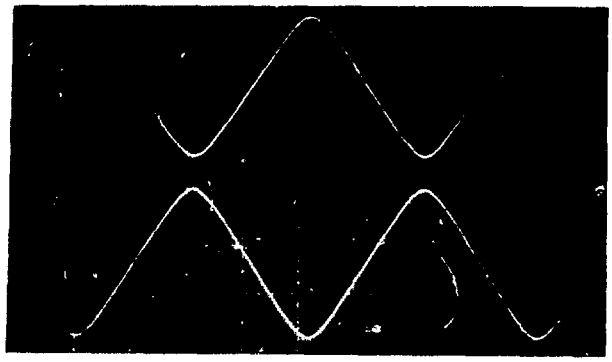


Figure 1

that they are very large: fifty-five volts peak-to-peak with only one MV on the terminal. The stator area always exposed is about seven and one-half square inches. Thus, we needed to attenuate the signals in order to utilize solid state circuitry. The need to preserve higher frequency response led to the use of capacity voltage dividing. By making a small variable capacity as part of the divider one can balance out the non-equality of the two stator signals.

The second problem of providing a ground reference to the triangular waveform was handled by a clamping circuit. It is shown in slide 2. The diode

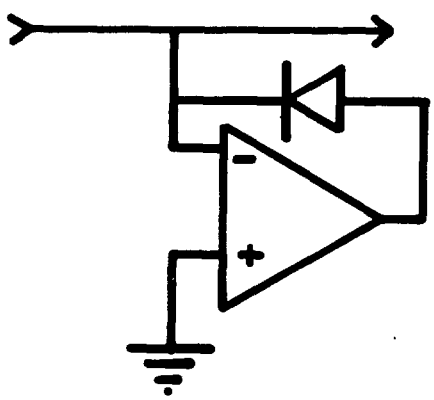


Figure 2

allows signals to build up in the positive direction, but then clamps any negative excursions. On repetitive signals, such as these, the waveform after

Figure 3

several cycles is referenced to ground, as shown in slide 3. Next the clamped signals are added. The coupling time constant must be long enough such that the change on the capacitors does not cause a glitch at the clamp point on the waveform.

The adding retains all the high frequency information, and if the stator signals are balanced, no filtering is required.

The rest of the regulator is a set point to obtain an error signal with respect to ground. The difference is supplied to the corona regulator tube.

We currently take a ripple correction signal off at the set point amplifier and feed this signal to the terminal stripper via an infra red lens system. The stripper regulator consists of a variable voltage supply connection between the foil or gas stripper and the terminal.

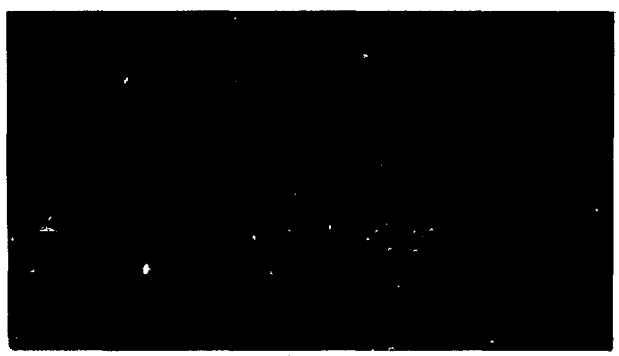


Figure 4

The regulator frequency response is shown in slide 4. The top trace is the mushroom signal; the lower trace is the AC component of the GVM adder amplifier.

Slide 5 shows the GVM signal now regulating on the GVM controller alone.

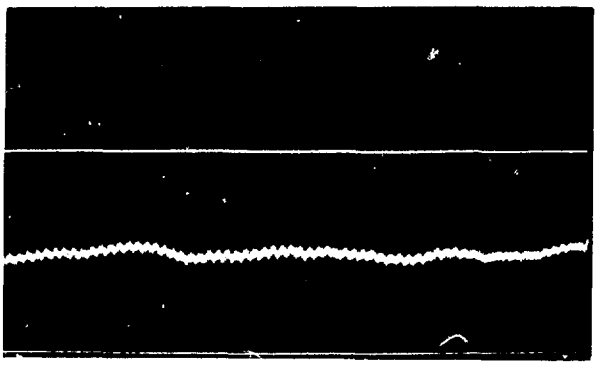


Figure 5

When operated from the GVM signal the terminal stripper regulator is not in a closed control loop. Our present readout system from the terminal does not provide high frequency response. We hope in the future to provide a suitable signal to add to the D.C. component of the GVM error, and thereby closing the loop. Currently the stripper correction is optimized by observing the beam on a scanner, or by minimizing the beam current jitter.

The regulator allows the experimenter to run with essentially no image slits, and therefore to utilize beams too small to provide the usual slit regulator control.

Beam jitter at the image position of the analyzing magnet is usually less than ±50 mils. This corresponds to an energy jitter less than 3.5 keV.

The GVM control is especially useful for heavy ion experiments. By switching to GVM control after spark conditions one is assured of returning to the correct change state.

The final version is now under construction. We hope to automate the GVM/slit control transfer, the transfer of control sensed on the slit current

sum set point, or spark conditions. A signal will also be generated to stop data acquisition during out of range time.

Experimenters typically run with the image slits at 100-100 and for hours will not observe any slit currents. The energy spread is 2 parts/thousand.

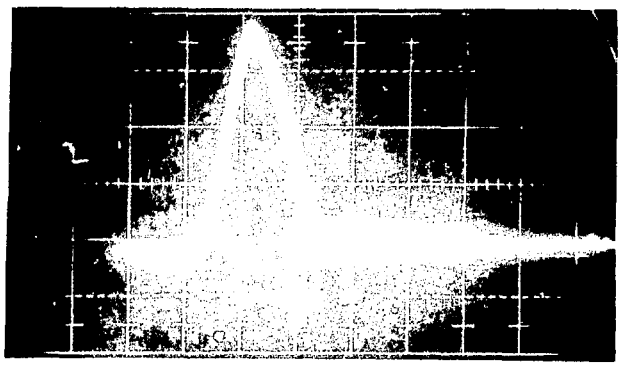


Figure 6

Slide 6 is a 1/2 second time exposure of the scanner beyond the image slit location.

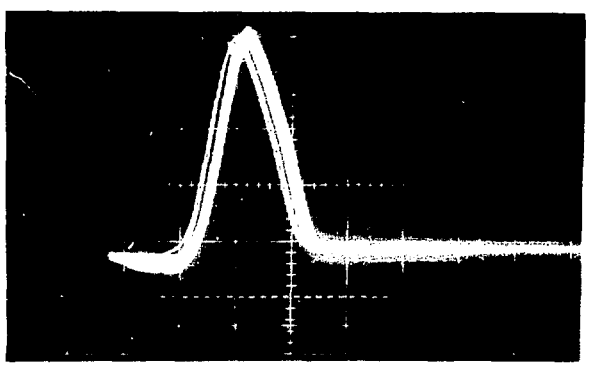


Figure 7

Slide 7 is a 1 second time exposure of the same scanner.

Both slide 6 and 7 are of a $^{12}C^{+4}$ beam.

The present circuit diagram is shown in slide 8.

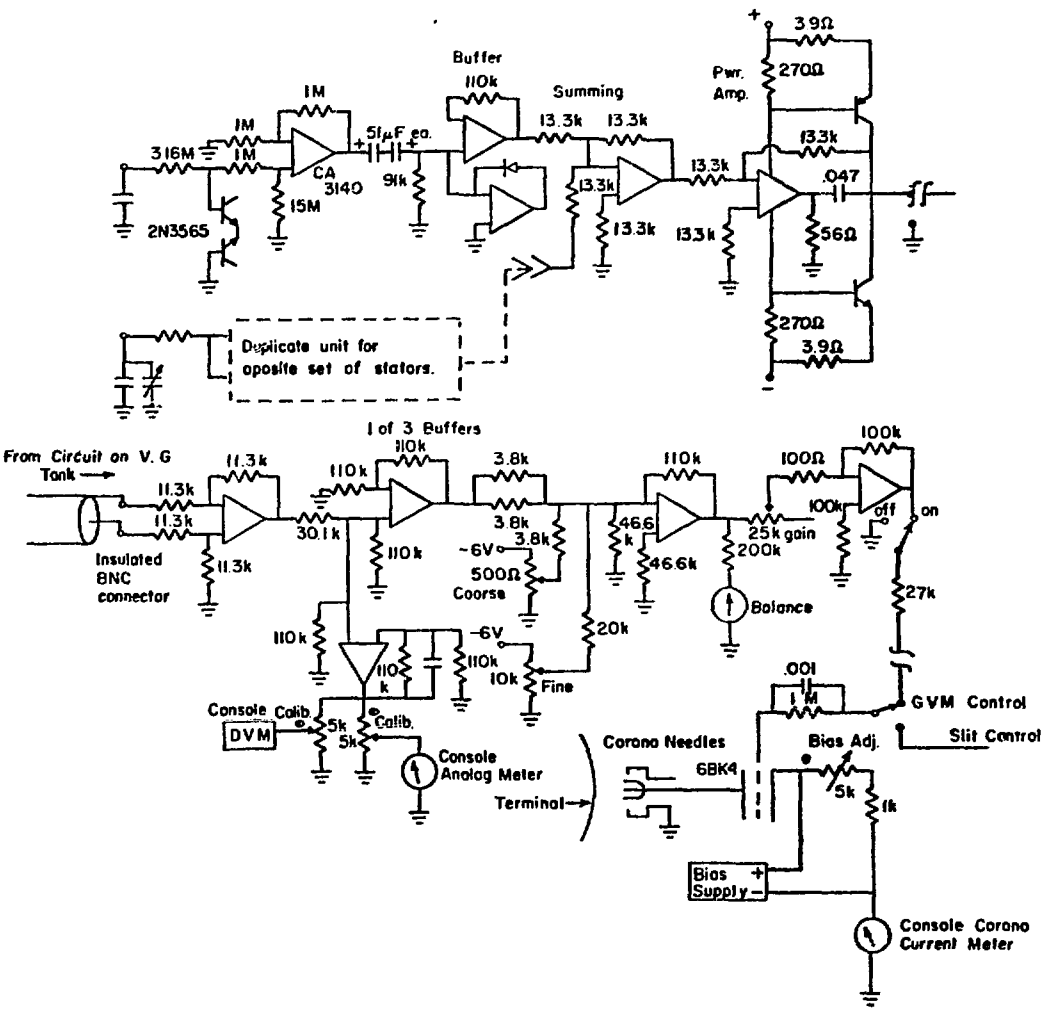

Buffer
110k
Summing
Pwr. Amp.
1M
316M
1M
1M
5fμF ea.
CA 3140
91k
15M
2N3565
13.3k
13.3k
13.3k
13.3k
13.3k
13.3k
39Ω
270Ω
.047
56Ω
270Ω
3.9Ω
Duplicate unit for opposite set of stators.
1 of 3 Buffers
From Circuit on V. G Tank
11.3k
11.3k
11.3k
Insulated BNC connector
110k
30.1k
110k
3.8k
3.8k
-6V
500Ω
Coarse
110k
3.8k
46.6k
46.6k
100Ω
100k
100k
off
on
25k gain
200k
Balance
27k
110k
110k
-6V
110k
10k
Fine
20k
.001
1M
GVM Control
Slit Control
Console Calib.
DVM
5k
5k
Calib.
Console Analog Meter
Corona Needles
6BK4
Terminal
Bias Adj.
5k
1k
Bias Supply
Console Corona Current Meter

Figure 8

Discussion:

Ratcliffe: Please describe in more detail your infrared light-link system between tank and terminal.

Fauska: First, I must say we had bad experiences using fiber optics. More recently we succeeded in making an infrared light-emitting diode and lens system operate. Some of the parameters of our system will be described by Robert Rathmell in the session on control systems. In the high voltage terminal of our FN tandem, we have installed a 6-kV power supply. Our stripper tube is electrically isolated from the terminal and tied to this power supply. The stripper tube normally operates at + 3 kV with its voltage modulated by a photo-diode amplifier circuit. This circuit requires a great deal of maintenance. Our experience has taught us that the terminal of our FN accelerator is a very hostile environment for any solid-state electronics and, for that matter, anything solid. While the terminal is perhaps the best place for energy modulation from the point of view of the beam, it for sure is the worst place from the point of view of my electronics.

McNaught: An alternative thing to do would be to modulate your target. Now that's not easy to do either. Have you thought about doing that?

Fauska: Yes, we have considered modulating the voltage of the target. We feel that it is probably easier than modulating the ion source voltage because of problems with the ion source inflection magnet and injection optics.

At this time, I would like to add one more thing which Laurie Miller has suggested to me. He visited us at Washington recently and noticed that we were using a generating voltmeter with aluminum stators. The choice of aluminum for the stator is a poor one because of the formation of aluminum oxide, an insulator, on the surface of the stator, which in turn affects the capacitance between the generating voltmeter and the terminal. This capacitance changes with time, with moisture, and with various other things. Laurie suggested that we plate the stator. However, we have not yet done this.

McKay: The McMaster tandem has a similar beam energy modulation system in its terminal. We use the slit signal to feed back to a photo-multiplier tube in the terminal which drives an operational amplifier. We do not use the GVM feedback mode.

Wegner: It would be nice if there were some way to independently calibrate a generating voltmeter which did not involve the use of beam from the machine. It sure would be nice to be able to push a button on the control console which produced an input signal to your generating voltmeter which corresponds to precisely 10 MV. There doesn't seem to be any way to do that with a conventional generating voltmeter. Is there any way to do this with yours?

Fauska: Well, I must confess I built a little generating voltmeter out of etched foil (the type of copper which is normally used for printed circuits). I ran it on the work bench and found that a 22.5-volt battery tied to an electrode mounted 1 cm away from this generating voltmeter gave me all the signal I wanted. I made my GVM the same size as the original High Voltage Engineering equipment. I suppose it's feasible to enter the tank and apply a known potential to a plate mounted in front of the generating voltmeter and calibrate the GVM using this potential. I have never done this. It certainly seems feasible.

Clegg: At Triangle Universities we have been running our tandem for approximately two years with an LED at ground potential driving a photo-multiplier mounted in the terminal which in turn drives the stripper tube. We run routinely this way. The research group which uses this system gets energy rms spreads of 450 eV at proton beam energies of 5 MeV. We have also used proton beams on carbon to look at the sharp resonance at 14 MeV. At 7 MV the tandem beam energy spread appears to be 650 eV.

REFINEMENTS TO THE BROOKHAVEN PELLETRON CHARGING SYSTEM AND GRADIENT CONTROL

P. Thieberger
Brookhaven National Laboratory

The installation and initial operation of our pelletron charging system were discussed at the last SNEAP Conference by Bob Lindgren. The charging system has continued to run very well for the last year. However, we felt it would be instructive to gain some more information about what is going on inside the machine while it is running. Namely, we would like to know the amount of charge being carried by each run of each chain, how uniform the charge is distributed, how much chain slippage is occurring, and what the velocity of each chain is. We designed a very simple system consisting of capacitively coupled pickup rings surrounding each run of each chain. Signals are brought from these pickup electrodes through the wall of the tank and to the control room. Figure 1 shows the down run of the chain. The chain is returning to the drive sheaves and is carrying charge down to ground potential. In the background can be seen the signal cables going to the wall of the tank. Figure 2 shows the up run. The chains are leaving the drive sheave and pass through the idler wheel assemblies, carrying charge to the terminal. The signal cables are clearly visible above the idler wheel assemblies. Figure 3 is an enlarged view of one idler wheel assembly. These assemblies support the chain, restrict the transverse motion of the chain, and remove transverse energy from the chain. To reduce the electrostatic field surrounding the chain, an electrostatic shield is installed by NEC to increase the capacitance to ground. We have supported our new pickup electrodes off this ground potential electrostatic shield by means of an insulated support block shown in Fig. 3. A 10-kΩ resistor was installed between the pickup electrode and ground to prevent the signal cable leads from attaining high DC voltages.

Figure 4 shows the pickup electrode signals for chains 1 and 2 up and down runs. Figure 5 is the same as Fig. 4 but with the time scale reduced a factor of two. By examining these figures, you can see that chain 2 has less current than chain 1 on its up run. However, on the down run chain 2 has more current than chain 1, and both chains have mechanical oscillations in them. This system has been useful for adjusting inductor electrodes while the tank is open. It has also helped us to find defective or damaged sections of chain. In one case, we had difficulty with the charge pickup wheel in the terminal which provides voltage to the induction electrodes. In that case, the amplitude of the charge on the down runs of those three chains went to zero. By triggering the oscilloscope on one of these signals, one can see relative slippage between chains as one signal walks by another. Differential slippage between chains turns out to always be a small fraction of 1%. Mechanical transverse oscillations of the chain can be greatly reduced by oiling the chain. In order to be able to determine the frequency of these signals to measure the velocity of the chain, one has to pass the signals through a band-pass filter. The band-pass filter is designed to get rid of the irregularities shown in Figs. 4 and 5 and to get rid of high frequency noise not shown in Figs. 4 and 5

but which, nevertheless, exists. Having passed the signal through a band-pass filter, one can then count using a frequency counter. What we observe is some slippage of the chain between no voltage on the terminal and full voltage on the terminal. The slippage turns out to be approximately 1%. Oiling the chain causes the frequency to go down by 7 or 8% for a few minutes, and it comes back to normal in about 10 or 15 minutes.

We have had resistor difficulties when running at 14 or close to 14 MV. The resistors change their value – both tube resistors and column resistors. We have done a few things to try to solve these problems. I think we have succeeded with regard to column resistors. The tube resistors are still giving some difficulty. Figure 6 shows three types of resistors used at Brookhaven. The top two are column resistors. The bottom one is a tube resistor. Figure 7 shows one end of the 20 element column resistor with our new inductive spring installed. Installation of this spring seems to have really solved our problems. The last several times we entered the machine the only resistors to go bad were those which did not have this new spring inductance. We feel that up to 14 MV the resistor configuration shown in Fig. 7 is satisfactory. However, we came up with another scheme which seems to be working also.

Figure 8 shows a cylindrically shielded resistor disassembled. These shields were originally purchased from General Ionics. They have a cylindrical spark gap. We have modified these resistors in such a way that we are able to use them close to the terminal, up to 13.5 to 14 MV. These cans now contain our old yellow RPC resistors. Figure 8 shows what we have done. There was a conical, solid connection leading from the spark gap to the resistor. We have replaced this conical connection by a ceramic insulator, and we wound a coil on it to provide sufficient induction to give protection. The hollow cylindrical shield which goes inside of the large shield provides sufficient capacitance to ground to give an effective voltage division between the inductor and this capacitor. We calculate that at 100 MHz the attenuation of this arrangement to be a factor of 100. We have had seven or eight such resistors installed in the accelerator. After a period of running, we removed these resistors and found that one had changed it value by 10%, while the others had not changed at all.

Finally, Fig. 10 shows a new way of making the column connections at the bottom of the column. It is the most satisfactory way we have found to make these connections. It is also convenient. A flat piece of metal is inserted under the right screw, which is tightened. One then slips the left end under the left screw and tightens without removing the screws. This strip carries the column current. We have had fewer column sparks since the installation of this metal strip.

Discussion:

Tesmer: How much additional inductance do your coils add to the inductance of the resistors themselves?

Thieberger: I don't have a number for that. However, the resistive impedance of the resistor is so much greater than the inductive impedance of the resistor that what really counts here is the voltage division between the external inductance and the capacitance which we have, so that no over-voltage can ever get to the resistor. The inductance of the resistor doesn't really help you at all. The inductance of the resistor allows voltage to develop which causes resistor damage.

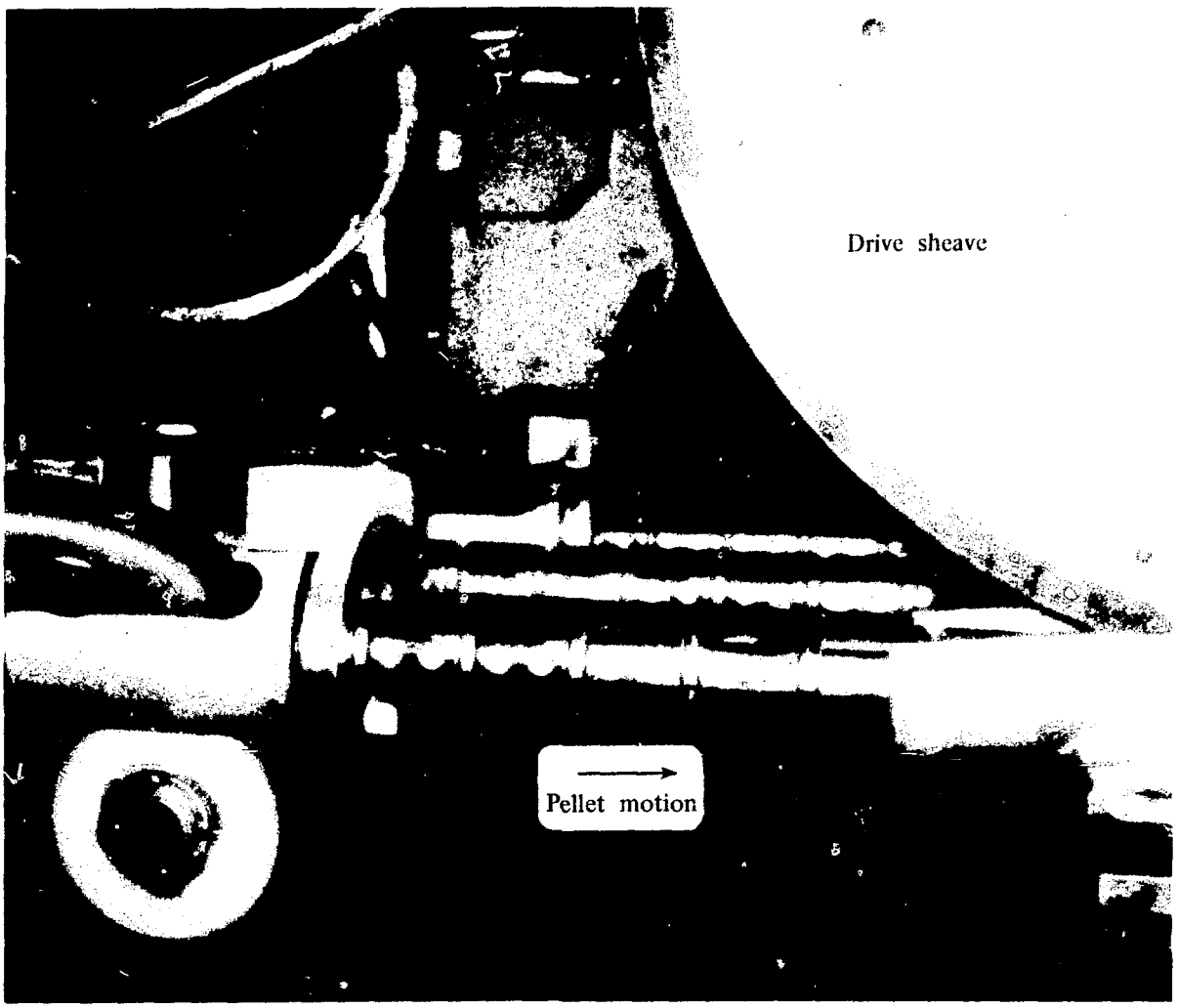


Fig. 1. The "down" run. Chain returning to the drive sheave carries charge down to ground potential.

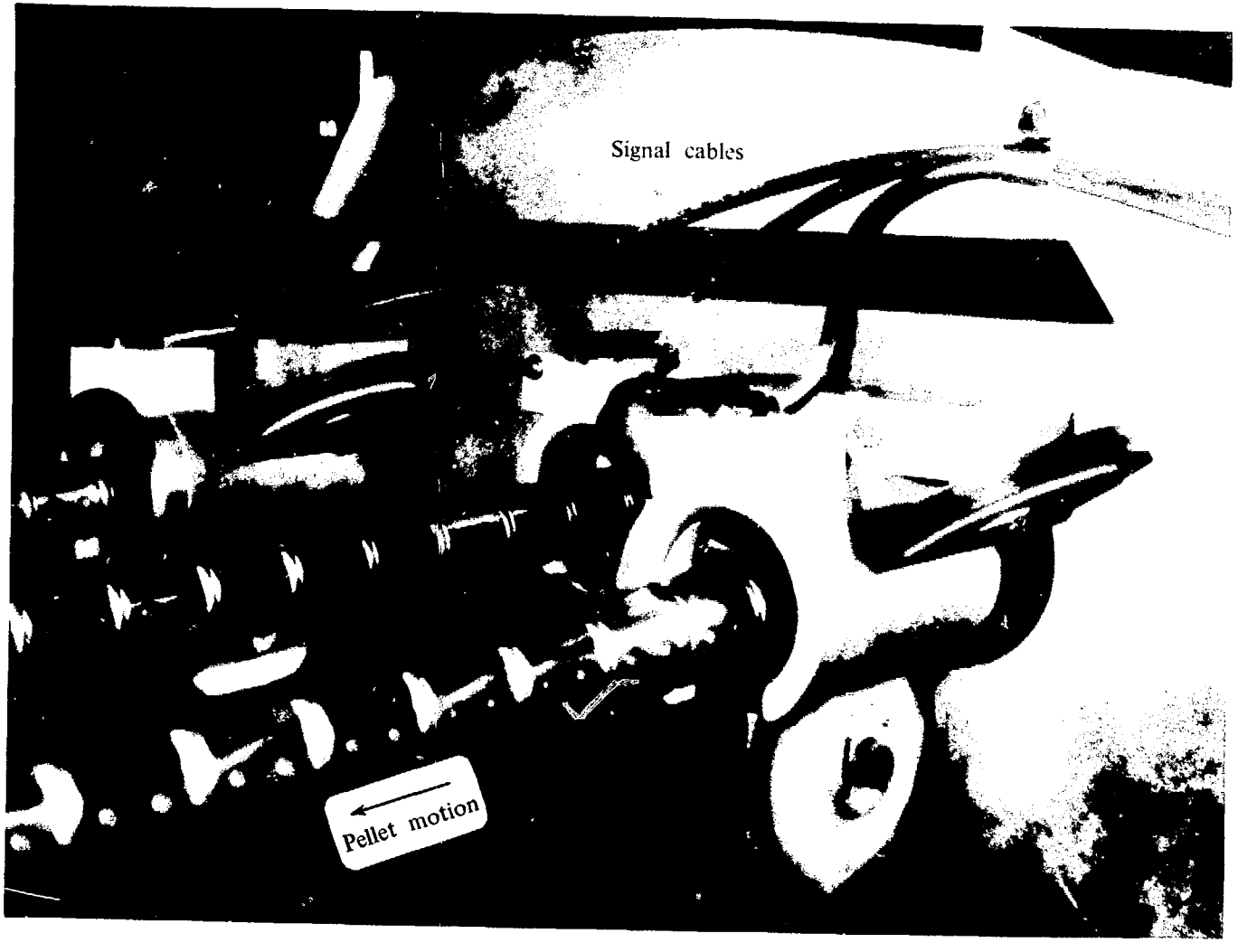


Fig. 2. The "up" run. Chains leaving the drive sheave pass through the idler wheel assemblies carrying charge up to the terminal.

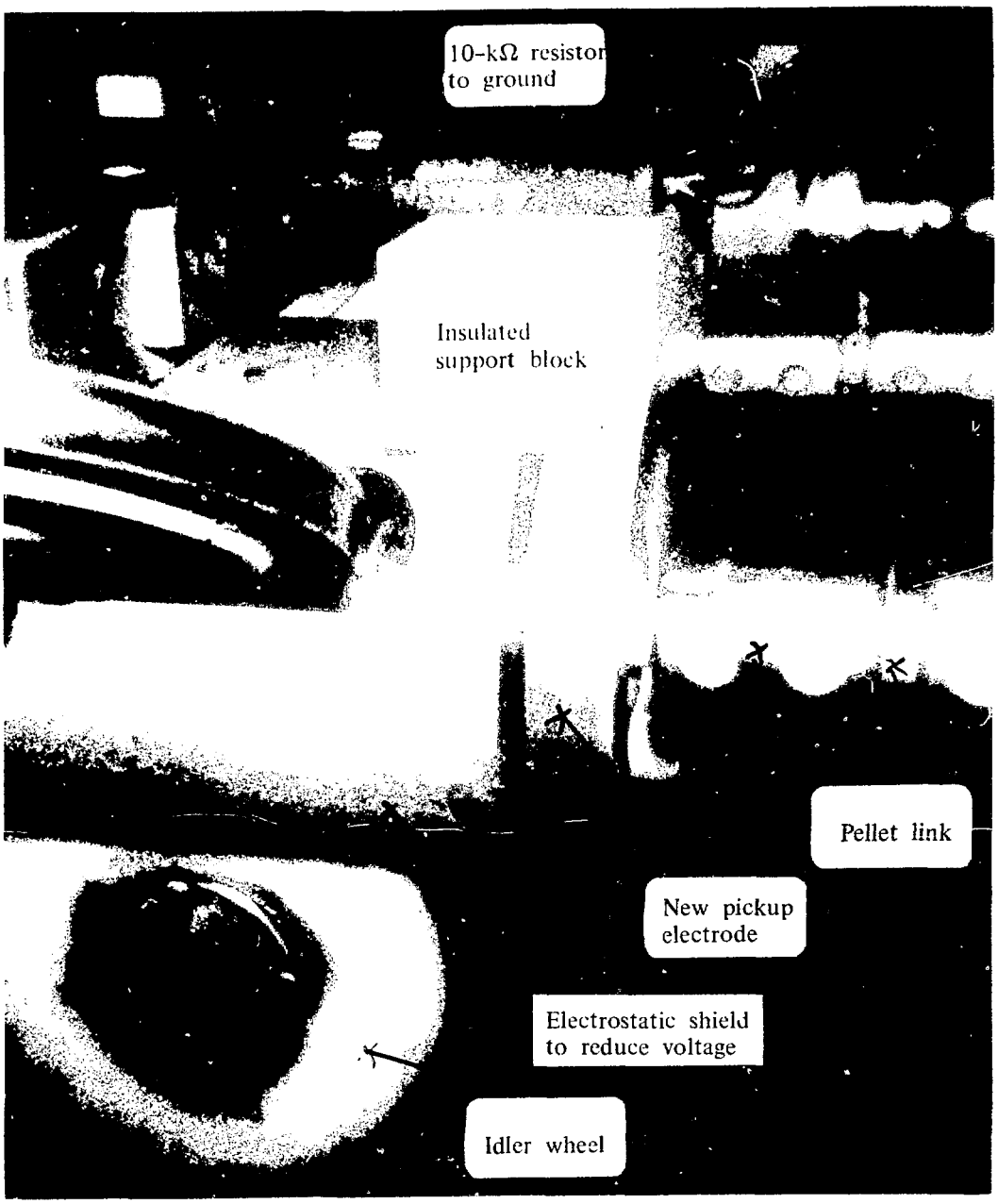


Fig. 3. One of 12 idler wheel assemblies.

Chain 1 up run

Chain 2 up run

Chain 1 down run

Chain 2 down run

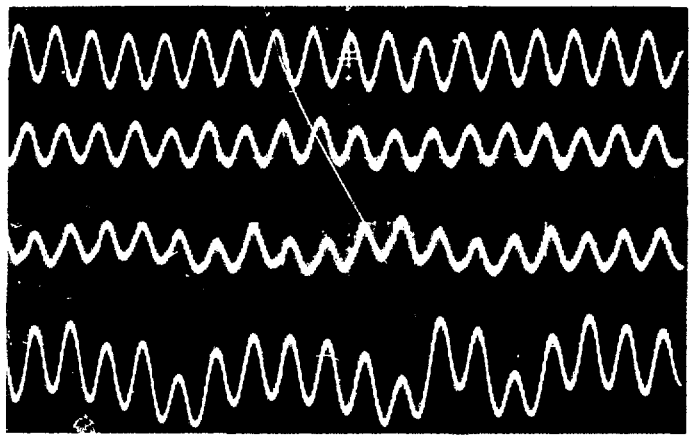
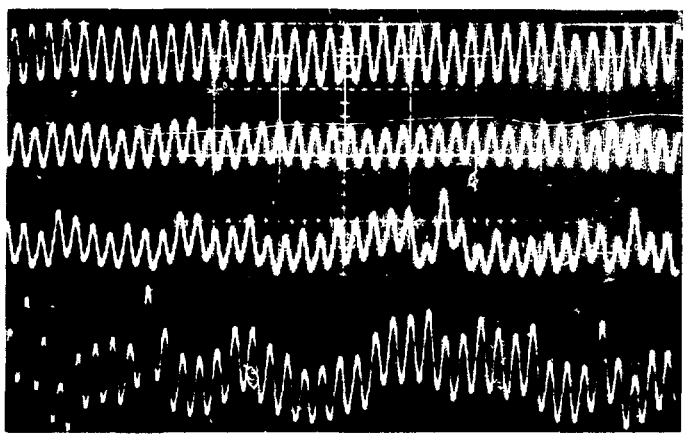


Fig. 4. Vertical scale: 50 millivolts/division
Horizontal scale: 5 milliseconds/division



Chain 1 up run

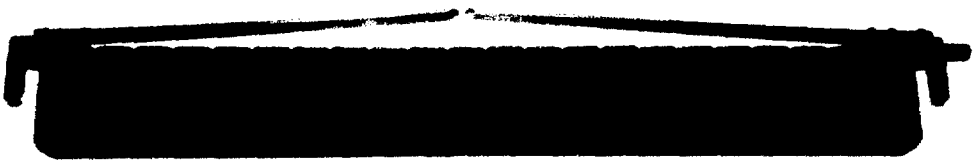
Chain 2 up run

Chain 1 down run

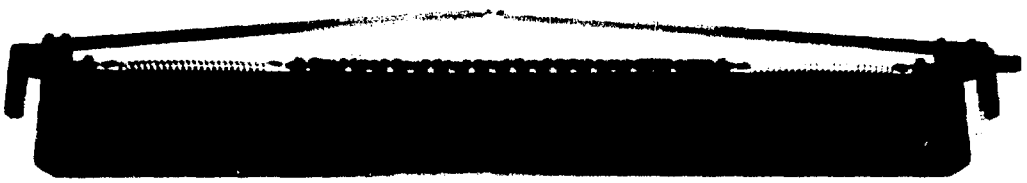
Chain 2 down run

Fig. 5. Vertical scale: 50 millivolts/division
Horizontal scale: 10 milliseconds/division

Forty-element column resistor



Twenty-element column resistor



Twenty-element tube resistor

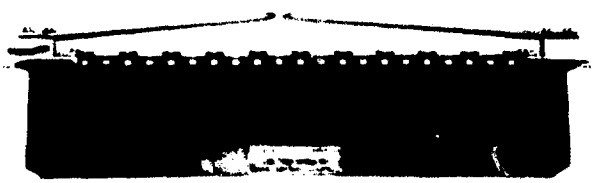


Fig. 6. Encapsulated resistors for the Brookhaven MP with inductance and spark gaps added.

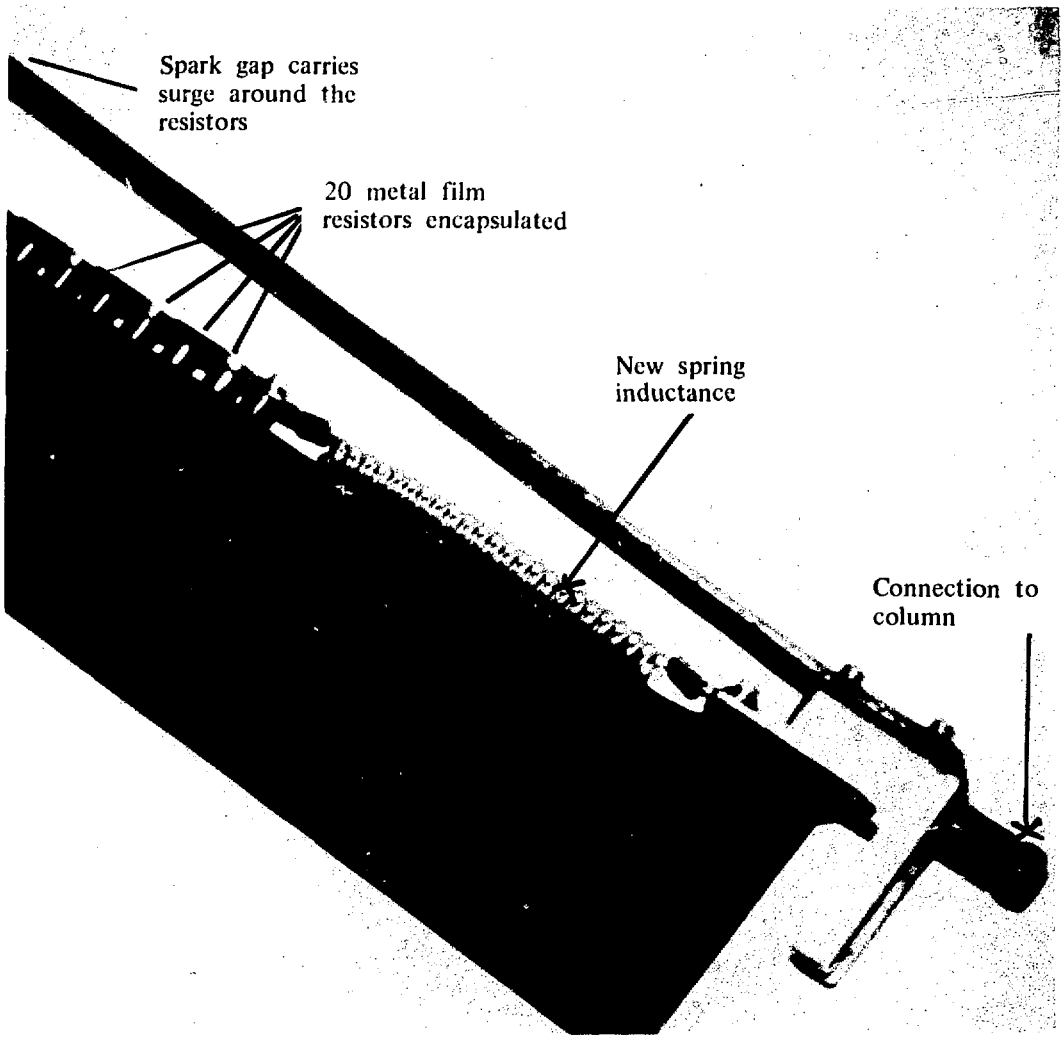


Fig. 7. One end of a twenty-element column resistor with the inductive spring installed.

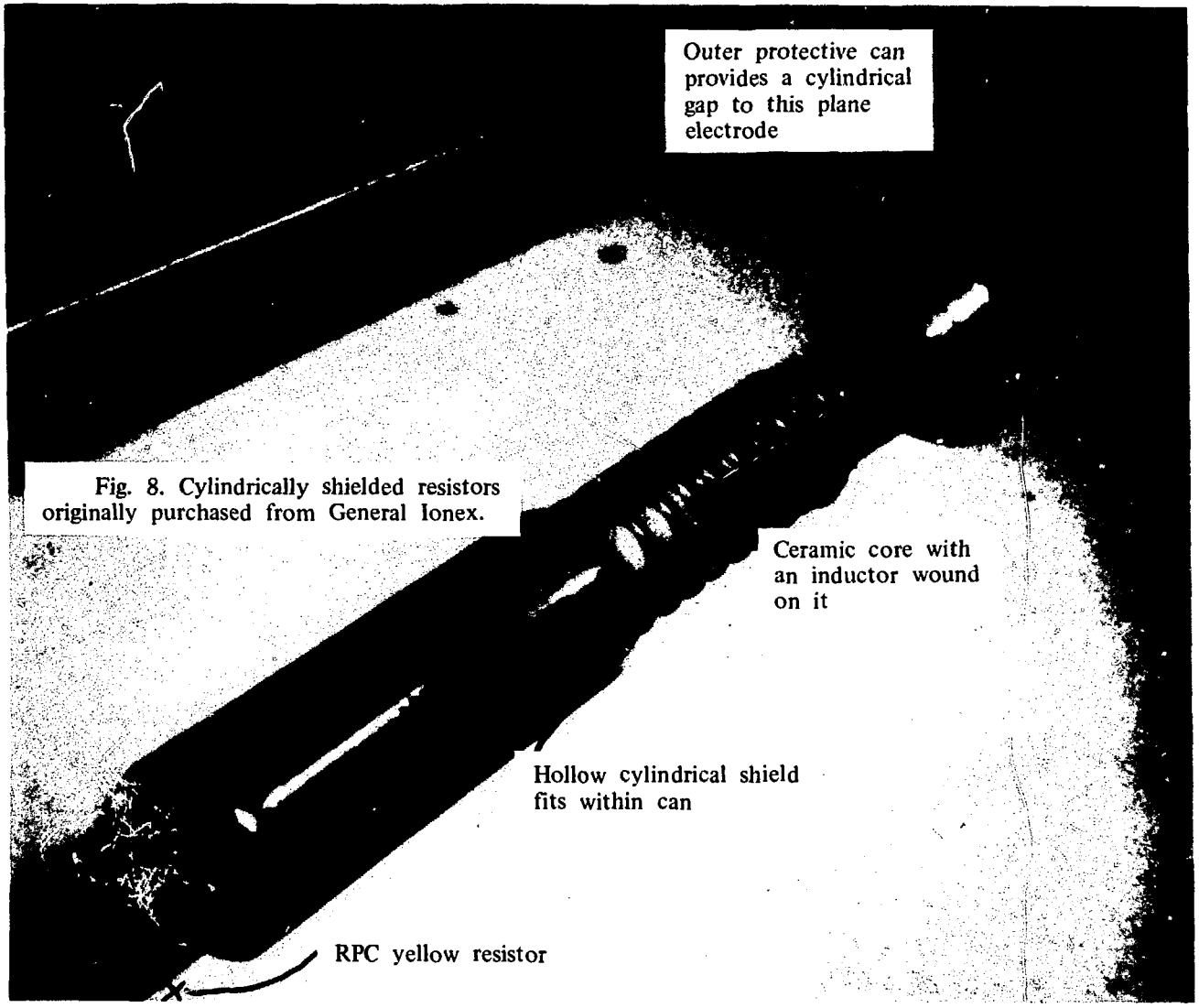


Fig. 8. Cylindrically shielded resistors originally purchased from General Ionex.

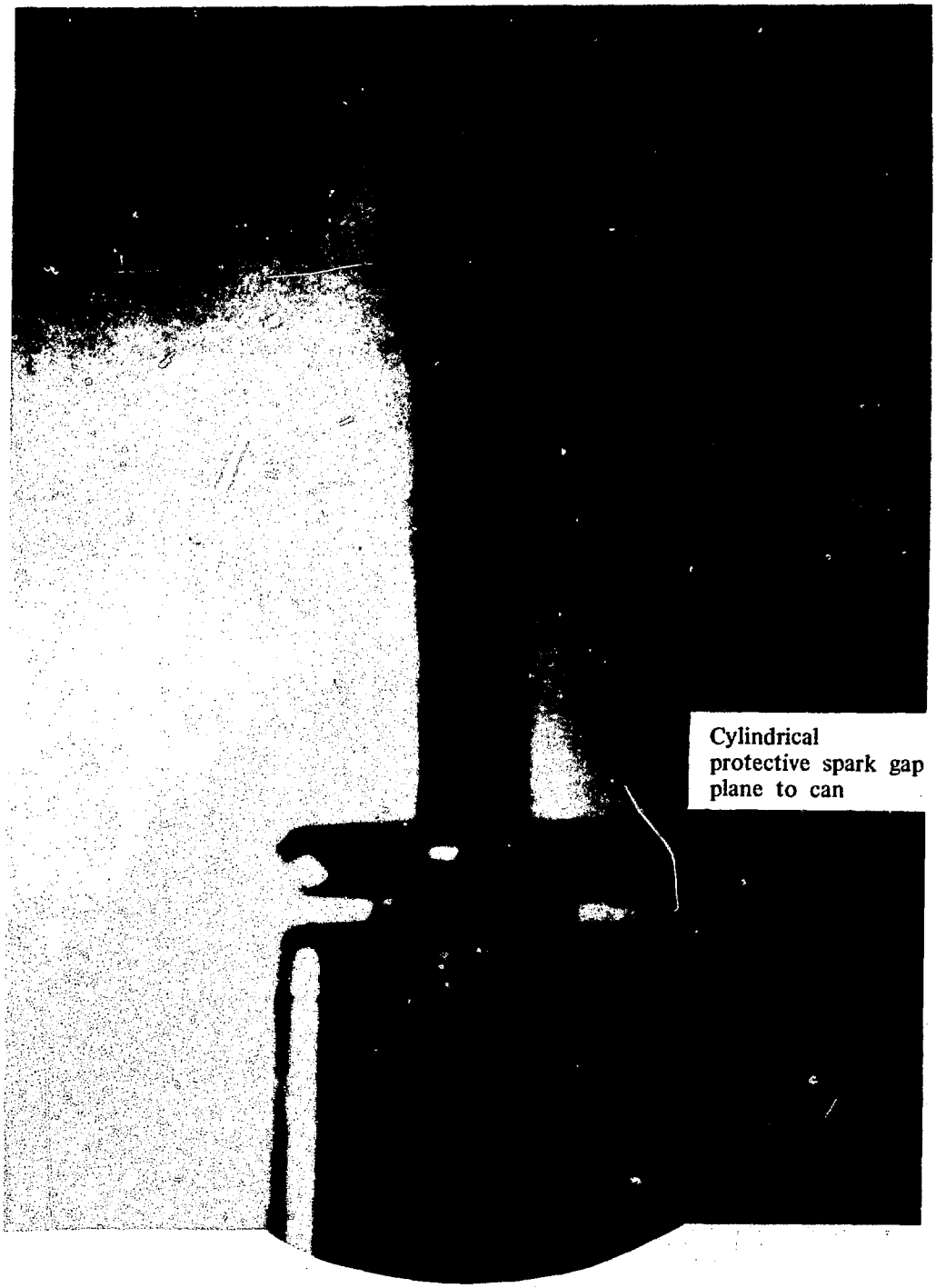


Fig. 9. RPC "yellow" resistor assembled in protective container.

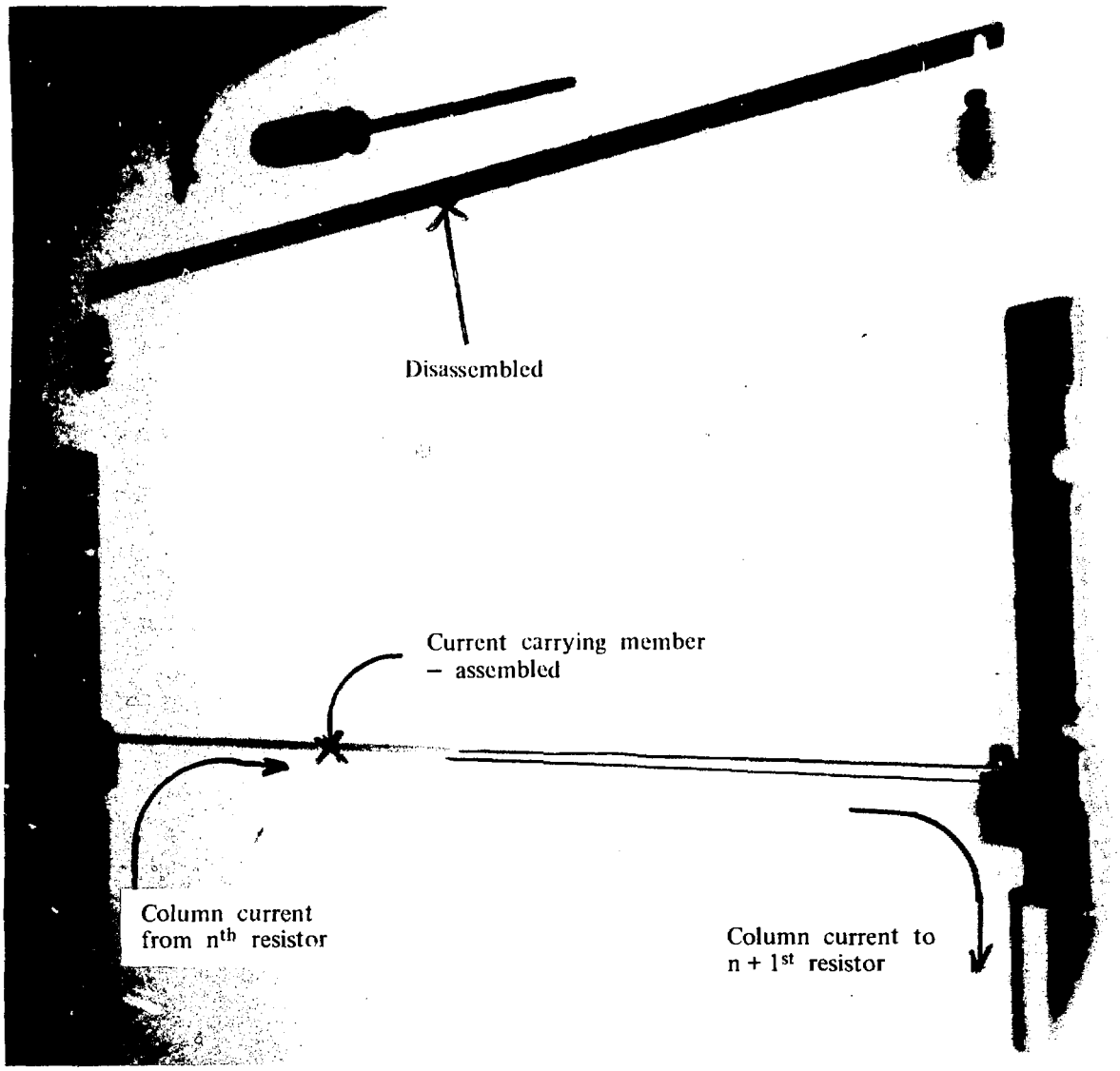


Fig. 10. The lower cross connection from left to right side of column.

GENERAL DISCUSSION ABOUT CHAINS AND RESISTORS

Led by J. W. McKay

McKay: I was looking through the minutes of last year's meeting, and I think that there were some reports which were made last year which people might like to bring up-to-date. Is there anyone here from Stony Brook? According to the minutes last year, a pelletron was about to be ordered, higher value resistors were planned, and many other interesting things were discussed.

Polvent: At Stony Brook our tandem is not progressing particularly rapidly. We are down at the moment. About two months ago, we blew our Norwalk compressor. We have been having a lot of difficulty getting it repaired. We still do have upgrading plans. We are going to go for a 400-keV open-air injector. We will be changing tubes in the next year. I don't know where we are on changing resistors. The after-burner is still at Cal Tech. They are still having problems plating. We still have plans to convert to either a pelletron or a laddertron charging system. We are divided as to which is the preferred charging system. To my knowledge, there are no FN tandems with laddertron charging systems. Is this true? Perhaps you are unaware that our whole operating staff has left, and we are in the process of revamping. Gene Schultz is no longer with us. We are rebuilding from the bottom up. I called my lab just a few minutes ago, and we are on the air with beam at the moment. We are having problems with oil contamination in our gas. Perhaps somebody here could help me with these kinds of problems. When the compressor blew, a person from Norwalk Compressor Company came down and pulled it apart. When he pulled it apart, he found large puddles of oil in the nonlubricated parts of the compressors – in the teflon-lined cylinders. This oil could be coming from our Kinney pump, or it could be coming from the main frame of the compressor. It appears that our gas is just saturated with oil. I spoke with High Voltage Engineering approximately one week ago. They told me that Argonne Labs had a similar problem. Is this true? Is anyone here from Argonne?

Billquist: At Argonne we did have a compressor failure. We destroyed rings and things and rods. However, our gas was not contaminated with oil.

McKay: I think either Neil Burn or Phil Ashbaugh might want to say something about oil in SF_6.

Burn: At Chalk River sometime ago, we had an identical problem. One of our operating people went into the tank after we had pumped it out and found literally a puddle of oil in the bottom of the tank, perhaps 10 or 20 gallons of oil. We wondered at that time whether the oil was from our oil-lubricated Norwalk compressor or from our Kinney pump. We did tests on the oil and discovered it was half compressor and half Kinney pump oil. We solved the compressor problems by getting a teflon-ringed compressor. The procedure previously had been to tie in the Kinney pump to the pump-out process as soon as the gas pressure in the tank reached 1 atm when evacuating SF_6. We changed our procedure. We now allow the compressor to go to 22 inches vacuum first, before turning on our Kinney pumps. We tried removing oil from the gas during these transfers. We used various types of filters – we used activated alumina; we used porous felt filters; we used centrifugal filters,

and other techniques. The most effective way to remove oil from SF_6, we finally found, was by adiabatic expansion of the gas, which usually occurs just as the gas enters the tank. The solution is to not allow oil into your gas in the first place.

Ashbaugh: At McMaster we have a system very similar to the Chalk River system. We started with a teflon-ringed compressor. We go to 15 inches or more vacuum with the compressor before turning on the Kinney pump, as we learned from Chalk River. I think it is fair to say that since our system was installed in 1967, we have observed no trace of oil in our accelerator. We installed a fairly large homemade gas/oil separator just after the Kinney pump from the beginning. It is a homemade baffle design, which is chilled. It is fairly effective at removing Kinney pump oil. We drain a few gallons of oil out of the bottom each time we pump down to vacuum. I would argue that it is certainly possible to design systems which do not allow oil through.

McKay: I would like to emphasize that McMaster designed its system in the few months just following the Chalk River problems with oil.

Levesque: At Bell Tel Laboratories we installed a K machine two years ago. In the beginning, we had a lot of trouble with the voltage holding capability of the machine. We suspected oil in our gas. We used 100% SF_6. We searched around for a method to scrub our gas. As it would happen, Allied Chemical is right across the street from us. We had their SF_6 experts come over. These experts explained to us that as long as SF_6 is in the gas phase, the solubility of oil in SF_6 is near zero. Apparently, the oil coming out of the Kinney pumps is not dissolved in the SF_6, but is rather in the form of vapor – small droplets. Therefore, the kinds of procedures you use with your Kinney pumps and the types of separators you install beyond the Kinney pumps, indeed, are very important.

McKay: I have a question. At McMaster we have just switched to higher valued column resistors. A logical next step for us at McMaster would be to purchase a laddertron or pelletron charging system. My question is this. In what ways are these charging systems better than the charging system we presently have? I understand that High Voltage Engineering Corporation is building a laddertron charging system for an FN tandem to be delivered to the University of Ife. What sort of improved performance can one expect when one installs a new charging system? How do you justify spending this much money for a new charging system? Let's assume that you have a smooth running belt-type machine at the moment. Why should you make this step to the newer technology? Bruce Thorburn, could you address this question?

Thorburn: The following comments will be completely unbiased. It is not clear to me that there are any significant advantages electrically. Perhaps, there is a little less terminal voltage ripple. The big advantage is mechanically. It is much easier to work with. It is extremely easy to remove and replace a link. The relative cost of repairing pelletrons or laddertrons vs. repairing belts becomes much more dramatic in the larger machines. In an MP, to change a belt requires man-days of time, while changing the link in a laddertron or pelletron requires just man-hours.

We have just completed our laddertron test. We ran a total of 5000 hours. The laddertron was tested in an MP, and it operated at 15 MV, 500 μA, and drove a terminal alternator loaded to 5 kW. We had no more time. We have now removed

that particular chain for installation on the MP tandem at Orsay. As far as I'm concerned, this chain is a real brute. We don't oil it at all. We have four idler pulleys in each dead section to support its weight. It has tremendous mechanical stability. It just does not move in the transverse direction. It moves smoothly in the longitudinal direction. Actually, it's rather quiet. You can stand at the drive motor end of the machine with the tank closed and carry on a conversation without any trouble. The console was located in that position. The tests were done with no acceleration tubes. The only problems were of a mechanical type. It took us awhile to get the inductor and suppressor plates into the right position. It took us awhile to determine how many power supplies we would need. As far as bearings are concerned, we had few problems. The idler pulleys are quite large, and so the bearing speeds are down. The bearing life is pretty good.

McKay: Would you care to estimate how those figures would change if you were to install the chain in an FN tandem? 12 MV? 400 μA? 2 kW?

Thorburn: A chain installed in an FN tandem will have a current rating of 250 to 300 μA. I don't think you will see any improvement in voltage holding capability by going to a chain. You will still have DC shifts in voltage as you do with the belt, which must be corrected by the Corona system. You will see a substantial reduction in AC ripple. Free running, it will be a few hundred volts, maybe.

Thieberger: At Brookhaven we achieved a factor of ten improvement in energy stability when we installed our pelletron charging system. This is for a free-running machine. But that does not tell you the whole story. The remaining fluctuations have a frequency spectrum which is quite different from the frequency spectrum which you have with a belt. The pelletron has fewer high frequency components, so it's easier to regulate terminal voltage, even in the GVM mode. We have run very weak beams where the slit signal was insufficient to regulate the terminal voltage. We were able to run successfully using GVM feedback, where we were not able to run several years ago when we had a belt system. Another advantage is the cleanliness inside the machine. There is little or no dust. That cuts down the amount of cleaning which you must do when you enter your machine. This results in a much better running machine. And then reliability: we could not run the machine beyond 12 MV without serious breakdown problems along our belt. Now with our pelletron system, we have run for substantial periods of time at 14 MV; however, that improvement may not be applicable to the FN tandem.

Chapman: I would like to ask Bruce Thorburn another question. Dr. Trump reported on the laddertron at the Ebeltoft conference. At that time, after about 2500 hours of running, there had been some problem with the joints effectively working loose. Trump reported that they had hoped that problem would be solved by tighter tolerances. There had initially been some trouble with lateral run out. That had been corrected by selective assembly. Has the selective assembly worked out? Has the tighter tolerances prevented the joints from working loose?

Thorburn: Yes.

Burn: I would like to comment on the lifetime of the pelletron at Chalk River. Our pelletron has a running time of 28,000 hours on it.

Session IV, Chairman, J. K. Bair
Editor, G. F. Wells

SAFETY IN THE OPERATION OF ELECTROSTATIC ACCELERATORS

by

N. Burn

Operations Division
Atomic Energy of Canada Ltd.
Chalk River Nuclear Laboratories
Chalk River, Ontario, Canada, K0J 1J0

Abstract

Safety in the operation of electrostatic accelerators is discussed in terms of analyzing the hazards and devising accident prevention techniques. Some practical guidelines are suggested. The usefulness of making a detailed safety analysis is stressed.

1. INTRODUCTION

In looking through back issues of the SNEAP Proceedings, I find that safety has not been a topic that is normally discussed at SNEAP meetings. However, I would assume that it is a topic that all of us have been, are and will continue to be concerned about. Probably all of us hold regular safety meetings and are visited by Building or Site Safety Committees as well as having to satisfy the requirements of State or Federal Safety Organizations. I myself have had the pleasure of being secretary of Atomic Energy of Canada's Accelerator Safety Committee for the past eight years.

In Canada, Accelerator Safety comes under the auspices of the Atomic Energy Control Board (AECB). Before an operating license can be issued for any accelerator facility, it is a requirement that the Owner of that facility must satisfy the AECB with respect to all aspects of safety involved in the operation of that facility. The Owner usually does this by preparing a document or submission in the form of a Safety Manual.

Safety is such a multi-facetted topic that in the next few minutes I can only briefly touch upon some of the areas that I hope can be brought up for general discussion later in this session. Radiation may be the major hazard in many accelerator facilities but around MP Tandems, for example, many other hazards pose a far more immediate threat of injury or death; these hazards include such things as high voltages, moving machinery, high pressures and toxic gases.

2. SAFETY SUBMISSIONS

I would like to start this session with a brief description of the Safety Manual that was prepared for the Chalk River MP Tandem Accelerator Facility by Peter Hurley and myself. This document was written as an internal report, CRNL-928, and in it we dealt not only with radiation hazards but also with non-radiation hazards in the following categories:-

1. Electrical hazards including high capacitances, high voltages and electrostatic potentials.

2. Toxic materials such as mercury vapour, ion source gases and ozone.

3. Mechanical hazards from rotating and moving machinery, cranes etc.

4. Fire and explosion caused by such things as oils and solvents.

5. Asphyxiation caused by nitrogen and sulphur hexafluoride.

6. High pressure from sources such as sulphur hexafluoride, bottled gases and evaporating liquid nitrogen.

7. Noise from moving machinery, escaping gas etc.

It is the purpose of a safety submission to demonstrate that every category of hazard has been considered, that detailed operating procedures are in effect and that adequate interlocks, warning devices and protective systems are installed. In addition it is necessary to examine the facility as a whole and to consider and analyze any accident which might "credibly" happen perhaps as a result of a combination of certain situations, even though the possibility of such an event may be very remote.

When we first started to write the submission, I myself considered it a little unnecessary, assuming that I had already thought of all the things that could go wrong. Having prepared the submission, however, I was forced to admit that there were several areas that I had overlooked. In short, I found it a very worthwhile exercise. I am not suggesting that by writing such a document we will think of and thereby eliminate every possible accident, but we can perhaps avoid many of them.

3. ACCIDENT PREVENTION TECHNIQUES

Granted then that we have prepared a submission, that we have thought about certain situations and looked at all of the "credible" accidents, what measures can we take to try to prevent them? We can use administrative procedures, we can use electronic, electrical, mechanical or electromechanical devices or we can use some combination of both. However, the complexity of the procedures that we use and the duplication of the devices should take account of three considerations; first, the likelihood of an accident happening, second, the severity of the consequences of that accident and third, the reliability of the devices being used. Later in this session I hope we can hear the opinions of other people as to whether we should rely on devices, procedures or a combination of the two.

3.1 Devices

I would like to consider first of all the sensors, transducers, transmitters, lights, alarms and other such devices that might be used. If reliance is to be placed on these devices, it is imperative that this reliance be accompanied by regular testing at some interval that is less than the expected mean time to failure of the devices. The most comprehensive test of a complete safety system is to put a real signal into the sensor and observe whether the correct output signals are produced. For example, the device that continuously monitors the oxygen content of the air in the accelerator room at Chalk River is tested on a weekly basis. Nitrogen gas is introduced into the sensor intake and verification is made that the area evacuation alarms start to sound. This procedure tests not only the pump that transports the gas sample to the monitor but also the integrity of the gas transfer line, the system response time and the output alarm signal. It is perhaps worthwhile considering that warning devices serve three distinct functions insofar as safety is concerned:-

1. To alert personnel to hazards unexpectedly arising in certain areas as a result of changes in the normally prevailing conditions, these changes not necessarily being related to operations that are deliberately being undertaken (e.g. toxic gas leakage from a storage vessel).

2. To alert personnel to hazards continuously or intermittently existing in certain areas (e.g. radiation from a fixed source).

3. To alert personnel to hazardous situations that might arise directly as a result of operating procedures (e.g. high pressure in a pipe during transfer operations).

The first two functions require devices that will operate continuou ly to monitor ambient conditions while the last requires devices that need only operate reliably for relatively short periods.

In reference to visible or audible alarm signals, we must also remember that although these signals should be recognized by the people who work in the laboratory, they may not be recognized by visitors unless we have made sure that the system has been demonstrated to them.

3.2 Procedures

Next, I would like to discuss the question of procedures and the operators who use them. If reliance is to be placed on operators, they must first of all be adequately trained and they must have proper written procedures to follow. However, speaking from personal experience, I find that it is not enough to have written procedures alone; operators may decide that they know a procedure well enough without needing to refer to it, especially if they themselves feel that they are well trained and experienced. On the other hand, they may be diligently following a procedure and be interrupted; one shift may end and the next crew may be unsure of where the previous crew left off.

An administrative way of ensuring adherence to a procedure is by the use of a check-list since it seems that, psychologically, if an operator has to check and initial that he has done a job, he is much more likely to have actually done that job. In addition, if an operator is interrupted or a new crew takes over, it is immediately obvious from the check-list what stage the job was at. A simple check-list can be backed up by including interlock devices at certain stages to ensure, for example, area searches.

At Chalk River we have a series of operating notes called Van de Graaff Instructions. These are not all necessarily aimed at safety but many of them contain check-lists. The check-lists referring to evacuation and pressurizing of the accelerator tank include such items as checking door retaining pins, but I sometimes wonder what may have been omitted!

4. SUMMARY

I do not want to make this a gloom and doom session, but when I walk around any facility, I become apprehensive thinking of the near-accidents that must occur from time to time. I can only imagine that as the size of facilities and the number of personnel increases, then the possibilities of accidents must also increase. The Owners of these large facilities must be even more vigilant because of additional hazards that are not a problem in most facilities. In large vertical machines, for example, there is the effect of gravity and the danger of falling objects.

In conclusion, I would like to stress that I am not claiming to have a super safe facility at Chalk River, neither am I dogmatically laying down rules for safety. I am simply passing on some ideas and approaches that I have found useful. If I make any specific recommendation, it is simply for those of us who have not yet made a conscious effort to write a comprehensive safety manual, to do so; I think they will find it a worthwhile exercise.

I would like to suggest that this session now continues with a general discussion on some of the points I have mentioned, perhaps beginning with views on accident prevention; devices versus people.

Discussion:

McKay:

This is not so much a question perhaps for Neil as for other people. Neil has pointed out the various boards and groups that he must reach for approval. Now we have to be cleared at McMaster by AEC-B. But in terms of general operating, we've had some discussions recently that are a little bit frightening. We are relatively free of external interference or regulations in that we are not subject to industrial safety laws, and so on, yet. I always thought this was good, until we had some recent discussions where some of the professors were saying, "Well, we're all reasonable people. We don't need a bunch of rules. We just will act reasonably and responsibly." Now, I was wondering about other people in this sort of a university setup where the regulations are less severe. If you've run into similar problems, how do you handle this question of people who don't see the problem?

Burn:

I will just take that as a comment because we are fairly strictly enforced at Chalk River.

Weitkamp:

Let me add a bit to that question — another clause. And that is, how do you pry loose funds and technician time to put safety devices and safety programs into effect when this directly has to come from research budgets?

Burn:

I don't know the answer to that.

Broadhurst:

I would like to make another inquiry which is particular to universities. How do you have safety when the senior people are the experimenters and the junior people are the people running the accelerator? I think that's a problem that we've always had in our lifetime.

Burn:

I'd just like to comment on the question of where do you get the funds for safety devices. OK, the thing that you have to balance there is what is it worth to either prevent a disabling injury or to save someone's life? One of the devices that we have inside the Chalk River accelerator - this is in case someone was not found during the last closeup. You hear the big door clang and you hear the kinney pump going and you wonder what you're going to jam against the 10 inch aperture to stop it from sucking the air out of there and — well, you can use your imagination. One of the devices we have is a large bung that is hanging in the end of the tank. OK, how do you then alert people that you're inside the tank? There are various buttons inside the tank which are interlocked to the kinny pump and sound klaxons, etc. Say the power's gone off and the klaxon's burned out since it was last tested. (This is where the testing procedure comes in.) One of the devices we have inside the tank is a large sledge hammer which allows you not only to pound on the tank and deafen yourself but also to break the accelerator tube to gain access to fresh air in the machine. You have to look at these devices. Some of them can be relatively simple — the bung and the hammer. But granted very often people can get carried away with safety and you can literally get "safety-committeed" out of your mind. We have about 6 or 7 that run around Chalk River, and sometimes I curse them. But I think you have to try and reach a happy balance between the two.

Bair:

I've a question for you. Do you have associated with your request for accelerator time any sort of safety review of that particular experiment? We have found this very profitable here.

(no response)

Bair:

Any more questions? I presume no one here has a safety review associated with the experiment.

McKay:

I want to ask just what does that review consist of? What do you look at in an experiment?

Bair:

Radiation safety, general safety including fire hazards, etc. Now this is not as time consuming as you might think because in many cases the experimenter says in answer to the question, "What are you

going to be doing?" He says, "The same sort of work with the ORTEC scattering chamber that we were doing last time using essentially the same sorts of targets." So the review can be very trivial in that case. On the other hand, you may have a visitor who comes in and part of his experiment is a 5 ft. diameter thin glass sphere of ether — you would like to catch this before he moves it into your laboratory.

Weitkamp:

At the University of Washington the scheduling committee is the safety committee. That works out very well because they're in the best position to ask questions and make sure the questions get answered before the experiment is done.

Bair:

Yes.

Connally:

We have a Health Physics group in our nuclear center and we work very closely with them. They coordinate the safety of the accelerator and the reactor and they are sort of the overseers of radiation safety. Safety as to mechanical things in the laboratory, we take care of ourselves. But all the radiation safety for the whole nuclear center is under these people. This helps us in a way to work with them and if we have any trouble with faculty we have someone to refer them to.

Schow:

We have an ion implantation accelerator and I wonder if any of you have begun to worry about the impact of TOSCA. Those of you in the U.S. might be aware of it. The Toxic Substances Control Act. It's sort of unusual in that it is my understanding of it that a person that pollutes can be physically incarcerated for pollution. Like, lets say we use arsine in the ion source to produce arsenic ions for implantation. If I release large quantities of arsine to the environment supposedly I would be incarcerated for it. Has anybody begun to worry about releasing arsine, phosphine or whatever else you may use as a gas for your ion source?

Bair:

With that sort of a penalty, we will be.

Session V, Chairman, K. R. Chapman
Editor, G. D. Alton

Recent Ion Source Development at the University of Pennsylvania*

R. Middleton

Physics Department, University of Pennsylvania

I would like to begin by giving a very brief historical review of the development of the cesium sputter negative ion source. I hope that not only will this be of some general interest but will demonstrate that source development has recently taken a new turn. Whereas a few years ago it appeared that source development was diverging it is my impression that it is now converging towards a simple, high efficiency device that will produce about an order of magnitude more current than the early sputter sources and with improved emittance.

Undoubtedly the starting point was the discovery by Krohn[1]) that when a surface was sputtered by Cs^+ ions a surprisingly large fraction of the sputtered particles were negative ions. Krohn further showed that this fraction could be significantly increased by overlaying the sputter surface with neutral cesium. A schematic diagram of the apparatus used by Krohn is shown in fig. 1.

The first source to be built embodying Krohn's discovery was the 'rotating wheel' source developed by Hortig & Mueller[2]) – a sketch depicting the principle of this is shown in fig. 2. The negative ion currents from this source were impressively large but the emittance was poor – largely as a result of the large sputter area (~ 5 by 20 mm).

At the SNEAP conference held in 1972 at Tallahassee the

* Work supported by the National Science Foundation.

author[3]) suggested building a source similar to that shown in fig. 3. Later that year two such sources were built and tested at Rochester & Penn and the results were quite encouraging.

While working with this source it occurred to the author[4,5]) that the duoplasmatron could be replaced with a simple surface ionization source and the lithium vapor canal dispensed with. A source similar to that shown in fig. 4 was subsequently built and the results are well-known. Although this source had some very desirable features which led to its widespread adoption in many tandem labs. it had some limitations and disadvantages and it is on these that I wish to concentrate. The emittance was relatively large, frequently the beam intensity distribution exhibited a ring structure and the yield of elements that sputtered readily (e.g. Cu) was disappointingly low. It is now well established that the last mentioned is due to inadequate cesium coverage of the sputter surface.

The first Cs^+ beam sputter source built at Penn did not have an accel.–decel. electrode system. This lead to the accidental discovery that a significantly large fraction of the negative ions were being channeled back along the positive cesium ion column and were remarkably well focussed onto the porous tungsten ionizer – much to the detriment of the latter! This suggested a new source configuration and the so-called inverted sputter source was born[6,7]). A sketch of this source, which has been most actively pursued by Chapman, is shown in fig. 5. Potentially, the largest advantage of this arrangement is that the beam comes from a 'plane' surface and should not exhibit the ring structure associated with a cone.

Several improvements have been made to the Cs^+ sputter source as introduced in 1973. Particularly noteworthy was the introduction of an einzel lens by Hyder et al.[8]) to focus the Cs^+ beam. The latter permits the use of smaller aperture cones and generally improves the brightness and emittance. A later ingenious development was made by Purser[9]) who sectored the central element of the einzel lens enabling it to be used to both focus and steer the Cs^+ beam.

The introduction of the sectored lens spawned yet another development – the so-called 'reflected beam source' – which has been most actively pursued by Brand[10,11]) at Bochum. A sketch of the configuration used by Brand is shown in fig. 7. It will be noticed that in the latest configuration that the sputter target pill is recessed in an attempt to increase the neutral cesium coverage.

Fig. 8 shows a highly schematic drawing of a very interesting Cs^+ sputter source that was developed in Russia[12]). This source not only has improved emittance but delivers much higher currents of the elements that sputter readily than the UNIS source. Although I was slow in realizing why, the reasons lie in the use of a relatively inefficient ionizer and a low sputter energy ($\sim$ 1 to 2.5 keV). Both factors enhance the neutral cesium coverage of the sputter surface and it is interesting to note, that to some extent, cesium is recirculated.

Paralleling the development of the Cs^+ beam sputter source important new negative ion source concepts were being developed at the Universities of Aarhus[13]) and Wisconsin[14]). Both groups

used a PIG discharge to generate a cesium plasma and the sources differed only in the arrangement of the sputter cathodes. The Aarhus group introduced a third sputter cathode, biased somewhat more negatively than the conventional cathodes, as shown in fig. 9. The negative ion output of the latter is very significantly better than that of a UNIS type source for elements that sputter readily.

Earlier this year while debating whether to build an Aarhus type source – but forever being deterred by an inherent dislike for PIG sources – it occurred to the author that it might be possible to build a conceptually similar source where the cesium plasma is produced by surface ionization. Fig. 10 shows a drawing of the first source that we built along these lines. The source embodies many of the features of the Aarhus source – same spherical cathode centered on the exit aperture and a similar extract system. The important difference was that the cesium 'plasma' was generated by a sheet tungsten annulus located slightly behind and out of the line-of-sight of the cathode. The tungsten annulus was joule heated to about 1100°C. It was anticipated that once Cs vapor was introduced into the chamber that the tungsten ionizer would produce and fill the chamber with a Cs^+ plasma – the latter would then form a boundary close to and contouring the spherical face of the cathode. Negative ions would then be formed uniformly over the face of the cathode as a result of Cs^+ ions being accelerated across the plasma boundary as in the Aarhus source and then accelerated and focussed by the radial electric field towards the exit aperture.

The first experiments were conducted with a copper cathode and the results were very encouraging. $^{63}Cu^-$ currents of up to 60 μA were observed after 90° analysis in our ion source test facility. Typical parameters were $V_c \sim -5$ kV, $i_c \sim 5 - 10$ mA and the extraction voltage was 20 kV. Later examination of the cathode produced a surprise — the surface was not uniformly sputtered but sputtering was confined to a small central spot of about 1 to 1.5 mm diam. It became very apparent that the mode of operation of the source was quite different from the Aarhus one — and more closely resembled that of the reflected beam source. However, the yield was much larger than that of the latter due to a higher presence of neutral cesium and the lower sputter energy. Stable operation of this source over prolonged periods proved difficult due to imprecise metering of the flow of cesium vapor. This difficulty has since been overcome by the use of a heated needle valve.

Fig. 11 shows a drawing of the second source of this type that we built and tested. This was basically similar to the first with the exception that a cylindrical ionizer was employed located between the cathode and the extraction aperture. The change was prompted in part because of the realization that the operating principle was more of a reflected beam nature than that of the Aarhus source and we had just learned of the source being developed by Richards and collaborators[15] at Wisconsin (described in the following talk by J. H. Billen). Rhenium was chosen as an ionizer material because, unlike tungsten, it retains its ductibility after heating.

The latest source that we have built and the one that we have most extensively tested is shown in fig. 12. A few changes have been made but since these are apparent from the figure they will not be discussed. Several elements have been tried in this source and the table lists some of the negative ion currents that we have obtained. Most of these were obtained with a cathode voltage of about -3 kV and current of 1 to 4 mA – the extraction voltage was 20 kV. Most currents are large and frequently about an order of magnitude larger than obtainable from the UNIS source. We have made no emittance measurements but since the current usually comes from about a 1 mm or less diameter spot are fairly confident that it is good. Recently we injected 41.5 μA of H^- ions into our FN tandem – the terminal voltage was 6 MV and carbon foil stripping was used. The current in the high energy cup was 37 μA, corresponding to close to 100% transmission after allowance is made for a 10% loss in the tube grid lens. Loading on the accelerator was very nominal and suggested that a considerably more intense beam could be accelerated. Due to the high radiation level very little time was spent in optimizing the analyzed beam (with protons this is usually about 10% less than in the high energy cup) – however, 28 μA was analyzed.

An interesting feature of the source is the cleanliness of the negative ion spectrum. This is well illustrated by fig. 13 which shows the spectrum of silver. Here the scale is 100 μA with the exception of a narrow region bracketing the two silver isotope peaks where it is 10 μA. The low level of impurity

peaks presumably reflects the good vacuum in the source head.

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DISCUSSION:

Purser: I would like to make one or two comments. First of all, the very obvious comment that when one got into this game almost 20 years ago now, a fraction of a microampere was an outstanding achievement, and that was for protons. I think the second point, I would like to make, and maybe Roy could comment on this in more depth, is where much of this technology is going. One thing that one sees now is a great interest in using tandems in a different modes and one of the modes which is receiving a lot of attention is the application of small ion beams. It seems to me that cesium has unique capabilities for producing beams which are down in the hundred amstroms or so size. This is because of the low energy spread. This is a point which is being pointed out by Lebol of Germany. I wonder if you could make some comments about some of these features of small beams.

Middleton: Well I think you are opening a sort of new area. I think from the nuclear point of view the interest is sort of minimum. But, certainly, from surface analyses, microprobe and so on, I think there is tremendous application. As you rightly point out, the energy spread of the cesium beam from the surface ionization source is incredibly low which enables it to be focused to very very small spots and therefore one can make scanning devices for studying surfaces. Certainly, I think that this technology has incredible implication other than in the area of interest of this conference which is mainly in nuclear physics. It is a big subject, and I could talk for a half hour on this easily but I think it would be inappropriate to spend more time on it here.

Kutchera: Roy, have you tried any of the more difficult beams with this source like the alkaline earths?

Middleton: No. There is one element that we have tried and I don't fully understand. We have very, very poor results. We've only had poor results with one element, naturally, that's the one I didn't tell you about and that was lead. We did try with a lead cathode and for some reason got current very little better than we got from the original sputter source. I really don't understand it. Operating these sources is a little bit tricky, we had to learn how to do it. It may have been that we didn't operate the source correctly but certainly, I think emphasis has to go on to heavier elements, rare earths, in particular. For reasons which totally escape me, heavy elements which (a) on the whole sputter fairly readily and (b) often have fairly high electron affilities are very difficult. I don't know why. In fact, I'd be very interested if anybody has any comment on this. There are a few exceptions, platinum and gold, which are very, very easy to form as negative ions but a lot of elements, particularly elements such as tin and lead, which have very respectable electron affinities, sputter regularly, but are difficult in terms of their negative ion generation capabilities.

Alton: I would like to ask how you generated your titanium hydride beam?

Middleton: That was just using a small pill of titanium that we had loaded with hydrogen. I don't know precisely the amount of hydrogen we put in that titanium but I suspect that it will be roughly one-to-one. I mean no hydrogen gas was admitted into the source, it was purely hydrogen absorbed in titanium.

Alton: There may be an obvious advantage of having the so-called P.I.G. discharge in this situation where one can introduce the molecular gas, ionize it in the discharge, ion implant into the titanium cathode and back sputter from the cathode surface. This is the technique we used.

Middleton: Yes, I realize this.

Clegg: What is the lifetime of some of these sputter cathodes? How long can you run before you begin to see deteriation in the beam current levels?

Middleton: That depends enormously on the material. It is all very good to talk about 60 μA of copper, but who wants 60 μA of copper! If you run at that sort of level the lifetime would be relatively short. I think the copper cathodes may only last 40-50 hours before it very severely sputtered. But on the other hand, if you would have run that copper cathode at a current more compatible with tandem requirements, say 5 or 6 μA, then the lifetime could be several hundred hours. I think its very difficult to generalize. I think the cathode lifetimes are fairly reasonable. Now, one thing I did not mention, that even though we don't have a valve on this source, we can change cathodes and be back in operation within about 15 minutes because of the very small source volume which operates at a temperature of about 300°C. After initial bake-out, the vacuum is very, very good. If one back fills the system with dry argon then one can pump down in a matter of about 5 minutes. Its really quite remarkable. I mean that within 20 minutes we can change cathodes and be running a different element. We are, of course, thinking of trying to build a source where there is a multiple cathode arrangement very much like the sputter cone wheel.

Ion	I^- (μA)	Cathode
H^-	40 – 60	1/8" diam. by 1/8" thick titanium pill loaded with H_2 gas
$^{12}C^-$	60 – 150	1/8" diam. graphite pill
$^{27}Al^-$	~1.0	1/4" diam. aluminum cathode
Al_2^-	~12	
$^{28}Si^-$	20 – 40	1/8" diam. piece of silicon crystal
$^{48}TiH^-$	4.1	same cathode as used for H^-
$^{59}Co^-$	10 – 23	1/8" diam. pill of cobalt
$^{58}Ni^-$	40 – 60	1/4" diam. nickel cathode
$^{63}Cu^-$	40 – 70	1/4" diam. copper cathode
$^{107}Ag^-$	20	1/8" diam. pill of silver
Pt	10 – 35	Small natural platinum nugget mounted in 1/4" diameter aluminum cathode

KROHN 1962

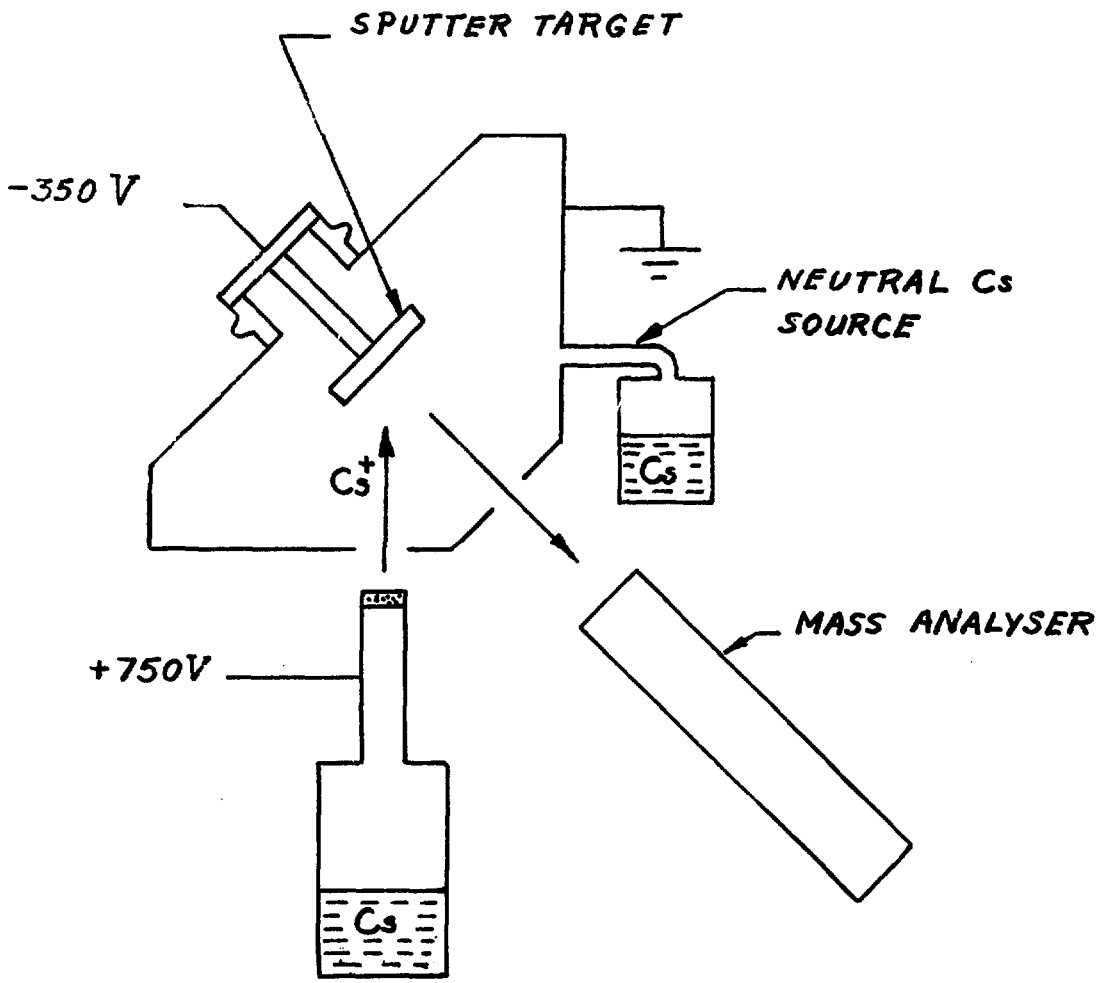

Fig. 1 - A highly schematic drawing of the apparatus used by Krohn[1]) to investigate Cs^+ ion beam sputtering in 1962.

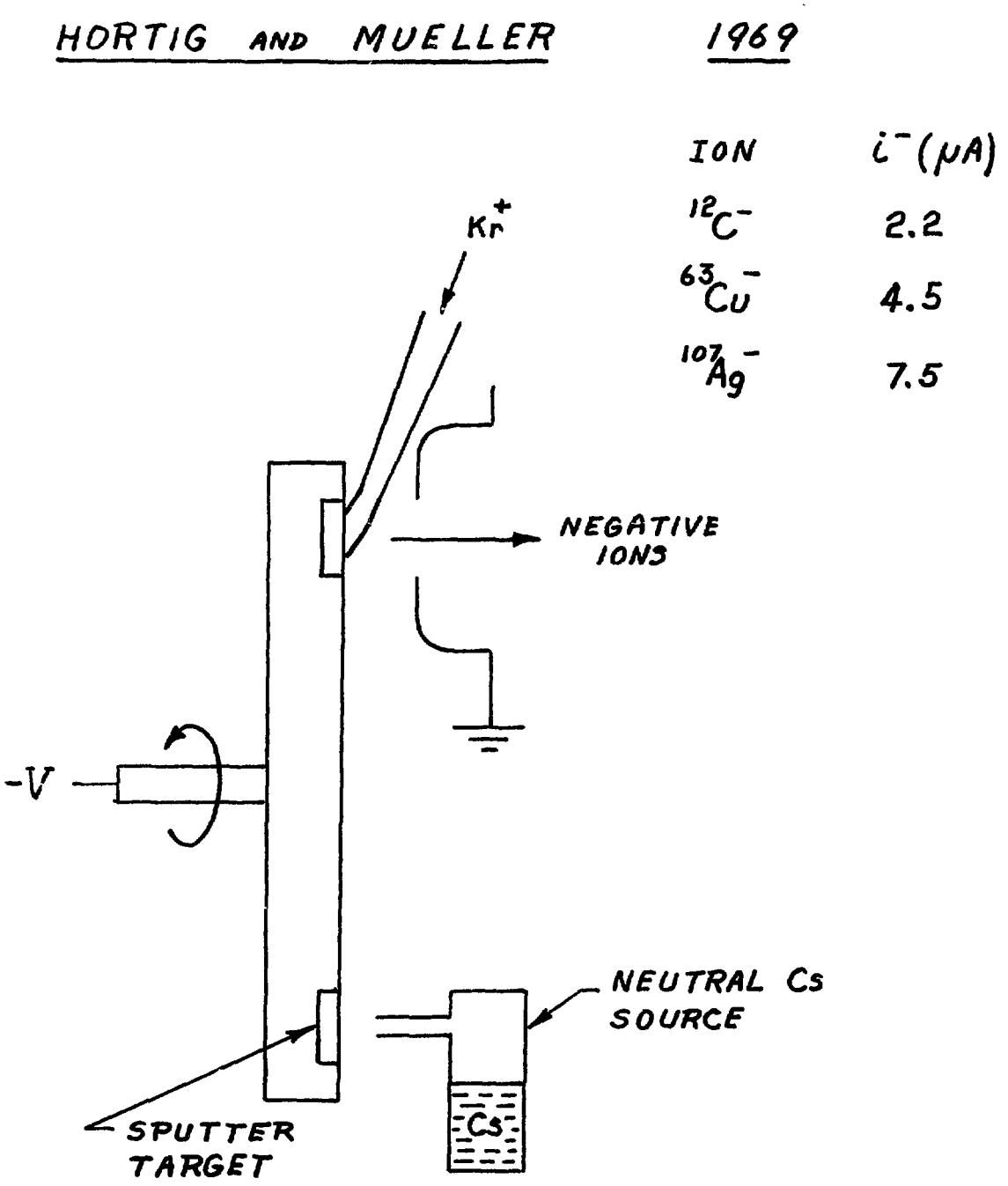

Fig. 2 - Principles of the first sputter ion source, developed by Hortig & Mueller[2]) in 1968/69, embodying the discoveries of Krohn.

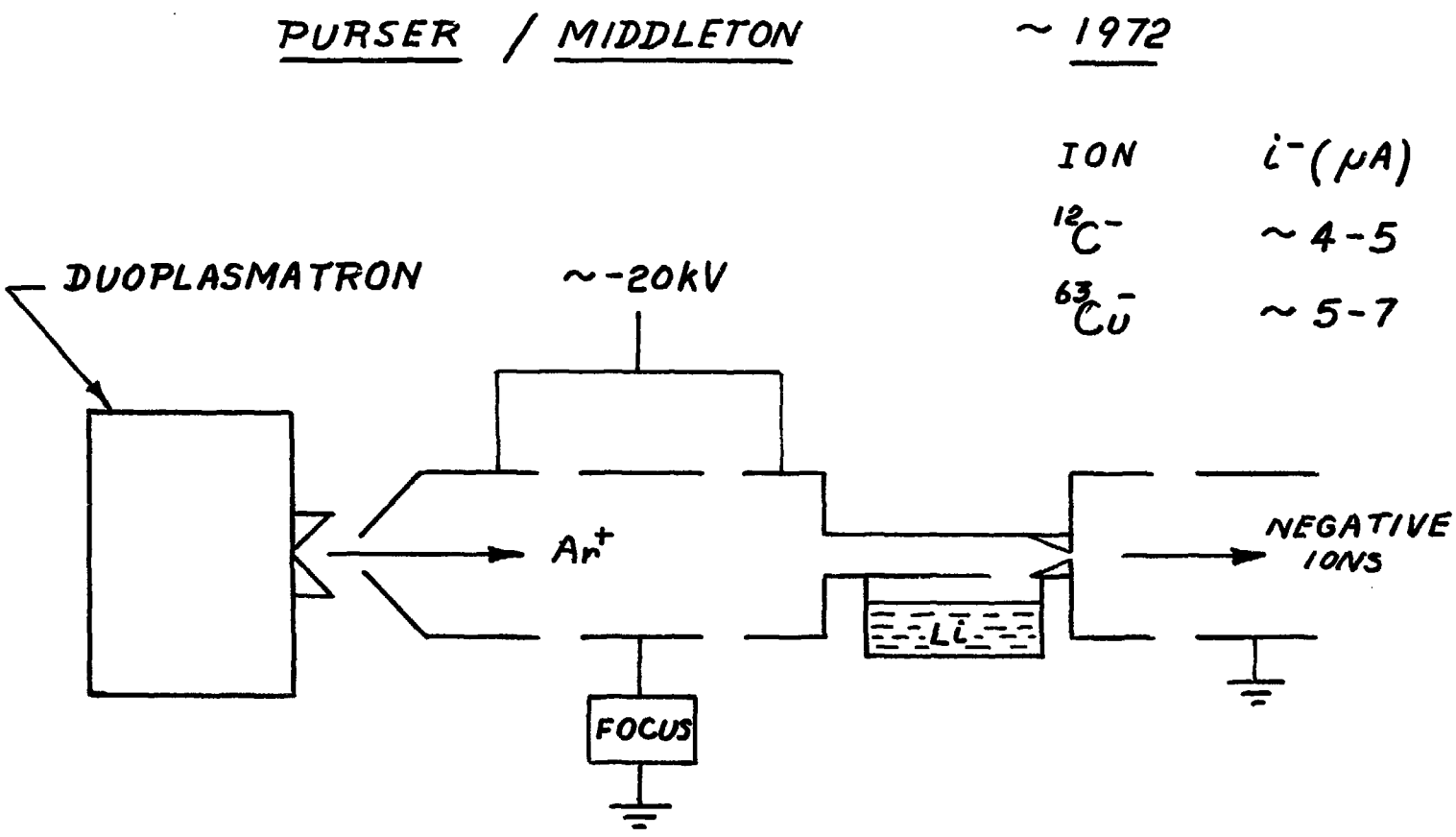

Fig. 3 - A schematic drawing of the sputter ion source proposed by the author in 1972 [3].

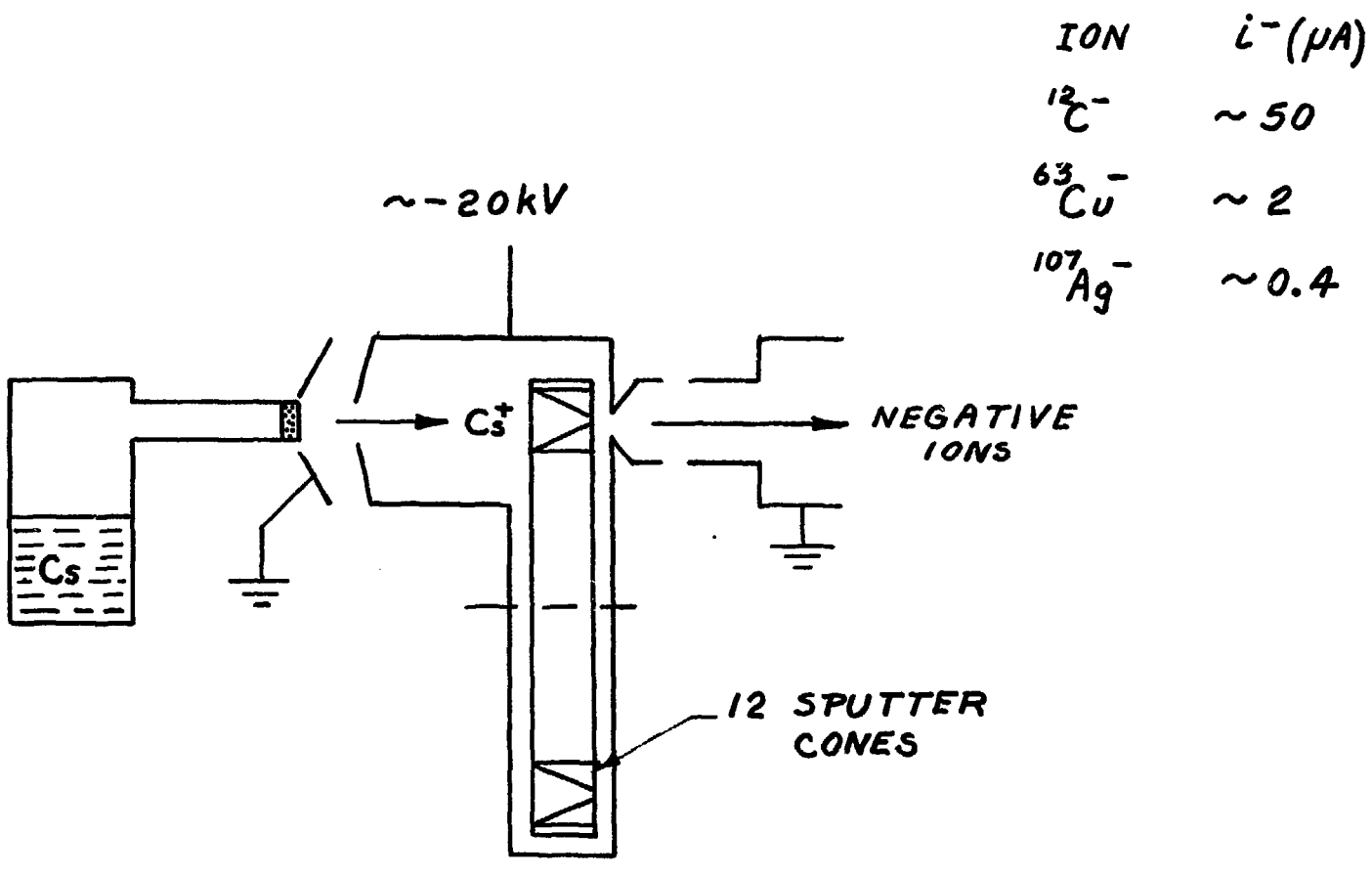

Fig. 4 - The Cs^+ beam sputter source as first constructed by Middleton & Adams[4]) in 1973/74.

CHAPMAN / MIDDLETON 1976

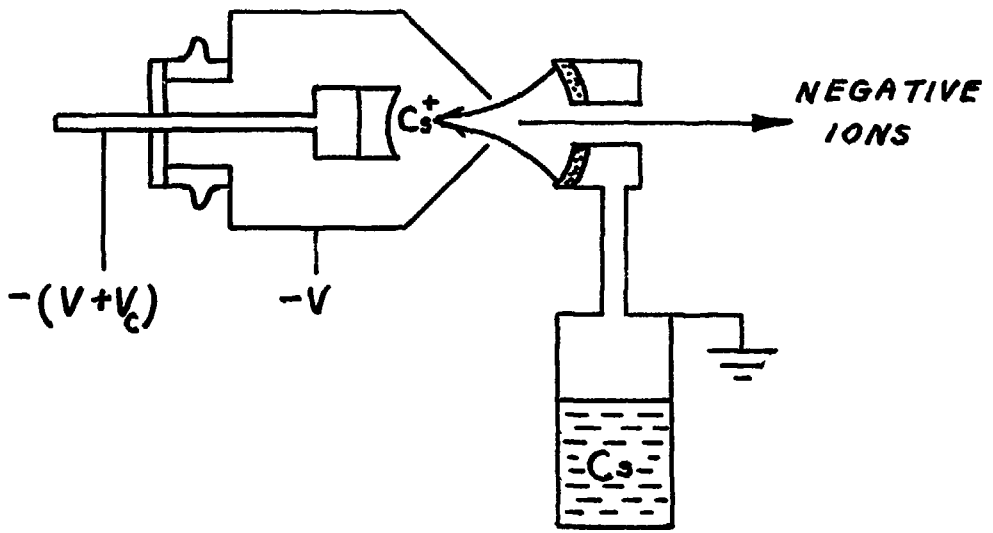

Fig. 5 - Schematic drawing of the 'inverted sputter source' as developed by Chapman[6]) and the author[7]) in 1975/76.

HYDER ET AL 1975

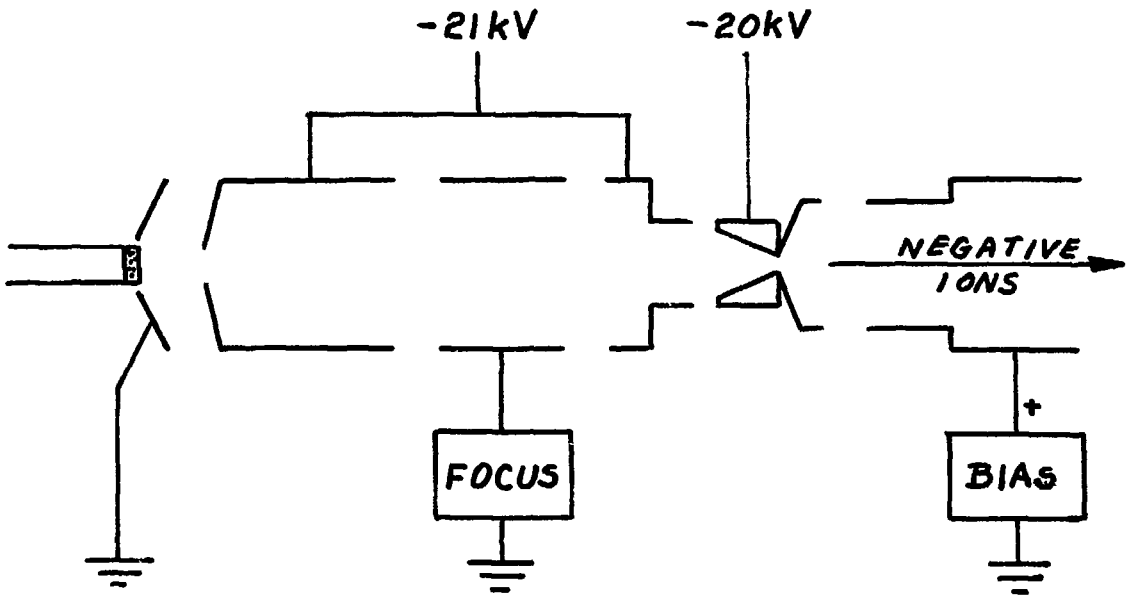

Fig. 6 - The modified sputter source incorporating a Cs^+ focussing einzel lens, developed by Hyder and collaborators at Oxford.

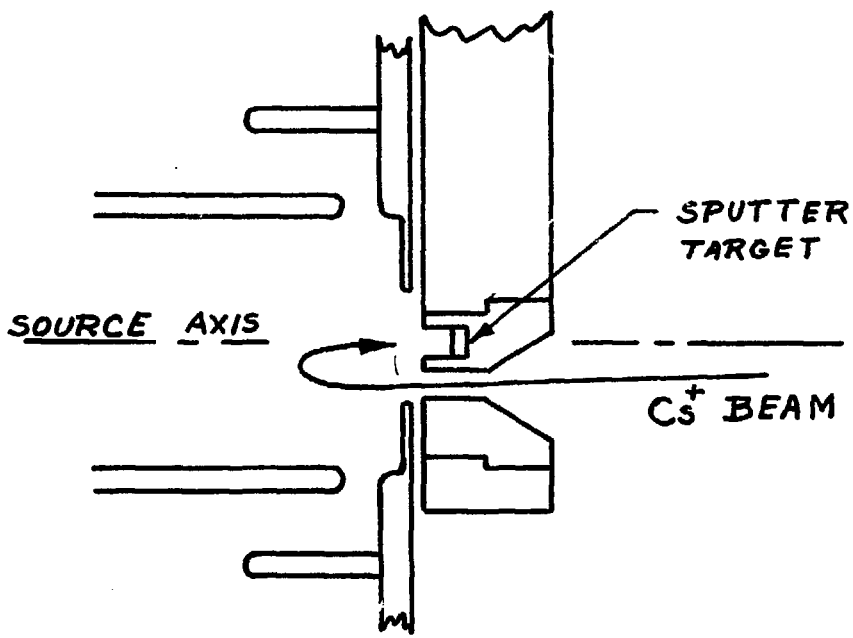

Fig. 7 - Showing the principle of the reflected Cs^+ source as principally developed by Brand[11,12]) during 1976/78.

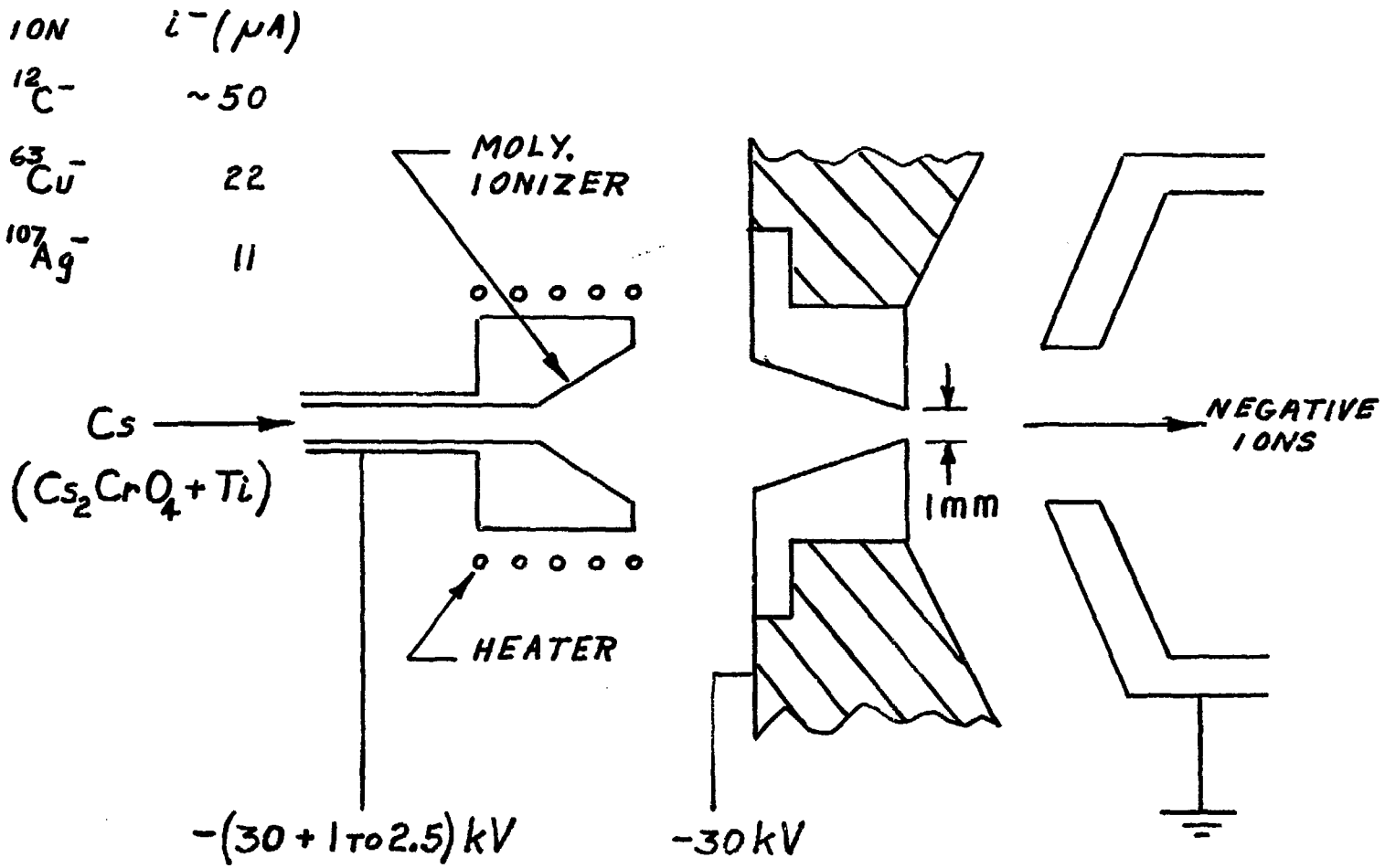

Fig. 8 - A simple and highly efficient Cs^+ sputter beam source developed in Russia[12].

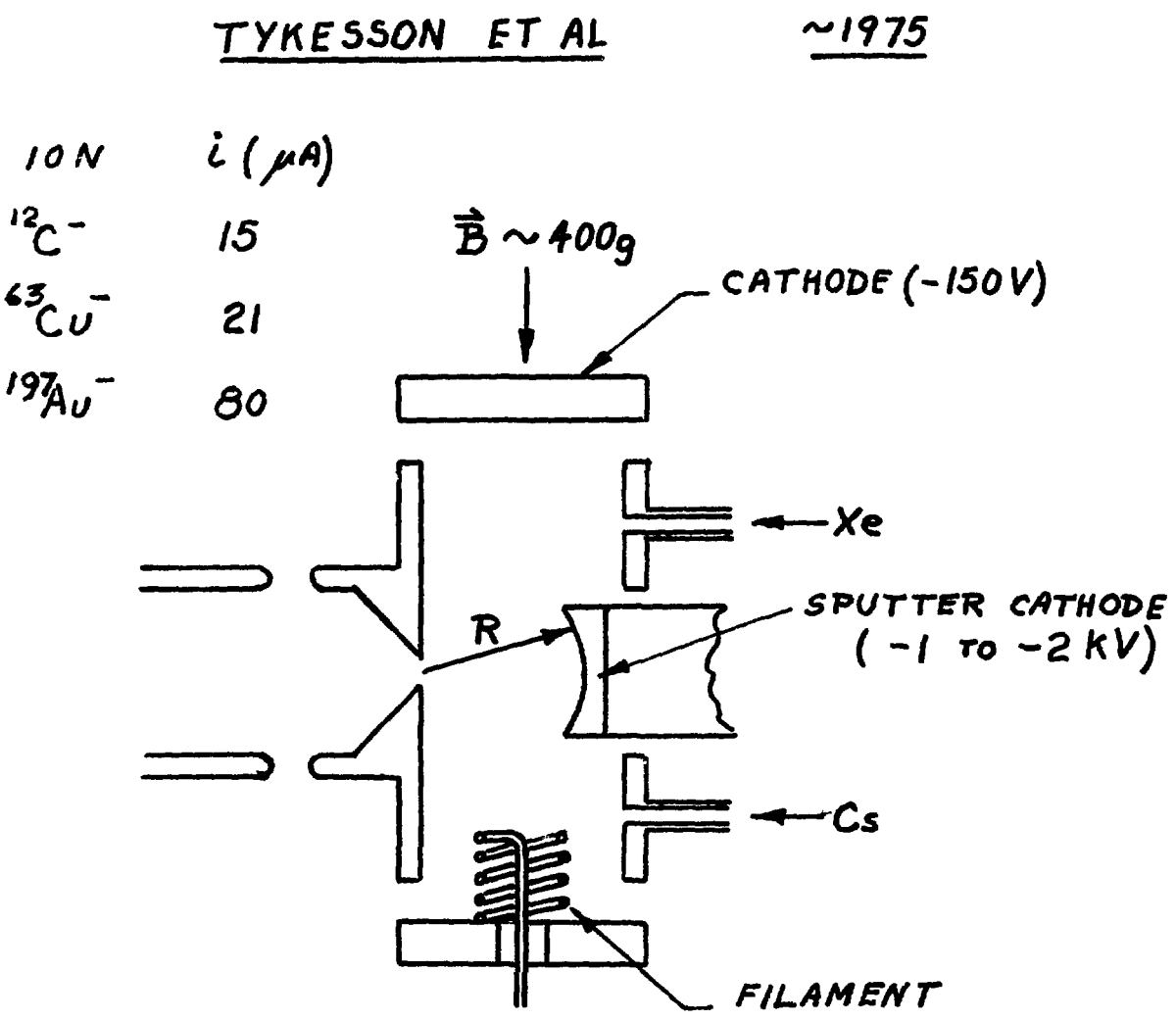

Fig. 9 - The Aarhus sputter PIG source[13].

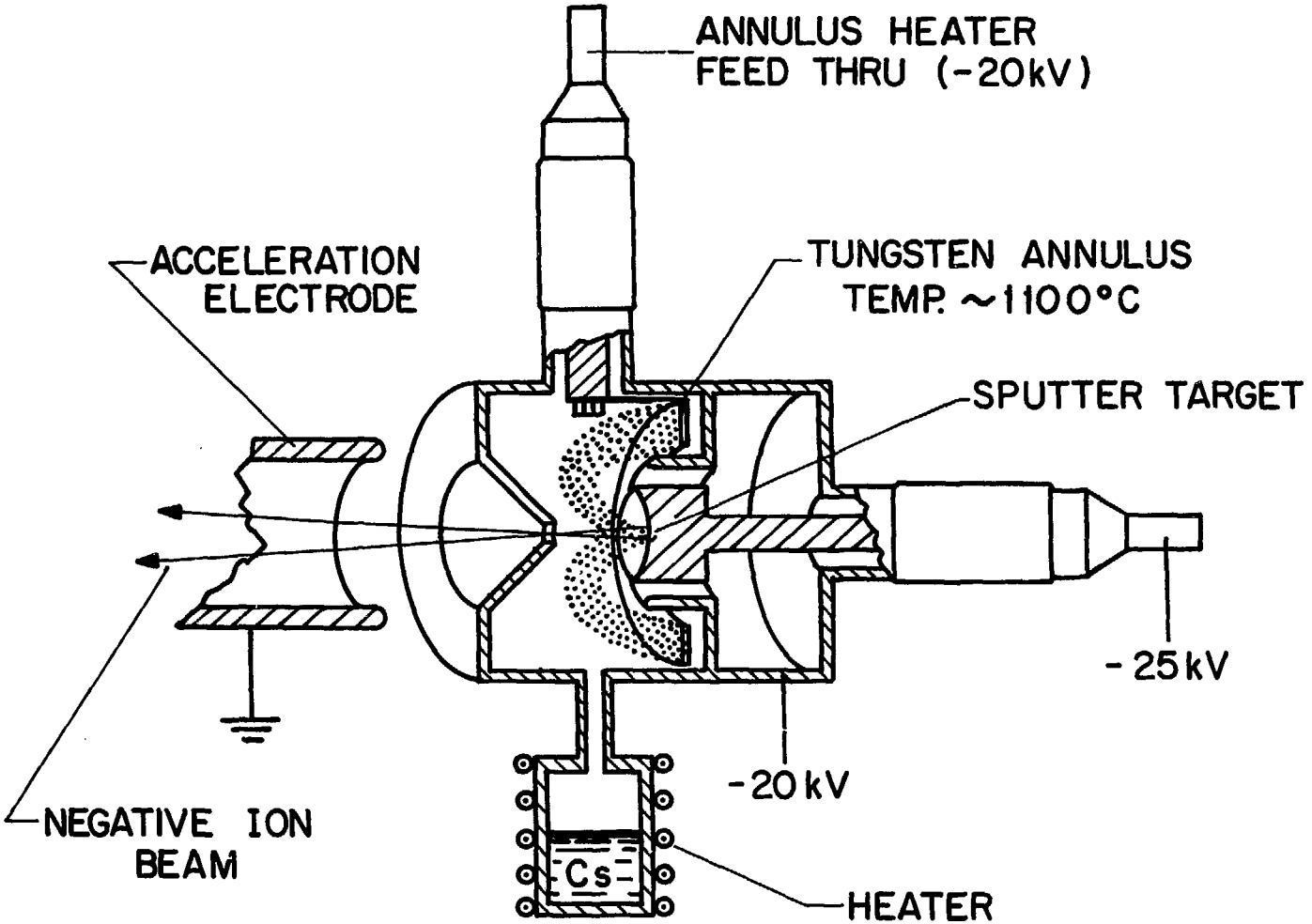

Fig. 10 - A modified version of the Aarhus source developed at Penn, where the Cs^+ 'plasma' is generated by surface ionization rather than by a PIG discharge.

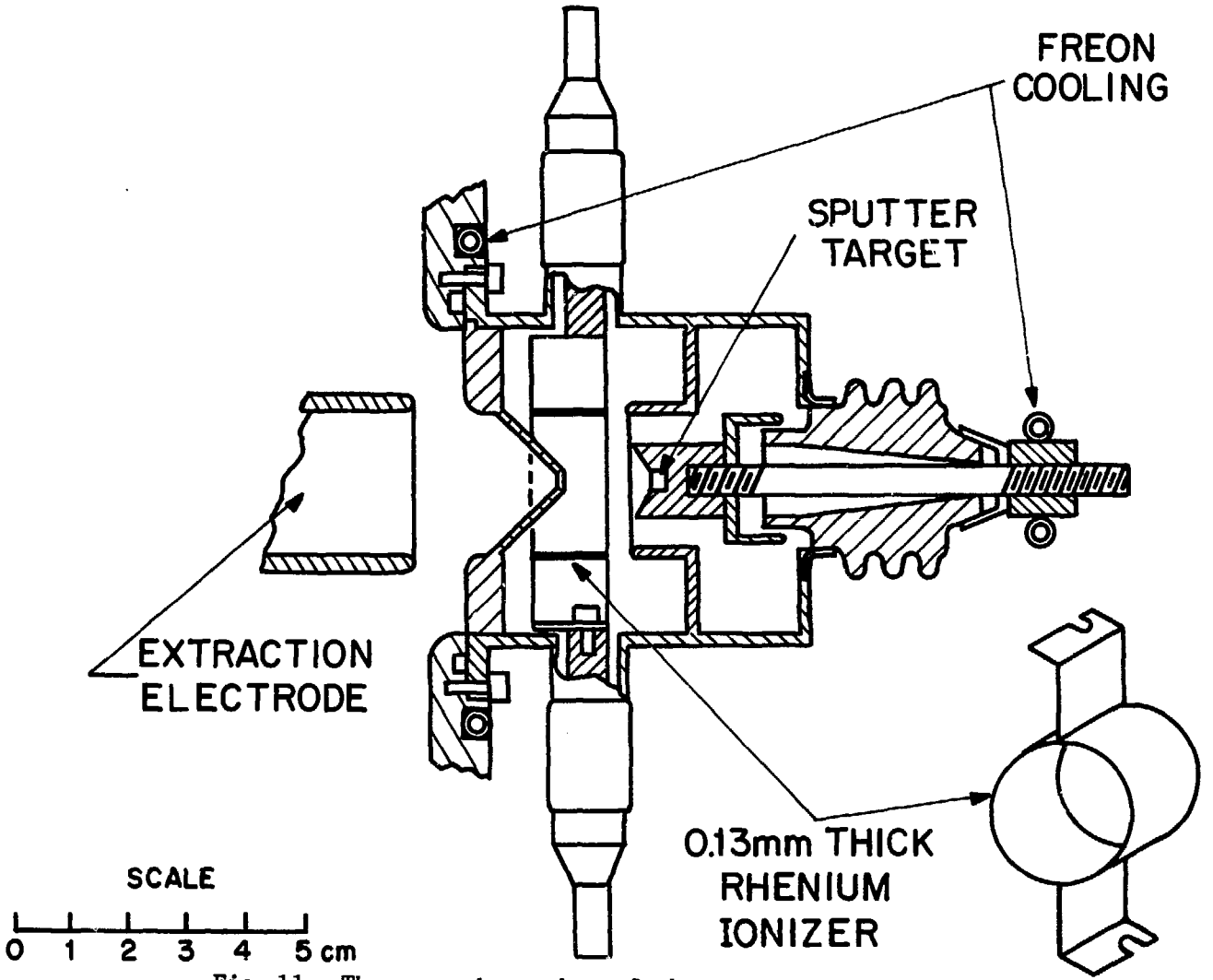

Fig. 11 - The second version of the source depicted in fig. 10 to be built at Penn.

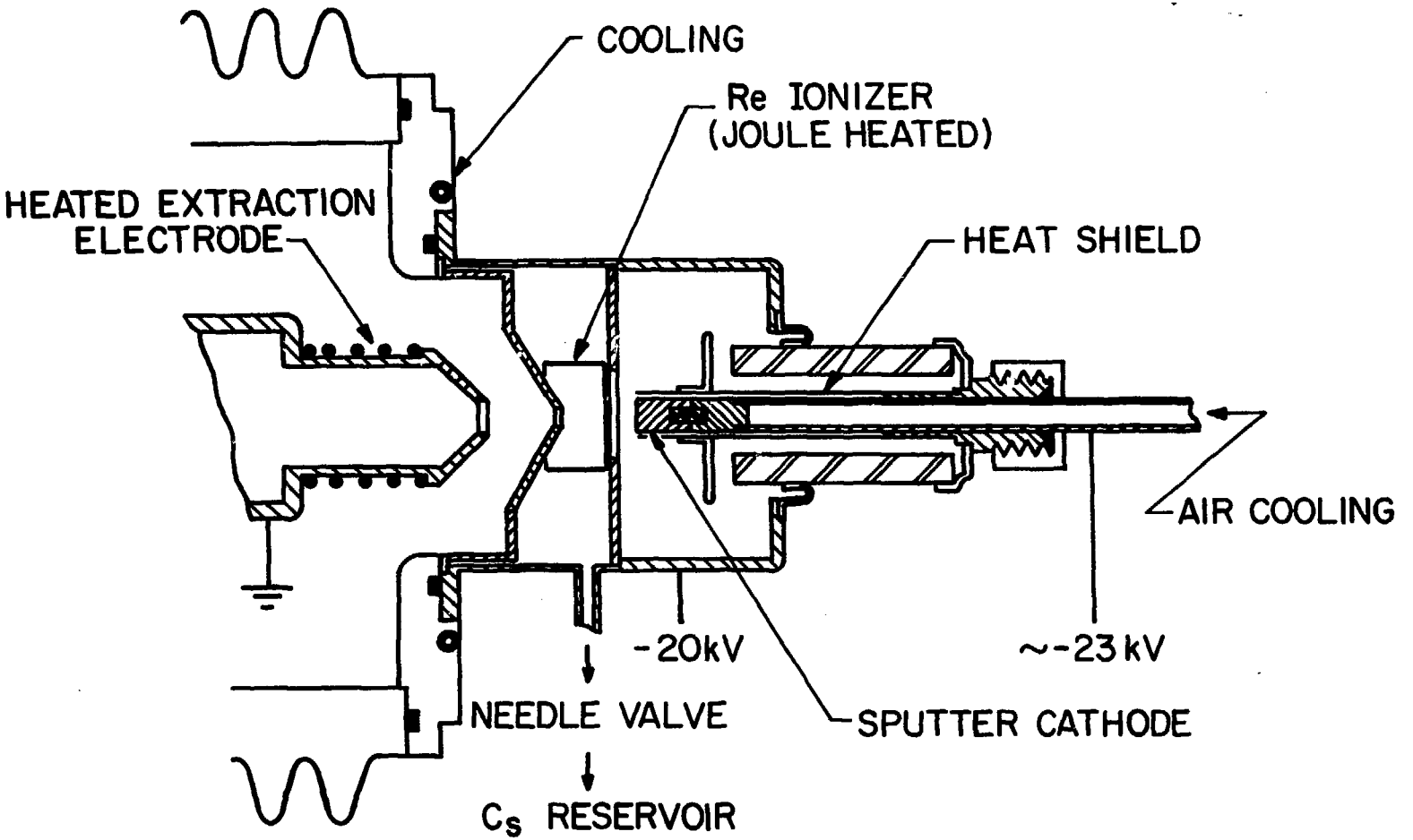

Fig. 12 - The third version of the source depicted in fig. 10 to be built at Penn. The table lists some of the currents obtained from this.

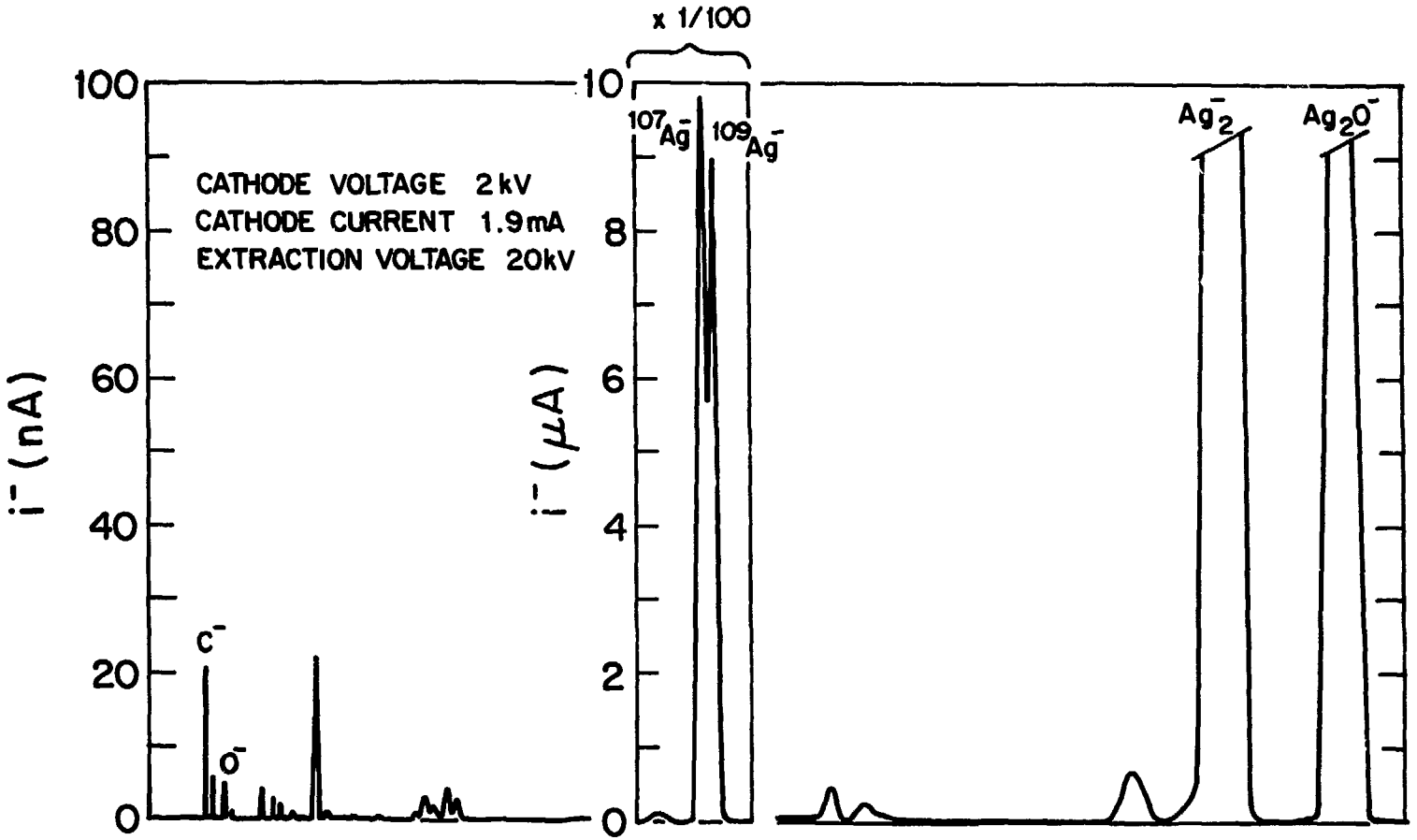

Fig. 13 - A typical spectrum obtained with the source shown in fig. 12, obtained while operating conservatively, with a Ag cathode. Of particular interest is the low intensity of impurity and molecular beams - presumably reflecting the extremely good vacuum in the source head.

SNICS -- A SOURCE OF NEGATIVE IONS BY CESIUM SPUTTERING

J.H. Billen and H.T. Richards

Department of Physics

University of Wisconsin, Madison, Wisconsin 53706

In this report we present recent work on the University of Wisconsin Source of Negative Ions by Cesium Sputtering (SNICS). By way of introduction I will quote from a paper[1] on SNICS that is now in press.

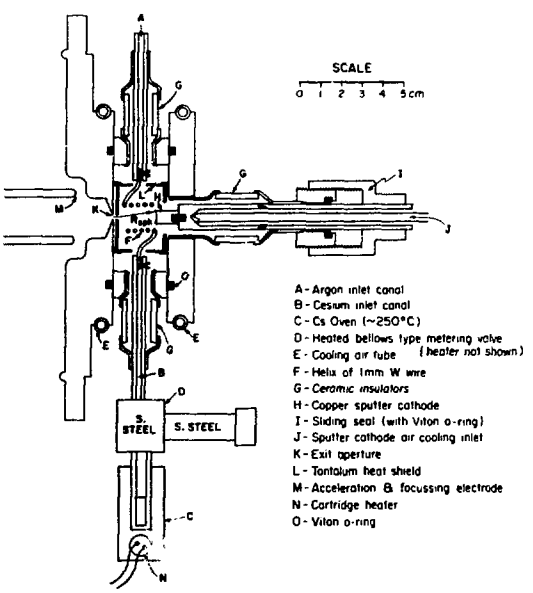

Fig. 1

Figure 1 shows schematically a cross section (side view) of the source. In our design we intended to combine good features of two earlier sputter sources. Thus the helical filament (of 1 mm W wire) and axial geometry follows the Hill-Nelson[2] sputter source for positive ions, and the concave sputter cathode is from the Aarhus Negative Ion Source[3] (ANIS). (Because of the way the sputtering occurs in SNICS, however, the shape of the sputter cathode is evidently not important. We discuss this in more detail below). Each of the filament leads has a gas

inlet canal. Cesium vapor enters the discharge region through a heated bellows type metering valve of stainless steel connected between the Cs oven and one of the inlet canals. The other canal may be used for a support gas such as argon, but best performance is with no argon gas flow. A filament current of ∿ 35 amperes serves primarily to surface ionize the cesium vapor. A set of radiation shields completely encloses the filament and insures that the interior of the source runs hot and yet permits flange temperatures low enough for viton o-rings. With only ∿ 200 watts power input to the source, air cooling of the sputter cathode and flanges is adequate.

A sliding seal permits adjustment of the sputter cathode-to-exit aperture spacing. This design also allows easy interchange of different sputter cathodes.

Figure 2 shows a schematic of source potentials. Some early tests with Cu^- and C^- ions indicated that higher output occurred for a positive filament bias, but for simplicity most tests with these and all tests with other beams correspond to the filament at source ground. The source base is ferromagnetic to keep diverging magnetic fields produced by the filament solenoid from

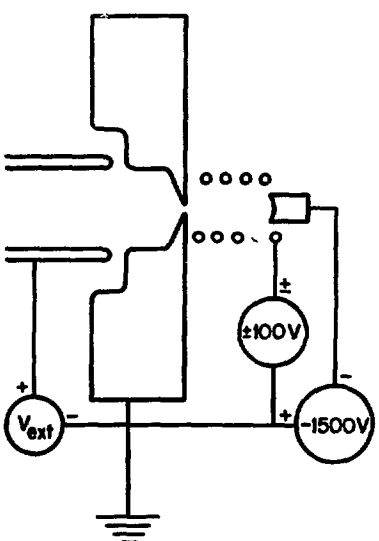

Fig. 2

the region beyond the exit aperture. Now, though, the presence of a magnetic field in the discharge region appears irrelevant. Initially we operated the filament on d.c. believing that the ∿ 100 gauss axial magnetic field this provides is important for sustaining a discharge. However, we get the same performance with an a.c. filament and the source output is insensitive to variations in the (d.c.) filament current between 35 and 55A. Also, Middleton's new source[4] which is very similar to SNICS, operates without any magnetic field. In our second design of SNICS the source base is of stainless steel and thus will not exclude the magnetic field of either a.c. or d.c. operated filaments from the focus electrode region. We have too little experience with the new design to know whether this is important.

We have studied the negative ion output of SNICS as a function of several source parameters and have alluded to some of the results above. Most of the tests have been with Cu^- beams but we observe similar behavior with other sputter cathode materials. The single most intriguing observation comes from an examination of the sputter cathode after running. It shows pronounced axial erosion which for an extended run always results in a narrow (approximately conical) hole in the center of the cathode. All cathodes (even initially flat surfaces) developed such central hole erosion.

Figure 3 is a photograph of some copper sputter cathodes which illustrate the central hole erosion after running. The cathodes in the top row were run with a 3-wire stranded filament and show a fluted erosion pattern as well as the central hole. The cathode at the top right of Figure 3 ran for 250 hours. The bottom row shows copper sputter cathodes

Fig. 3

before and after a run involving a solid wire tungsten filament. In our more recent work we used only solid wire W filaments like the one at the lower left of Figure 3.

If the sputtering occurs by Cs^+ ions originating in the plasma and which have been accelerated across the cathode sheath to the sputter cathode, then the narrow sputtered holes require a Cs^+ ion density that peaks on the source axis. At first we thought that the magnetic field from the filament and the sputter cathode potential were both important for focussing the Cs^+ ions onto the sputter cathode, but results of Middleton's work certainly indicates that the focussing is only electrostatic.

While the mechanism by which the narrow hole is eroded is not entirely understood, the fact that the hole forms probably explains the lack of a sharp peak in the negative ion output as a function of the cathode-to-exit aperture spacing S. We had anticipated a peak at a spacing equal to the radius of curvature of the concave sputter cathode (19 mm). Figure 4 shows the Cu^- current versus cathode spacing S for an initially concave sputter cathode. We observe a broad maximum in the region of 16-19 mm. An initially flat sputter cathode showed stronger dependence on S and peaked at S=15 mm (see Figure 5).

Unfortunately we do not measure the Cs vapor pressure in the source, but instead rely on the relative settings of the metering valve above the Cs oven. A heater around the valve keeps the valve temperature

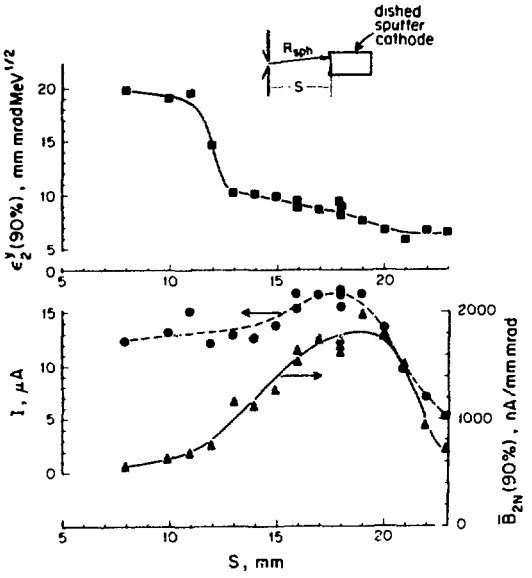

Fig. 4

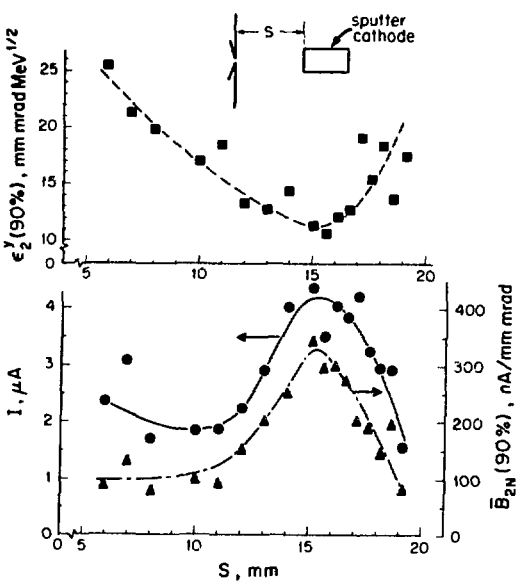

Fig. 5

at ∿ 300°C, and the oven is maintained at ∿ 230°C. Output current is very sensitive to Cs input only for low Cs vapor flow into an initially clean (cesium free) source. After enough Cs has entered the source to produce several mA of sputter cathode current we often run many hours with the metering valve closed but with very little decrease in the negative ion output. Indeed a 2g charge of Cs is enough for several hundred hours of operation.

Table I summarizes the results obtained with SNICS to date for several different sputter cathode materials. The source operated at each of the quoted beam currents for at least several hours and in some cases for days. Thus these negative ion currents are not necessarily the maximum beam currents obtained. All of the results in Table I correspond to

Table I

Negative Ion Beams from SNICS

Cathode Material	Hours Tested	Beam	Analyzed Current (μA)	Emittace for 90% of beam (mm mrad $MeV^{1/2}$)
Graphite	89	C^-	20	4.0 π
		C_2^-	10	—
Aluminum	62	Al_2^-	5.0	2.1 π
Vanadium	60	V^-	4.0	—
Nickel	82	Ni^-	6.5	—
Copper	many	Cu^-	17	3.0 π
Tantalum	72	Ta^-	0.25	—
		TaO^-	2.5	—

zero filament bias potential and to sputter cathode potentials less than 2500 V. With essentially all beams we observe a monotonic increase in negative ion output as the sputter cathode becomes more negative. These currents could probably be pushed higher with a higher sputter cathode potential. Our two-dimensional emittance measurements for three of the beams indicate SNICS' excellent beam quality. Figure 6 shows a mass spectrum of the negative ion output after running for 20 hours with a graphite sputter cathode. We see peaks corresponding to molecular carbon through C_8^- but the C^- beam dominates. Figures 7 through 9 illustrate the

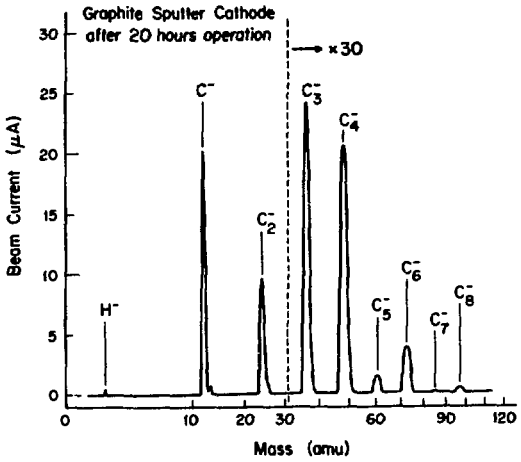

Fig. 6

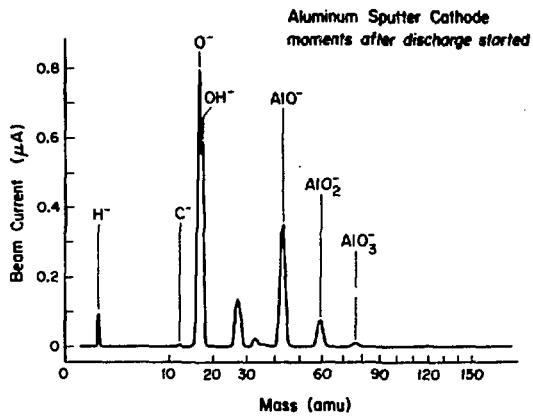

Fig. 7

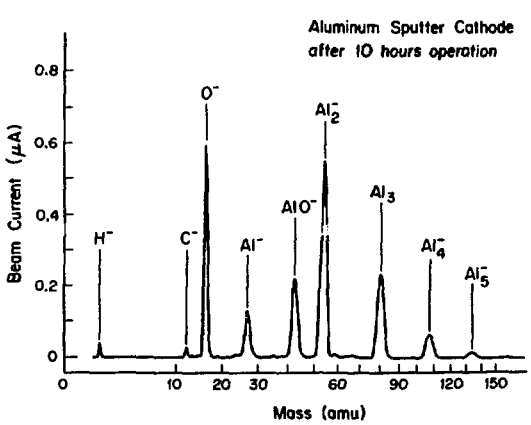

Fig. 8

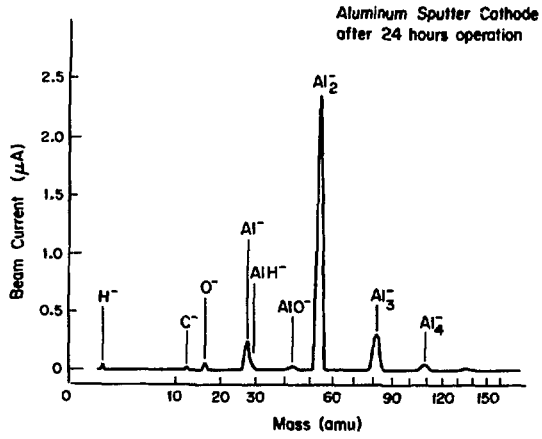

Fig. 9

evolution of the Al_2^- beam with running time. Initially we see mainly negative ions of aluminum oxides and also a strong O^- beam. As the oxide layer on the cathode is sputtered away these beams diminish and molecular

beams of aluminum through Al_5^- become the main components. Eventually the Al_2^- peak dominates the spectrum. The behavior with a vanadium sputter cathode is similar. Figure 10 shows a mass spectrum for a vanadium sputter cathode after only a few hours of running. The largest

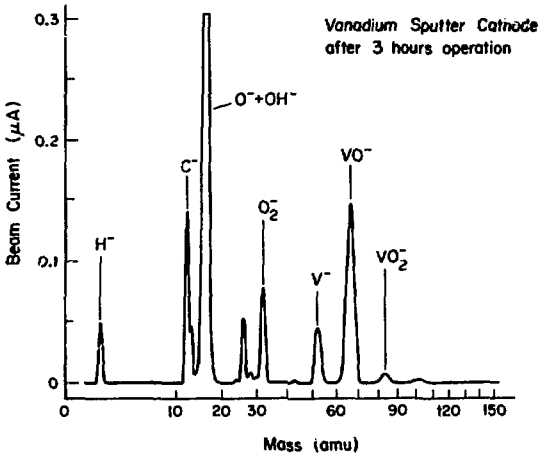

Fig. 10

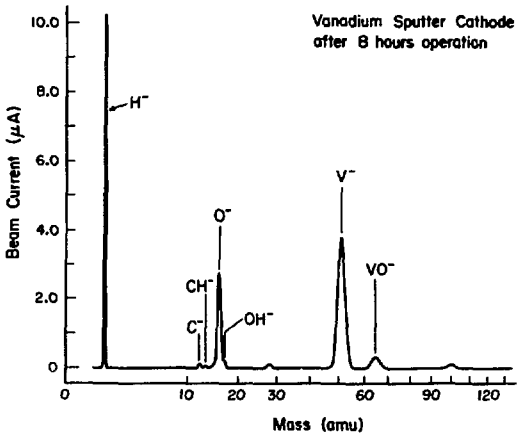

Fig. 11

components are O^- and OH^- and the VO^- beam is larger than V^-. After 8 hours (see Figure 11) the oxygen and the oxide beams are down compared to the V^- beam, but H^- is the most intense beam. With a tantalum sputter cathode we never did get an appreciable intensity of the pure metal ion beam even after a long run. Figure 12 shows that the Ta O^- beam is the largest component.

In addition to the cathode materials listed in Table I we have tried sputter cathodes of titanium, molybdenum and niobium, so far with somewhat less encouraging results than for the other beams. The largest beams of these materials that we have produced are in the tens of nanoampere range. While we ran the discharge with the titanium sputter cathode

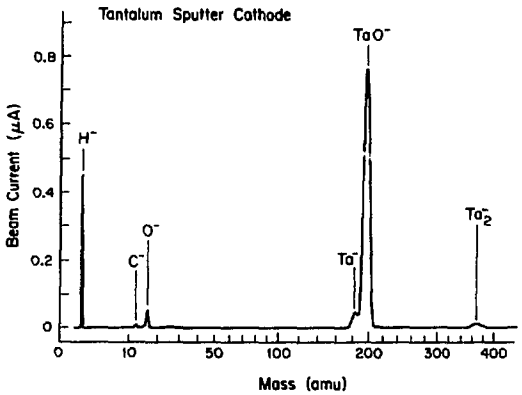

Fig. 12

installed we tried flowing a small amount of hydrogen gas into the source in an attempt to get an H^- beam, but we produced only $\sim$ 1μA of H^- in this manner. We have not yet tried a hydrogen-loaded titanium cathode as Middleton has. Our results with the vanadium cathode (see Figure 11) do indicate the presence of some hydrogen in the vanadium, so we are persuaded that a hydrogen-loaded titanium or vanadium cathode will yield a large H^- beam.

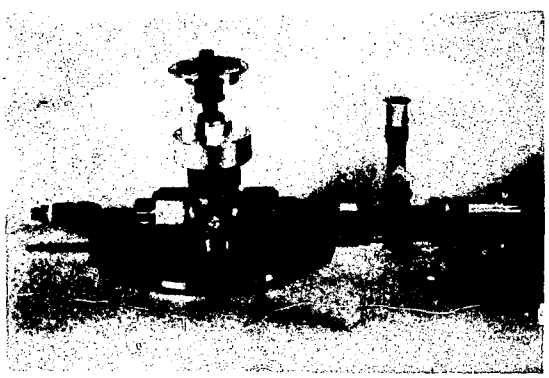

Fig. 13

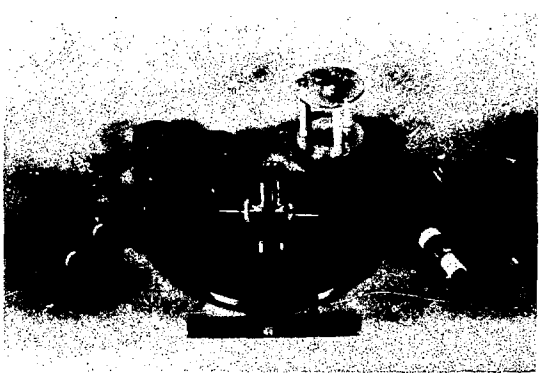

Fig. 14

Presently we are beginning tests on our new source which is shown in Figures 13 and 14 in assembled and exploded views. It uses the same filament as the original source and employs the same basic geometry and cesium supply system. The main differences are in construction and in the control of the sputter cathode position. The body of the source is a 1-inch Cajon tee welded to a base plate of stainless steel. The combined filament leads and gas feed canals are built into detachable Cajon glands as is the sputter cathode assembly. Control of the sputter cathode position is by an all-metal bellows seal instead of a sliding seal.

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DISCUSSION:

Jones: I have the impression that your source likes to make molecular species and Roy's likes to make elemental species. Is that a correct impression?

Middleton: I don't think so. I think we make molecular species just as regularly. But we do seem to clean up faster than you.

Billen: Right. We don't have as good as pumping to the source, probably. Our aperture diameter is only a millimeter and we did have a fair amount of trouble with this initially and we tried to install a bypass for pumping around the aperture, but it didn't increase the pumping speed significantly over the millimeter aperture because we had to go through a fairly long tube. So it does take awhile to clean up, several hours or so, unless we're fairly careful about keeping water vapor and alcohol out of the source when we are cleaning it.

Alton: Hot cesium in the source itself is one of the best pumps that one can imagine. Therefore, it is hard to visualize that additional pumping would affect the vacuum level, and hence, molecular species that you observe. I think it just a reflection of, perhaps, surface contamination of the cathode.

Billen: Oh yes. I don't claim that if we had additional pumping, we get more of any molecular beams, it just that it takes us several hours for the source to pump down, until we get a fair reduction in, say, O^- beam.

Alton: How long do you expect that you would have to run the tantalum oxide in order to start seeing the decrease in the yield?

Billen: We ran with that for four days and it didn't decrease.

Wegner: Two questions. First, have you tried introducing oxygen or any gas like that to make the tantalum oxide beam perhaps larger by enhancing oxide production and secondly, it would appear that your system performs very well when it get a conical hole in it. Have you ever tried putting in a cathode with a conical hole built into it so you can start up under, you might say, long term running conditions?

Billen: Yes, I did that just recently. It did improve the starting time a little bit but we still had a fair amount of time in cleaning up the surface. I did this with vanadium and, indeed, it took maybe 8-10 hours

before we obtained an appreciable vanadium beam and it took maybe 3 or 4 hours after I had drilled the hole in it. But the main thing is, I guess, getting the surface layer of the oxide sputtered away and it could be, we just don't push it hard enough. Usually we're a little bit conservative about putting the cesium into the source because we do have the problem that we get arcing if you get too much cesium into the source. You have to get a lot to do that but we still like to be a little bit conservative, so we don't have to take things apart so often. And the other question, I don't think we have tried oxygen, lately we've tried flowing hydrogen into the source to try to increase either hydride beam of vanadium or to enhance the hydrogen beam with the vanadium on a titanium sputtered cathode. We are sort of confused about this, it turns out that the least amount of hydrogen into the source ruins the negative ion output. That is, if I can sit and watch the vanadium beam at 4 μA and put in just a few microns of hydrogen above the base pressure and the vanadium will drop down to a μA or less. The hydrogen beam is affected the same way. The 10 μA beam of hydrogen also decreases when I let in hydrogen gas. So the source just loves to run with just cesium vapor and no support gas, it appears.

Middleton: I'd like to make just two comments. One, we literally let our source up to dry argon. I think that really enables us to clean up very much more quickly. I don't know whether you do the same or not.

Billen: Yes we do. Maybe I've given a little bit of the wrong impression. This evolution of the beams for the aluminum and vanadium is not characteristic of all the beams. For copper ions, we usually can obtain a beam in just a couple of hours of operation. Indeed, when we're running on the tandem, some of the engineering students were radiating samples with a copper ion beam and another one of the students wanted to use an aluminum beam and after finishing the copper run, we went up to the source and let it up to dry argon and we had 4 or 5 μA of aluminum beam in less than 45 minutes, so indeed, if you're careful about keeping the source clean, you don't see these problems.

Middleton: I'd like to make one other comment and this is really addressed to Harvey Wegner's question. I think Harvey, at the back of his mind, dearly wants to know whether we can make calcium hydride with this source? We have tried a number of gases in the source and have run into problems. Certainly, you can admit oxygen gas and get the horendous oxygen beams. We have tried a number of other gases, we did, in fact, try ammonia and I think this was a disaster for the ionizer. We also tried CO_2. The reason there was actually to make carbon beams ready for ^{14}C dating. The carbon interacted very drastically with the ionizer, its conceivable that if one uses the right kind of ionizer, one might be able to operate this type of source with gases, in the sense, that the Unis source uses gases. It is possible, maybe by using, say, a platinum ionizer, one would have to be a little bit careful on temperature, it is conceivable that if one uses platinum that one could introduce obnoxious gases without destroying the ionizer.

Nichols: I don't have a question, just some comments I'd like to make on some information we've gained. Our needs are such that we're running a titanium beam one day, a nickel beam the next day, a carbon beam the next day, and the 2 or 3 hours that it was taking to get on target was killing us, so we started sandblasting the metal slug before we put it in. We finish one run one day, we sandblast the slug that we're going to use for the next day, and by morning, we fire up and have beam on target in something like 20-30 minutes. Another interesting thing, we had a little trouble getting our tantalum beam when we were air cooling the cathode. We started freon cooling the cathode and this improved it significantly. But an interesting observation is that, we must turn off our freon cooling first while the source is cooling down at night, otherwise the cesium will make the feedthrough insulators breakdown quite easily. The other thing I might mention, we tried the tantalum beam with both oxygen and krypton and if we sandblast that tantalum slug before we put it in, krypton seems to do just about as good as the oxygen.

THE ^{14}C BEAM AT THE MUNICH MP TANDEM

W. Kutschera*

Fachbereich Physik der Technischen Universität München,

D-8046 Garching, W. Germany

At the Munich MP Tandem, we are running a ^{14}C beam since about the beginning of this year and I should like to report here on our experiences with such a beam. The basic development work has been published recently[1], so I am going to discuss mainly the subjects which are not covered in that paper.

^{14}C is a radioactive isotope and therefore we wanted to use as little material as possible for the first trials. On the other hand, we wanted to get sufficient ^{14}C-beam current to perform nuclear physics experiments. As a compromise, we started with an activity of 1 mCi which corresponds to 0.225 mg ^{14}C and from which we hoped to produce 10 nA $^{14}C^-$ in a Cs-beam sputter source. To give you an impression for how little activity this is, I can tell you that the new (1976) and rather strict law for handling radioactivity in Germany states that a person under radioactive health control is allowed to eat six times this ^{14}C activity per year. For the practical use of the radioactive material it was, however, important to find a safe method for putting the activity in the ion source. Especially since we were aiming to use higher activity material later on. About one and a half years ago, I learned from Michèle Dumail of Orsay, that a good way would be to fuse the ^{14}C into iron. After several trials with non-active material, one of our target makers in Munich, Hans-Jörg Maier, managed to fuse 24 mg of elemental carbon containing

1% ^{14}C (1 mCi) by means of electron beam melting into 350 mg iron powder[1,2]. The result was a sphere-shaped pellet of about 4 mm diameter, which should consist of mainly the compound iron carbide (Fe_3C). This pellet was put into an old HICONEX 810 source, which we bought from Ken Purser several years ago. It is worthwhile to notice that this source was considered to be a piece of junk by some people when we bought it. However, it was so junky big that we could play around with it very well and finally, by putting in a self-made Cs-beam lens, it became our most liked toy for producing exotic beams[3,4]. At present, this source is solely devoted to produce the ^{14}C beam and is permanently installed at the negative ion test injector[5].

Figure 1 shows the sputter geometry in this source. The pellet sits on the backside of an Aℓ cone and the Cs beam is steered through the off-axis cone hole and reflected back to the pellet by the extraction potential. This method of running a Middleton-type sputter source was developed by Klaus Brand from Bochum[6-8]. Figures 2 and 3 show photographs of the pellet after a running time of 100 h. Without going into much detail, it is seen that a very narrow groove is sputtered into the pellet resulting in a small negative-ion emitting area and thus in a small beam emittance[7].

The negative ion mass spectrum measured at the test injector is seen in Fig. 4. As expected, the ^{14}C peak is very small. In addition, it is obscured by a small shoulder occurring at mass 14.2. This is due to a negative water molecule extracted from the ion source dissociating into O^- before it enters the analyzing magnet. The mass 14 peak of course contains also $^{12}CH_2^-$ and $^{13}CH^-$. To investigate the relative contributions of these components, we measured the energy spectrum of the beam components coming out of the tandem by elastically scattering it on a thin gold foil into a Si surface barrier

detector. Figure 5 shows that the charge states split up into three components, due to the different energy portions given to the isotopes by the break-up of the respective molecules at the terminal stripper foil.

Having proved that we indeed get a couple of nA of ^{14}C out of a 0.6 mCi sputter target we made a new pellet with much higher activity, namely 80 mCi. The negative ion mass spectrum of this pellet is shown in Fig. 6. The ^{14}C-current has increased to 500 nA which is high enough to perform almost any kind of nuclear physics experiment.

Table I summarizes the relevant properties of the ^{14}C-beam production and operation. The low efficiency in making the first pellet (40% loss) was not due to the lack of experience, but due to a different behavior of the low and high activity material. It was not possible to press a convenient tablet prior to the electron-beam melting process for the low activity material. The activity level measured at the surface of the pellets indicate an attanuation of about a factor 1000 because of the absorption of the low energy β-rays (E_{max} = 156 keV) inside the pellet. Thus, even the 80 mCi pellet can be handled in a rather simple way. The ion source was operated with each pellet for more than 100 hours. The 80 mCi pellet should be good for at least another 100 hours, since the ion source was operated in a cold mode until now. The ^{14}C-beam currents at different experimental areas (gamma chamber, scattering chamber, Q3D spectrograph) are comparable with those of standard beams. The total activity brought into the accelerator system by the beam until now does not exceed 200 μCi. Compared to what is acceptable for a person per year, this is a very low number and therefore we can go on running a ^{14}C beam for quite awhile without hesitating about the contamination.

Finally, I should like to add a few words about the precautions we set up at the ion source to observe a possible activity outflow. Figure 7 gives a schematic of the pumping and monitoring system at the ion source. A 200 ℓ/sec turbo pump is located directly underneath the cone wheel. Except for the extraction electrode hole, we have closed up all other pumping holes downstream to reduce the outflow of activity into the main pumping system of the test injector. The exhaust of the fore pump is fed through a standard gas mask filter which absorbs micro particles. It then runs through a monitoring device consisting of a 15 cm wide x 30 cm long x 30 cm high lucite box with a large-area gas ionization (butan) counter located at the bottom. Since the cross section of the box is about 15 times larger than the cross section of the exhaust pipe, the box acts as a sink for radioactive particles. The monitor counter therefore collects most of the radioactive particles. From the recordings, we calculate an activity flow rate of 1.5 mCi per hour. At present, this measurement is not very reliable, but even if we allow a 10 times higher value it is reasonably low for a long-time operation of the ion source.

The only major problem which occurred during the ^{14}C-beam operation until now was a complete break down of the ion source turbo pump mentioned above, which had to be replaced by a new one. At that time, the ion source had been operated already 50 h with the 80 mCi pellet. The highest activity level was found on the entrance grid of the pump, but still being as low as 0.5 μCi.

In conclusion, the ^{14}C beam produced with the Cs-beam sputter source has developed into a stage of a close-to-routine-operation. However, one question remains: What happens with the ion source when it breaks down and

needs to be dismantled. It is not yet clear whether we are going to consider it as a radioactive waist and throw it away, or whether we are bringing it to a laboratory equipped for handling radioactive components. The first solution is much simpler and for the future of producing radioactive beams it seems to me useful to have inexpensive one-way ion sources available.

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DISCUSSION:

Miller: Did you smear test the inside of your scattering chamber or do you have any comments on how much ^{14}C activity was distributed over other parts of the system?

Kutchera: Actually, most of the activity sits at two places. In injecting the beam from our test injector into the tandem, we go through a small aperture at the entrance to the preacceleration tube and we lose some activity there. The other place where activity sits is on the inside of the 90° magnet chamber – naturally from the other charge states which you don't pick up. But in the scattering chamber, I can't tell you. We looked once but we had a Faraday cup which was difficult to go into with the counter. Since the β-ray energy is so low you cannot measure from the outside, you have to go inside the cup. To answer your question in another way, we calculated that if you have 60 or 70 MeV carbon beam hitting a solid material the ions penetrate far enough into the solid so that the β-ray energies are almost completely attenuated, but this has not been checked experimentally.

Table I. CHARACTERISTICS OF THE ^{14}C-BEAM OPERATION

	Low activity pellet	High activity pellet
Starting material:	24 mg C with 1 mCi ^{14}C[a] + 350 mg Fe	20 mg C with 83.6 mCi ^{14}C[b] + 350 mg Fe
Activity in the Fe_3C pellet:	0.6 mCi	80 mCi
Activity measured at the surface:	∿ 1 μCi	∿ 100 μCi
Ion Source:	HICONEX 810[c] with Cs Lens[d]	HICONEX 810 with Cs Lens
Cs Temperature:	240° C	200° C
Average source output of negative mass 14 ions:	5 nA	300 nA
Total running time:	110 h	165 h
Beam on target (electrical):	1.5 nA, $^{14}C^{4+}$, 30 MeV	200 nA, $^{14}C^{5+}$, 54 MeV 30 nA, $^{14}C^{6+}$, 63 MeV
Summed activity carried by the beam:	∿ 2 μCi	∿ 200 μCi
^{14}C activity allowed to be ingested by a supervised person per year[e]:	6 mCi	6 mCi

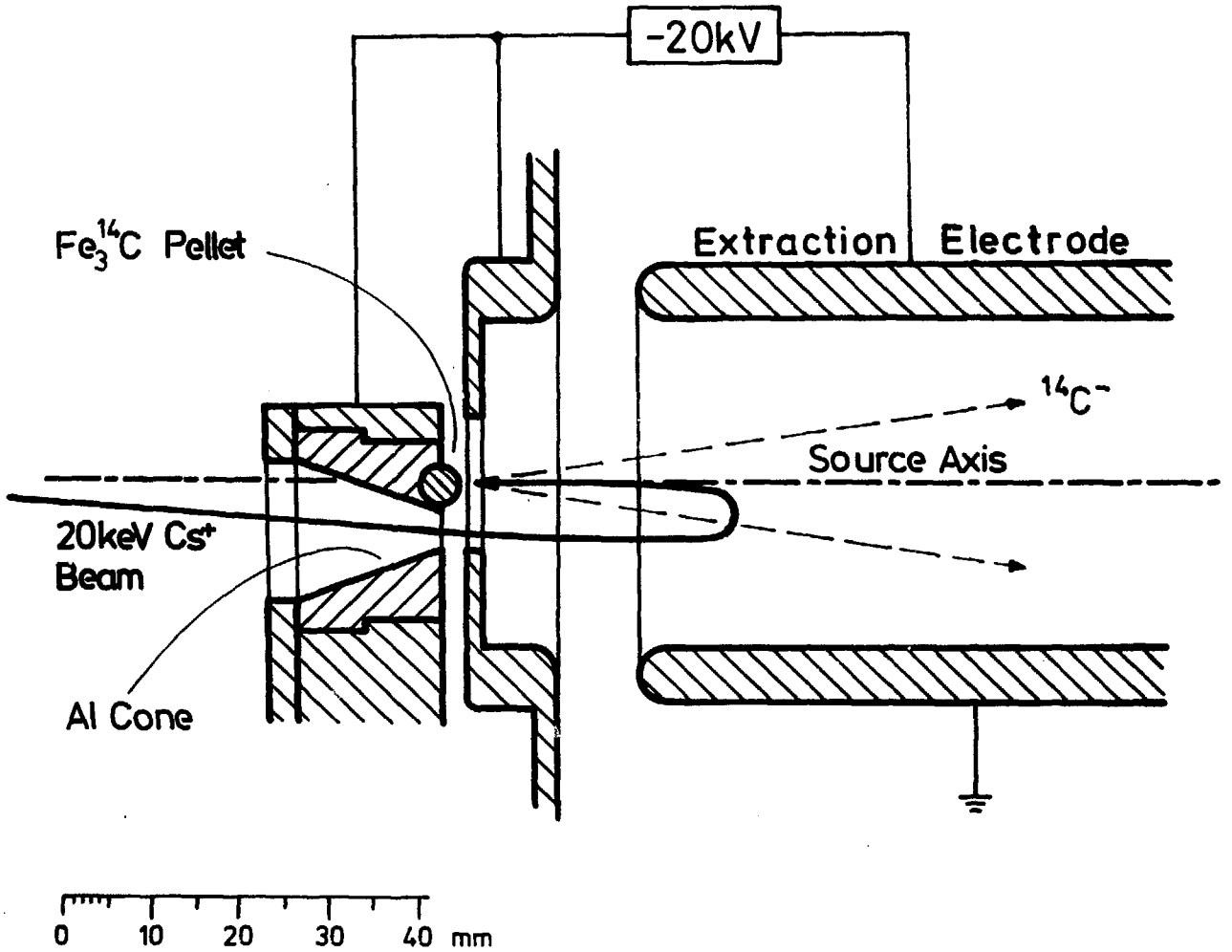
[a] Delivered by the Institute of Nuclear Research, The Radioisotopes Production and Distribution Centre, Swierk, Otwock, Poland.

[b] Delivered by New England Nuclear, 549 Albany Street, Boston, MA 02119.

[c] Manufactored by the General Ionex Corp., Newburyport, MA 01950.

[d] Home-made.

[e] According to the Federal Law of West Germany.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$Fe_3{}^{14}C$ Pellet
Extraction Electrode
$^{14}C^-$
Source Axis
20keV Cs^+
Beam
Al Cone
0 10 20 30 40 mm

FIG. 1

FIG. 2

FIG. 3

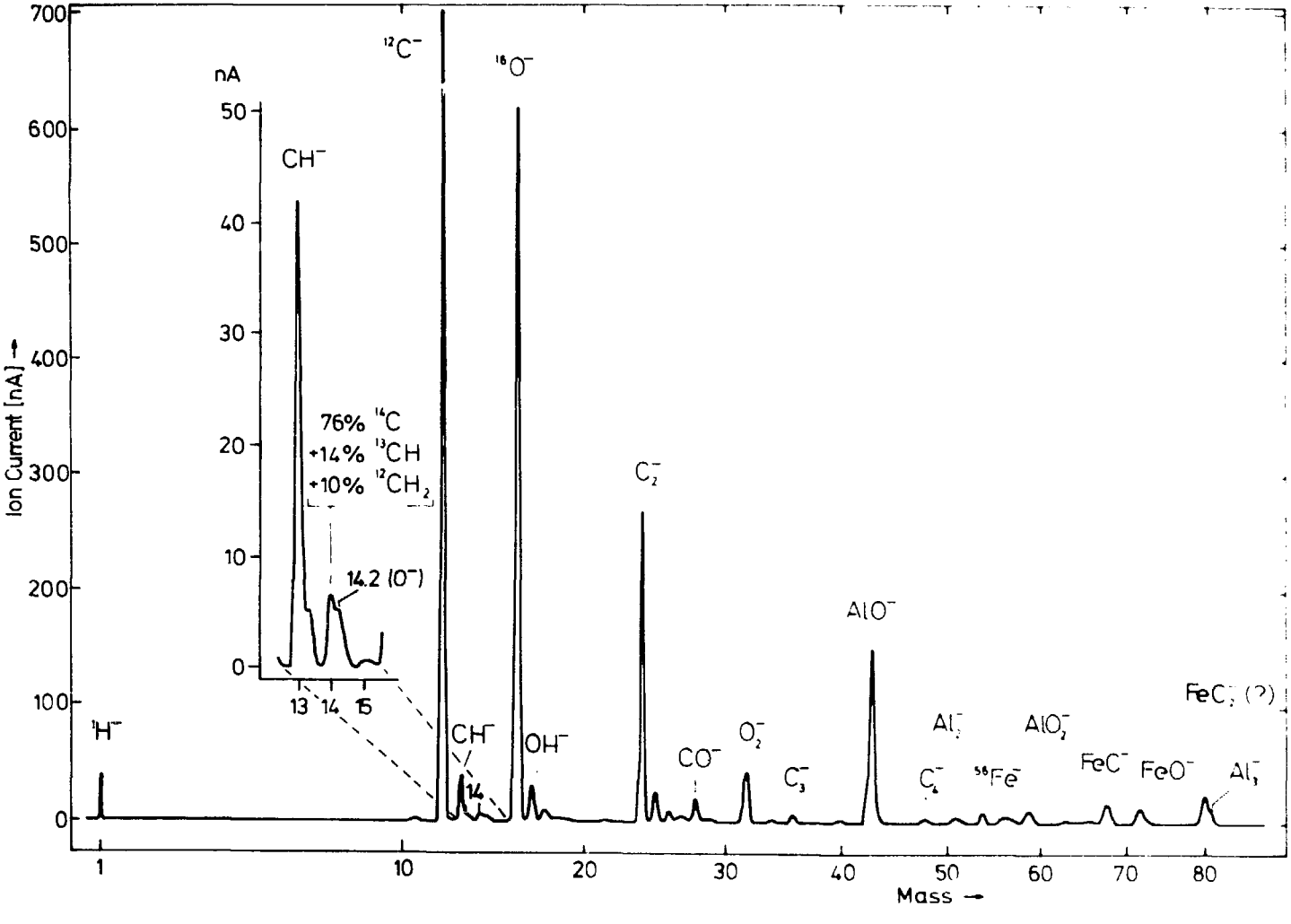

Ion Current [nA] →
700
600
500
400
300
200
100
0
nA
50
40
30
20
10
0
CH⁻
76% ¹⁴C
+14% ¹³CH
+10% ¹²CH₂
14.2 (O⁻)
13
14
15
¹²C⁻
¹⁶O⁻
C₂⁻
AlO⁻
¹H⁻
CH⁻
OH⁻
CO⁻
O₂⁻
C₃⁻
Al₂⁻
C₄⁻
⁵⁶Fe⁻
AlO₂⁻
FeC⁻
FeO⁻
Al₃⁻
FeC₂ (?)
1
10
20
30
40
50
60
70
80
Mass →

FIG. 4

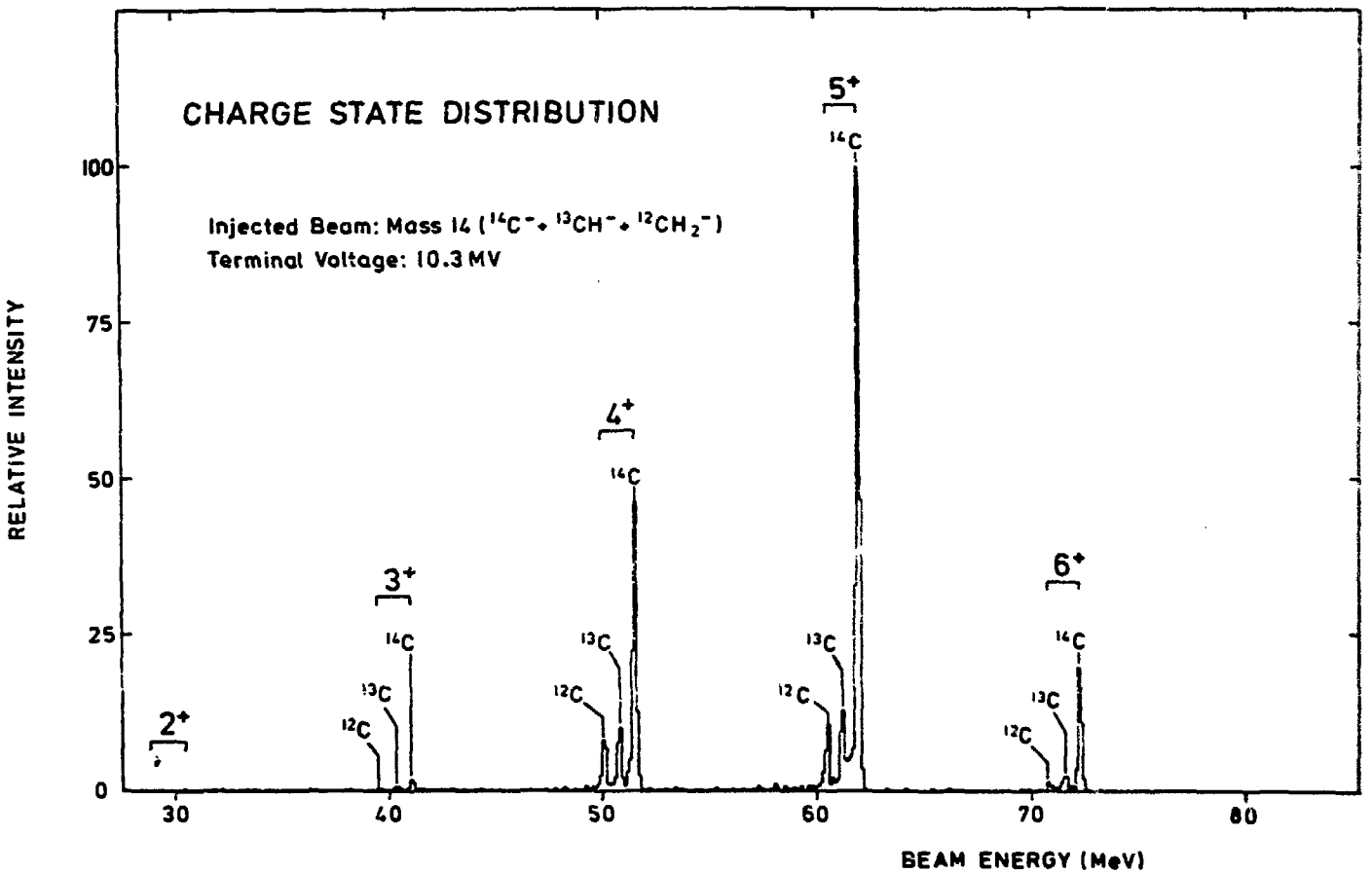

CHARGE STATE DISTRIBUTION
Injected Beam: Mass 14 ($^{14}C^-$ + $^{13}CH^-$ + $^{12}CH_2^-$)
Terminal Voltage: 10.3 MV
RELATIVE INTENSITY
BEAM ENERGY (MeV)
2+
3+
4+
5+
6+
^{14}C
^{13}C
^{12}C

FIG. 5

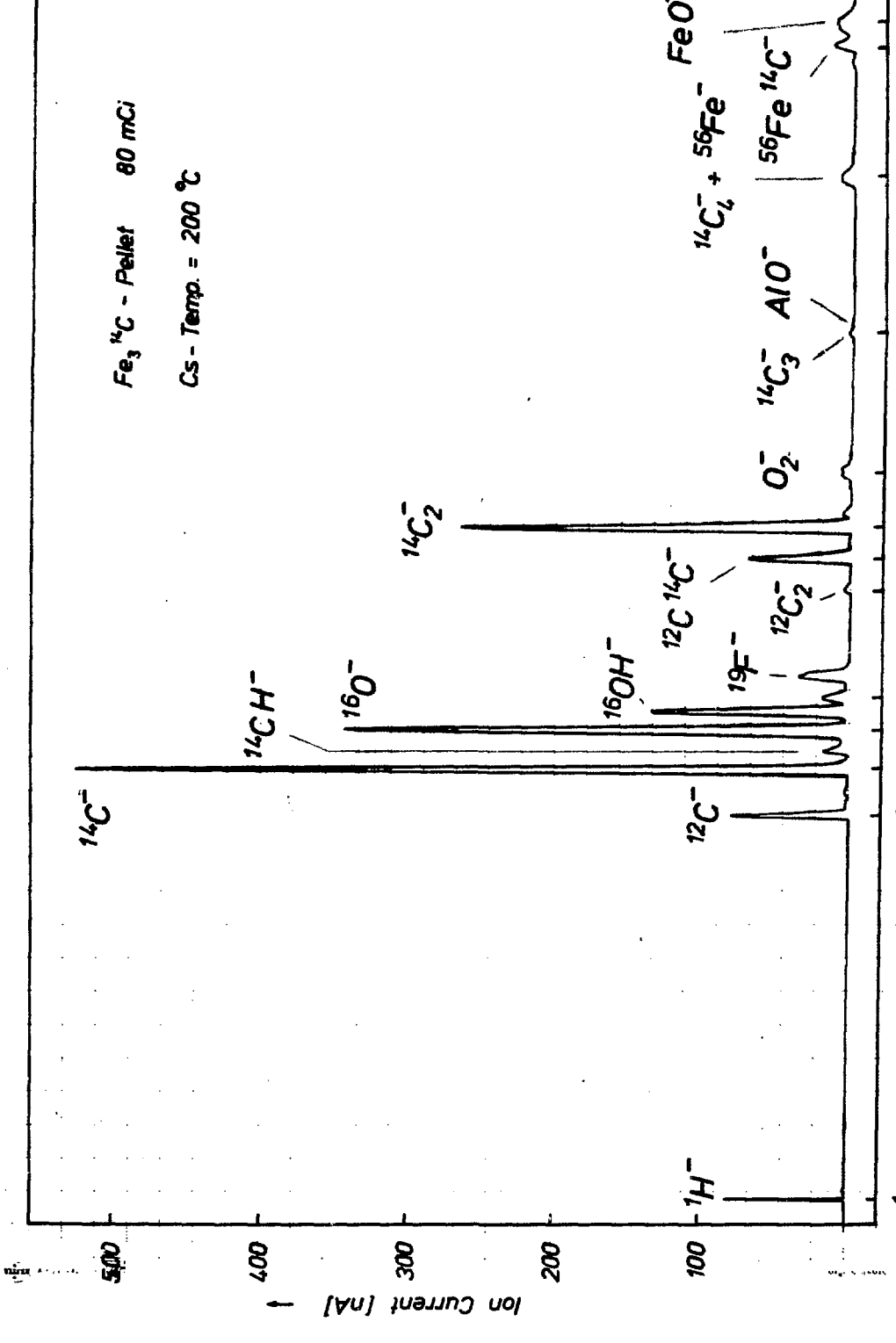

Fe3 14C - Pellet 80 mCi
Cs - Temp. = 200 °C
Ion Current [nA]
Mass →
1H⁻
12C⁻
14C⁻
14CH⁻
16O⁻
16OH⁻
19F⁻
12C2⁻
12C 14C⁻
14C2⁻
O2⁻
14C3⁻
AlO⁻
14C4⁻ + 56Fe⁻
56Fe 14C⁻
FeO⁻

FIG. 6

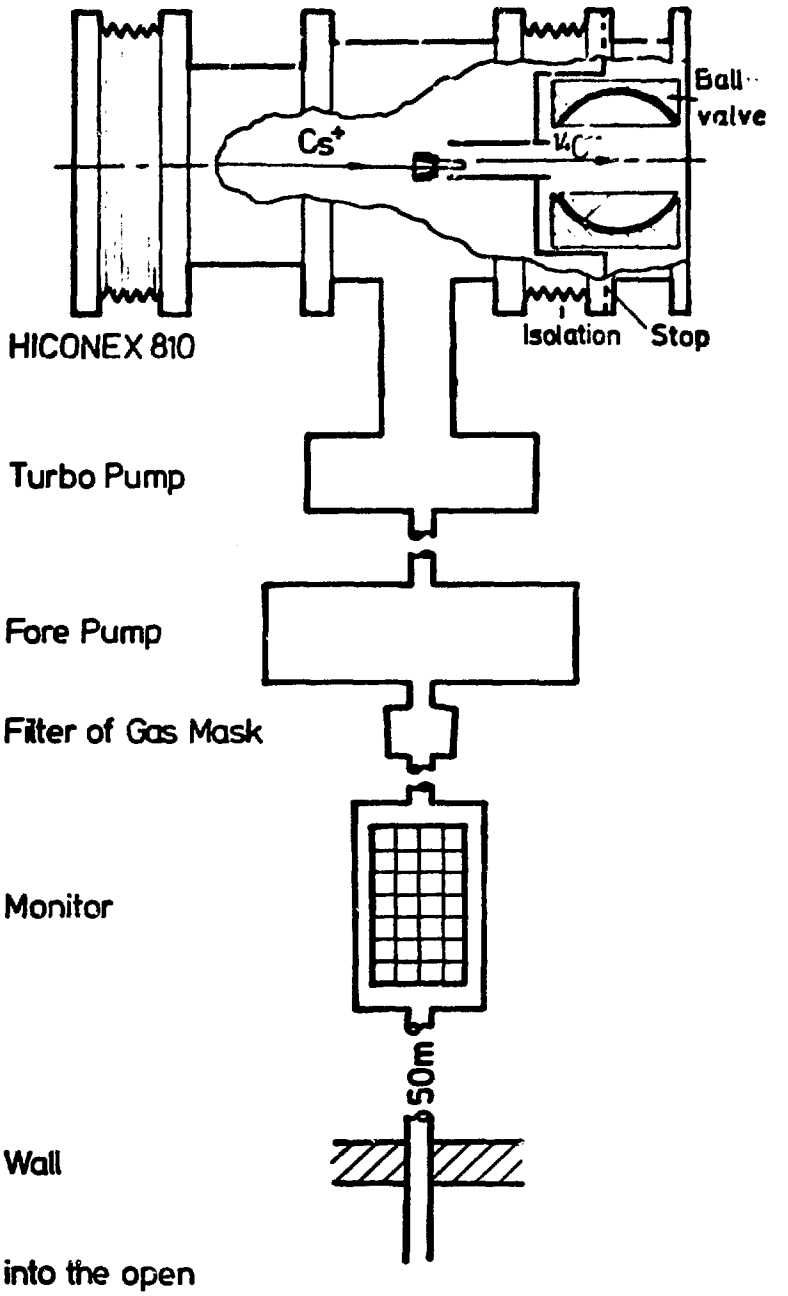

Ball
valve
Cs+
¼C
HICONEX 810
Isolation
Stop
Turbo Pump
Fore Pump
Filter of Gas Mask
Monitor
50m
Wall
into the open

FIG. 7

A Test of Usable Sputter Target Size in the Florida State University Type Inverted Sputter Ion Source

P. J. Billquist
Argonne National Laboratory
Argonne, Illinois 60439

At attempt to determine the smallest sputter target (cone) size usable to obtain reasonable negative ion beams, from materials difficult to obtain or handle, with the Argonne-built Florida State University type inverted sputter ion source, was made during mid-October 1978.

For this purpose, four test cones were prepared, each being made of graphite, with a steel mask with openings of 2, 4, 6 and 8 mm diameter respectively, to simulate cones with active areas of these sizes (Fig. 1).

The negative beam output of each cone was compared to a used, full-size, graphite cone (15 mm dia.) thru one of four different selectable apertures (3.2, 5.8, 10.6 and 19 mm.). The beam (C_{12}^-) was analyzed with the 40° inflection magnet to the low-energy Faraday cup (Fig. 2). The test was run twice. Once at low cesium temperature (low source current) (see Table I), and once at moderate cesium temperature (normal source current) (see Table II).

The data shows that the full-size cone performance was below average, and appeared to be contaminated.

Each cone was in position, with the source operating, for one hour, at which time the Faraday cup current was recorded through each

of the four selectable apertures. The beam line Einzel lens was kept at zero volts for all cases.

Although comparison of the performance of the test cones to the full-size (15 mm) cone is obviously not valid, we can conclude that cones of the 2-4 mm size can be used to achieve reasonable fractions of the maximum available beam from the Florida State University type inverted sputter ion source.

This work was supported by the U. S. Department of Energy.

TABLE I

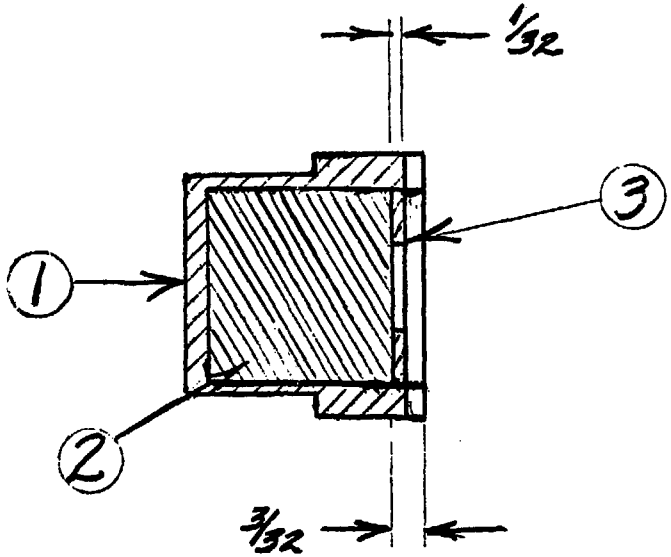
C^- beam current on L.E. Faraday cup (in n.a.) for o.3 ma Cs^+ beam as a function of C sputter target size and selected defining aperture.

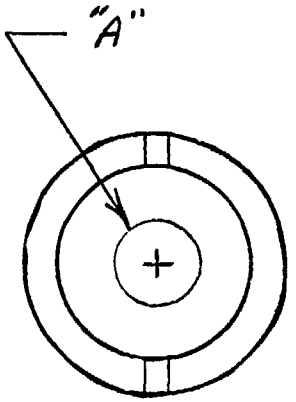
Sputter target (mm) \ Selected aperture (mm)	19	10.6	5.8	3.2
15	180	150	15	.8
8	163	125	78	38
6	115	80	48	22
4	86	64	34	15
2	70	46	24	9

TABLE II

C^- beam current on LE Faraday cup (in n.a.) for 1 ma Cs^+ beam as a function of C sputter target size and selected defining aperture.

Sputter Target (mm) \ Selected Aperture (mm)	19	10.6	5.8	3.2
15	570	350	170	68
8	870	500	225	88
6	780	450	190	77
4	540	320	143	57
2	370	190	80	32





DIMENSION "A"

1 EA	.080
1 EA	.160
1 EA	.240
1 EA	.320

1 CONE CUP (SUPPLIED)

2 GRAPHITE

3 STEEL

Fig. 1. Graphite Test Cone Inverted Sputter Source

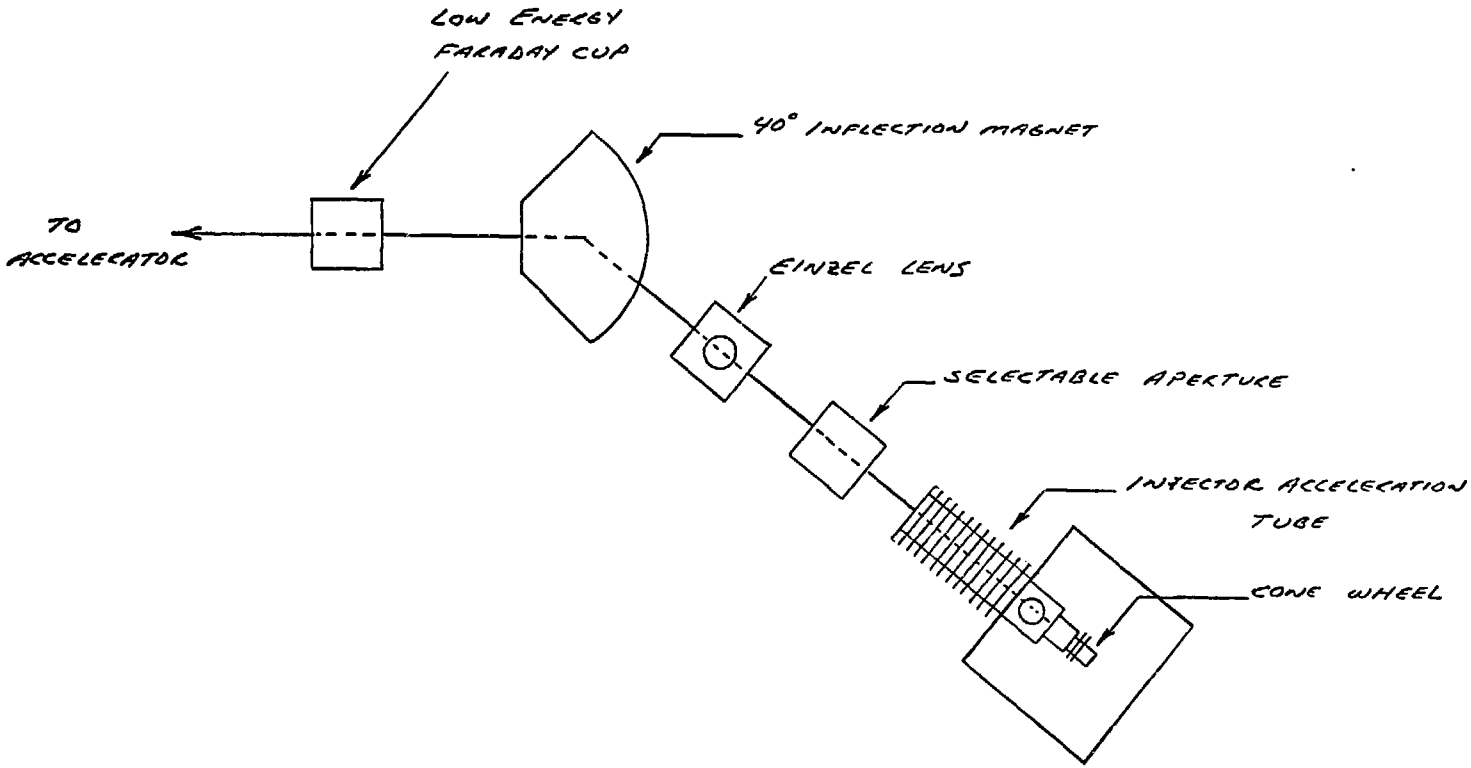


Fig. 2
TANDEM INJECTION LAYOUT

Session VI, Chairman, K. H. Purser
Editor, G. D. Alton

Recent Negative Ion Source Developments at ORNL

G. D. Alton
Oak Ridge National Laboratory*
Oak Ridge, Tennessee 37830

The advent of large tandem accelerators such as the 25 MV device under construction at ORNL has given additional impetus toward the development of negative ion sources which can meet the intensity, beam quality and species requirements of these research facilities. Tandems such as this, in conjunction with other acceleration devices, will have the capacibility of propelling many nuclei above the Coulomb barrier and thus extend the range of heavy ion species required of negative ion sources.

According to specifications written for the 25 MV ORNL tandem accelerator,[1] the ion source used during acceptance testing must be capable of producing a negative ion beam of intensity $\geq$ 7.5 μA within a phase space of $\leq$ 1 π cm-mrad $(\text{MeV})^{1/2}$. The specifications were written prior to the development of an ion source with such capabilities but fortunately Andersen and Tykesson introduced a source in 1975 which could easily meet the specified requirements.[2] The remarkable beam intensity and quality properties of this source has motivated the development of other sources which utilize sputtering in the presence of a diffuse cesium plasma[3-6] — some of which will be described in these proceedings. This report describes results of studies associated with the development of a modified Aarhus geometry and an axial geometry source which utilize sputtering in the presence of a diffuse cesium plasma for the production of negative ion beams.

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

The Modified Aarhus Geometry Source

The modified Aarhus geometry source, shown in Fig. 1, while retaining many of the design features of the original, differs significantly in certain aspects. Attempts have been made to improve the operational stability of the source by carefully designing and characterizing the cesium evaporation oven, improving insulator shielding and employing a method for producing a more uniform plasma discharge which, in turn, promotes uniform cathode wear. The latter aspect is extremely important for the efficient extraction of negative ions over an extended operational period.

General Design Features and Operational Characteristics

The plasma discharge is produced principally by electron impact ionization of the neutral cesium vapor as opposed to surface ionization which is the predominant mode of plasma production in the Andersen-Tykesson source. Either one of two 1½ mm tantalum filaments mounted in modular holders situated at each end of the discharge chamber may be used to produce a primary electron beam for initiating the plasma discharge. The two filament arrangement effectively doubles the lifetime of the source whenever the limiting lifetime factor is attributable to filament breakage. The discharge is collimated along an axis perpendicular to the negative ion extraction direction by a weak transverse magnetic field ($\sim$ 150 G).

Negative ions, produced by sputtering cathode material through a very thin cesium layer ($\sim$ 1 monolayer), are focused through a 2 mm aperture situated 15 mm from the spherical geometry cathode in the usual fashion. Typically sputter probe voltages of $\sim$ -1000 V at 10 mA of current are observed for optimum negative ion production from most cathode materials. Of course, lower or

higher voltages may be required depending on the sputter characteristics of the particular cathode material. Plasma discharge operational parameters of 50 V and 0.5 A are typically observed.

The source is also equipped with a mechanism for positioning the probe relative to the negative ion extraction aperture.

Cesium Oven Design

Cesium vapor is introduced into the discharge chamber by means of a carefully designed vapor transport system which effectively thermally decouples the oven from the discharge chamber. The cesium vapor is fed through a 5.9 cm long stainless steel tube of inner diameter 3/4 mm under conductance limited flow conditions. In the temperature regime of oven operation, the Knudsen flow formulation is appropriate.

The theoretically derived flow relation which incorporates the Knudsen flow formulation and an integrated equivalent of the Clausius-Clayperon equation derived from experimental vapor pressure measurements[7] is shown in Fig. 2. The open circles are measured relative flow rates observed by reversing the polarity of an axial geometry form of the source which operates exclusively in a surface ionization mode and measuring the analyzed positive cesium current as a function of oven temperature. The theoretical curve is seen to accurately represent the flow rate characteristics of the oven. The relatively slow increase in flow rate with oven temperature is very desirable from the standpoint of stable and controllable ion source operation.

A nominal source oven operational temperature of 200°C is characteristic of most elements (see Table I). This temperature corresponds to a flow rate of 0.7 mg/hr or 8.8×10^{14} particles/sec which correlates to a pressure in

the ionization chamber of 2×10^{-4} Torr. (The ionization chamber temperature is estimated to operate at 400°C.) At this temperature and pressure, a neutral density of 3.4×10^{12} particles/cm^3 and a plasma density of $\sim 1 \times 10^{12}$ ions/cm^3 exists within the discharge chamber.

The maximum negative ion yield is dependent on the deposition of approximately a monolayer of cesium on the sputter probe. Too little cesium will not minimize the work function while excess cesium may actually raise the work function above the minimum value and hinder as well the sputtering process. Both effects are detrimental to the production of negative ions and therefore the parameters which control or affect the cesium layer must be known for stable and efficient ion source operation. The flow rate, ionization chamber temperature and sputter probe voltage determine the effective thickness on the sputter probe which is essential in order to minimize the surface work function and thus maximize negative ion yields.

Yield Versus Cesium Oven Temperature

The oven temperature determines the rate at which vapor flows into the ionization chamber and is perhaps the most critical source operational control parameter. Figure 3 displays the relative negative ion current as a function of cesium oven temperature for a number of elements. The optimum oven temperature is observed to occur at $\sim$ 200°C for most sputter probe materials. The fact that the relative yields rise and fall rather abruptly is evidence of the importance of knowing the functional temperature dependence of the yield curves.

Yield Versus Sputter Probe Voltage

The probe voltage also affects the cesium layer thickness due to variation of the sputter rate with sputter probe voltage. A voltage too low relative to the arrival rate of neutral cesium on the surface will not optimize the negative ion yield from the standpoint of sputtering the probe material and/or the cesium away fast enough to maximize the negative ion yield. On the other hand, too high a voltage can sputter the cesium layer too fast which increases the work function and decreases the negative ion yield. These effects are illustrated in Fig. 4.

Magnetic Field Influence on Negative Ion Yield

The primary electron beam is collimated by the transverse magnetic field - the magnitude of which affects the electron beam trajectories and hence may affect the probability of positive ion formation. Another beneficial effect is through suppression of the electron current which would otherwise be extracted from the source.

A typical negative ion yield curve as a function of magnetic field is displayed in Fig. 5. The yield is seen to be rather weakly dependent on magnetic field which suggests that a permanent magnet could be used without compromising negative ion yield. This, of course, would further simplify the source.

Negative Ion Yield Data

Maximum negative ion yield data and source operational parameters which have been observed to data are shown in Table I.

The Axial Geometry Negative Ion Source

During evaluation of the Aarhus source, an axial geometry version of the source was designed. The source, shown in Fig. 6, is a natural evolutionary consequence from that of the Aarhus geometry because of the simplicity of the design. Adaptation from the Aarhus geometry simply required redesign of the discharge chamber. The source arrangement is very similiar to the Hill-Nelson positive sputter source.[7] Recently, Caskey *et al.*[3] have reported on a source similiar in design. The two sources differ in design detail and modes of source operation.

All aspects of the source except the ionization chamber and the modular filament holder arrangement are identical to those of the Aarhus geometry source. The modular filament holder has an electron reflector mounted between the exit aperture and the 1½ mm tantalum filament which discourages axially directed discharges.

The mode of plasma generation is solely that of surface ionization in contrast to the electron impact method used in the Aarhus source. The flow rate data shown in Fig. 2 were taken with the source operated in the positive ion mode. (Positive ion operation can be easily accomplished by polarity reversal of the pertinent power supplies.)

Yield Versus Cesium Oven Temperature

The yield versus cesium oven temperature are not sharply peaked as illustrated in Fig. 7 – in contrast to those measured from the Aarhus source.

Two modes of operation have been investigated. The C^- data shown in Fig. 7 were taken with a weak 150 G transverse magnetic field while in Au^- data were obtained without a magnetic field. Examination of the cathodes

after having run for 16 hours each revealed a uniform wear pattern for the C cathode and a steeply pitted wear pattern on axis of the Au cathode. The axial wear patterns suggests a very inhomogeneous plasma density having a maximum on axis. The transverse field appears to homogenize the plasma which, in turn, allows more uniform cathode wear. This effect is being investigated further using other cathode materials.

Yield Versus Sputter Probe Voltage

Figure 8 illustrates the negative ion yield dependence with sputter probe voltage for the two cases discussed above. The yields appear to increase linearly for Au^- (no magnetic field) and almost linearly for C^- (with a transverse magnetic field).

Yield as a Function of Probe Position from the Extraction Aperture

As indicated previously, the source is equipped with a mechanism for adjusting the probe position relative to the exit aperture. Figure 9 displays the results obtained for C^- and Au^-. The Au^- yield curve falls linearly with cathode position from the exit aperture while the C^- curve reaches a maximum whenever the sputter cathode is positioned a distance away from the aperture equivalent to the radius of curvature of the cathode. This data further confirms that the cathode wear pattern is uniform.

Discussion and Conclusions

The modified Aarhus geometry source has been operated continuously for periods of time up to $\sim$ 400 hours using an Au cathode and $\sim$ 80 hours using an Ag cathode. In both cases, the source ran until the probes were completely erroded away. The source thus far has demonstrated surprising operational

stability and appears to have a very long lifetime. The stability of operation is largely attributable to the cesium oven design and rather detailed knowledge of negative ion yields as a function of cesium oven temperature.

The axial geometry source appears to be less stable and requires higher sputter probe voltages than the Aarhus geometry source at this point in time. The materials investigated thus far indicate that the yields are equivalent to those obtained from the Aarhus geometry whenever the source is operated with a transverse magnetic field, but lower whenever the source is operated without a magnetic field. In this mode of operation, a strongly pitted axial wear pattern is observed. Further investigations with this source geometry are continuing.

References

1. C. M. Jones *et al.*, Technical Specification for a 25 MV Tandem Electrostatic Accelerator, ORNL-TM-4942 (1975).
2. H. H. Andersen and P. Tykesson, IEEE Trans. Nucl. Sci. NS-22, 1632 (1975).
3. G. T. Caskey, R. A. Douglas, H. T. Richards, and H. V. Smith (to be published).
4. R. Middleton (these proceedings).
5. J. H. Billen and H. T. Richards (these proceedings).
6. G. V. Chemyakin, A. G. Troshikhin, "A Negative Heavy Ion Source with Cathode Sputtering," ERDA-TR-236 (1976).
7. K. J. Hill and R. S. Nelson, Nucl. Instrum. Meth. 38, 15 (1965).
8. A. N. Nesmeyanov, "Vapour Pressure of the Elements," Trans. and Edited by J. I. Carasso, Academic Press, New York (1963).

DISCUSSION:

Tykesson: Do you have any idea of the temperature of your sputtering cathode?

Alton: I do know that it is greater than 360°C because we have melted a lead cathode. We have observed cathodes visually and they appear to glow a very dull red. I would anticipate maybe 400-500°C or so, but I don't know. We haven't measured the cathode temperature.

Table 1

Maximum Negative Ion Yield Data (Aarhus Source Geometry)

Ion	Probe Material	Probe Voltage (V)	Arc Conditions Voltage (V)	Arc Conditions Current (A)	Oven Temp (°C)	Negative Ion Yield (uA)	Support Gas
H^-	Ni	80	175	1.1	175	176	H_2
BeO^-	Be	1200	50	0.75	195	25	O_2
C^-	C	1500	65	1.0	235	20	H_2
Al^-	Al	1000	75	0.6	165	2.3	H_2
P^-	GaP	1100	40	0.6	190	8	Ar
S^-	Be	1500	55	0.45	195	26	CS_2
Cl^-	Be	1500	45	0.50	195	33	CCl_4
Cl^-	NaCl	2000	50	0.50	180	20	H_2
TiH_3^-	Ti	800	65	0.8	195	2.5	H_2
FeH^-	Fe	900	60	0.55	198	2.8	H_2
Ni^-	Ni	900	40	0.75	210	54.6	Ar
Cu^-	Cu	1000	45	0.75	200	51	Ar
Se^-	CdSe	1000	40	0.40	197	7.8	Ar
Ag^-	Ag	950	37	0.55	190	36	H_2
Pt^-	Pt	1100	35	0.6	195	~40	H_2
Au^-	Au	1000	100	0.4	200	55	Ar

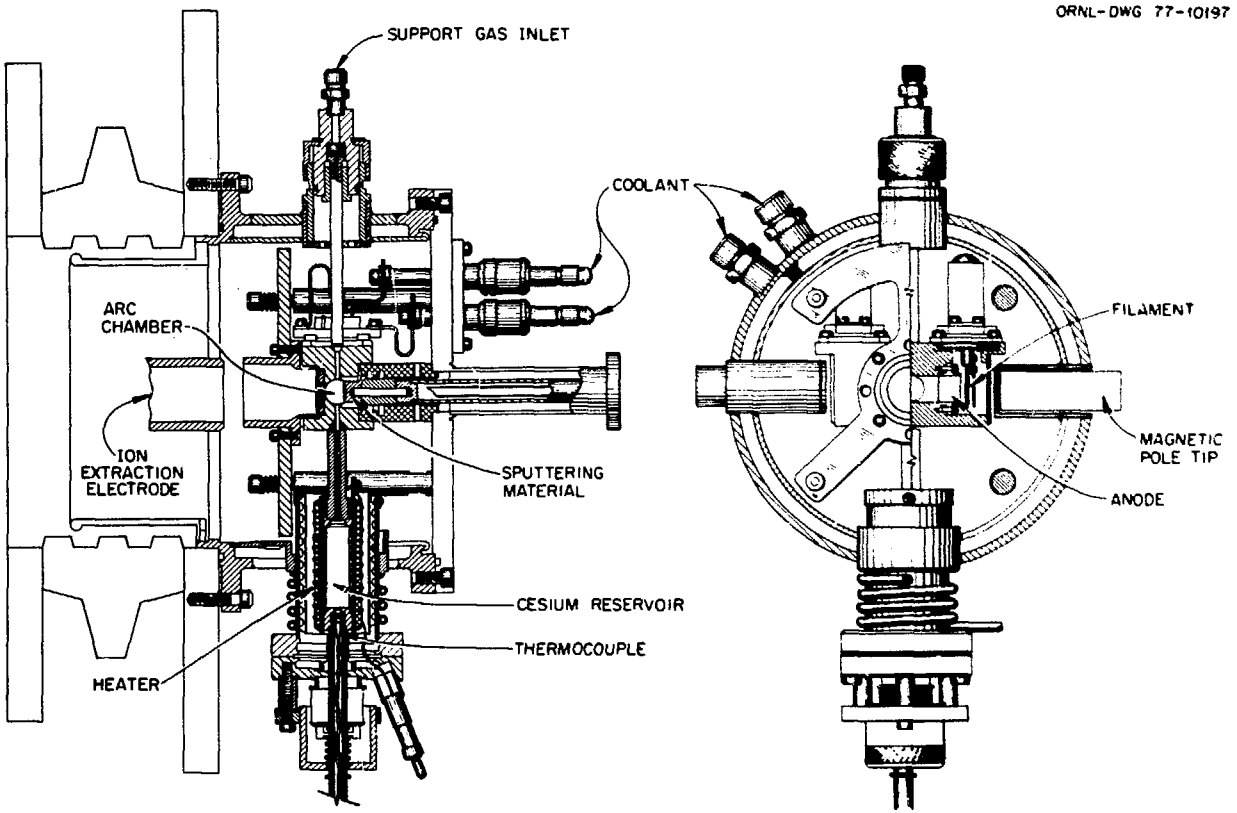


Fig. 1. Modified University of Aarhus Source.

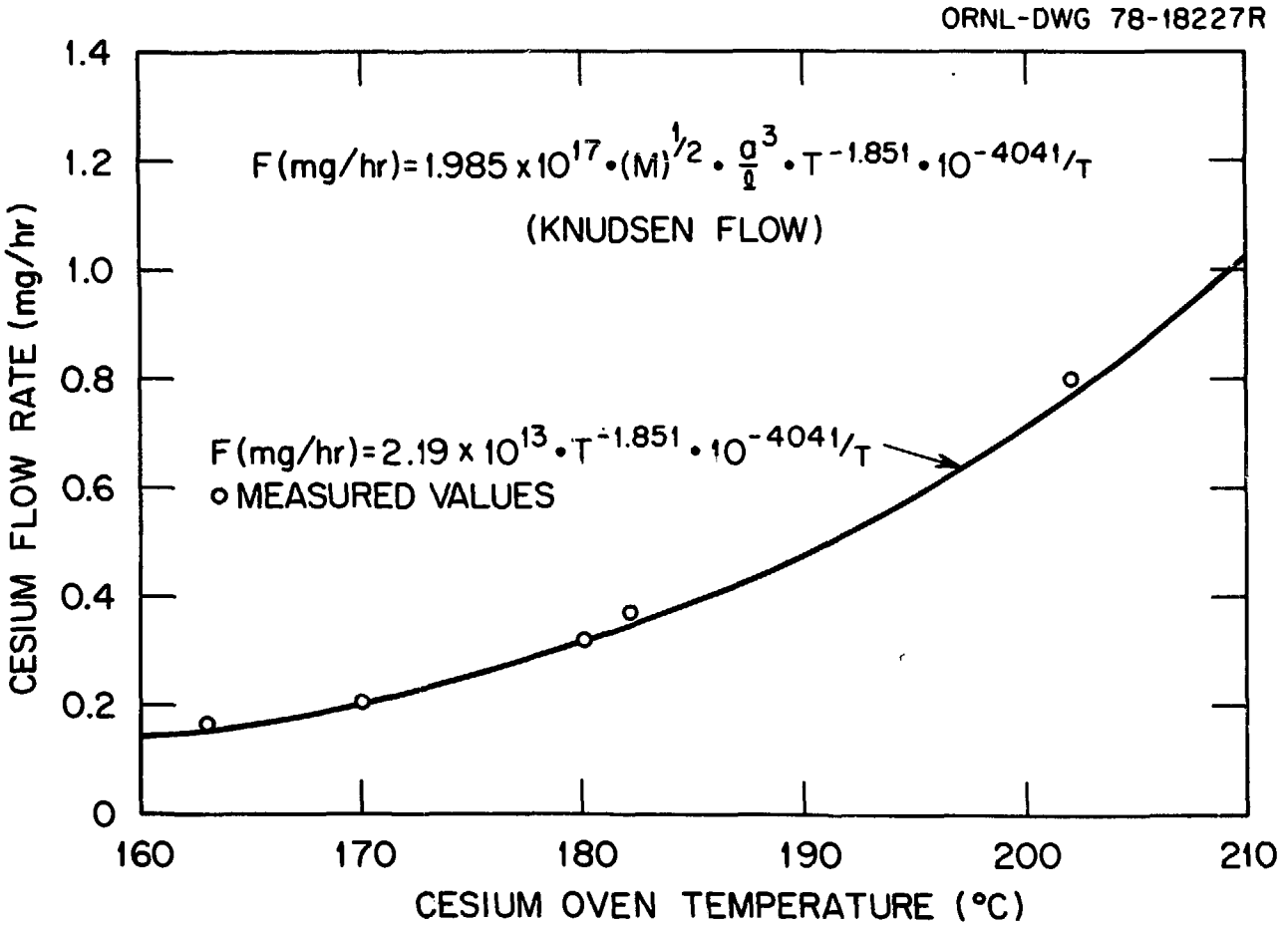


Fig. 2. Cesium Flow Rate as a Function of Oven Temperature. (The solid line is a theoretically derived curve; the open circles are measured values.)

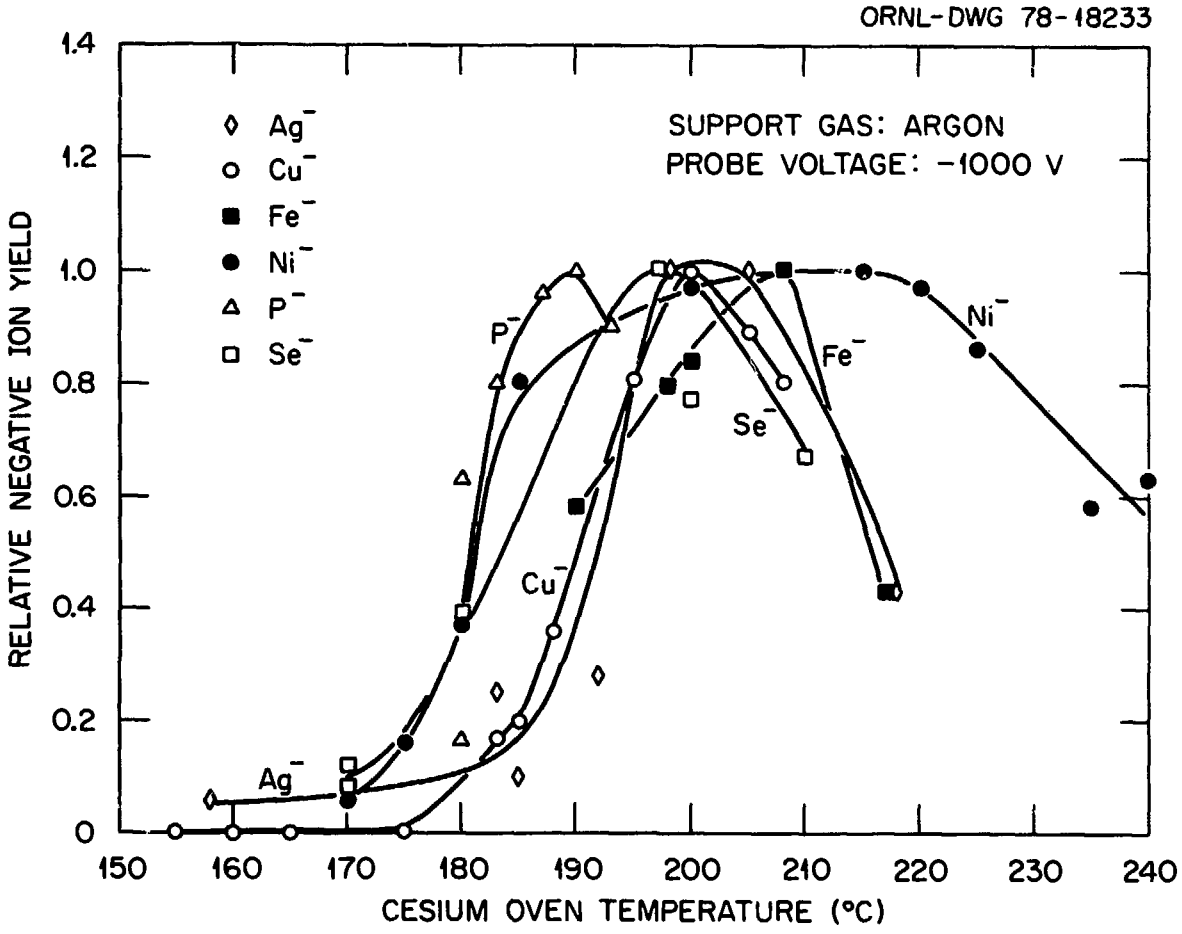


Fig. 3. Relative Negative Ion Yields as a Function of Cesium Oven Temperature.

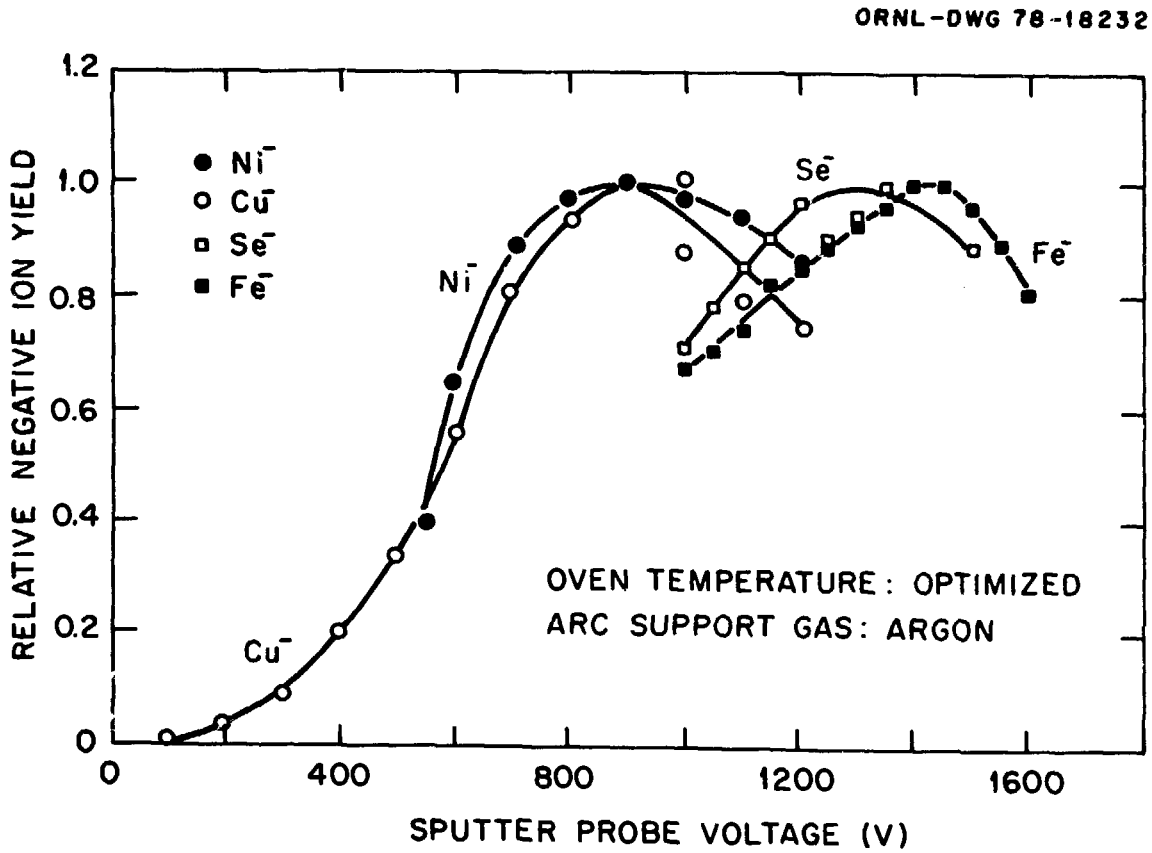


Fig. 4. Relative Negative Ion Yield as a Function of Sputter Probe Voltage.

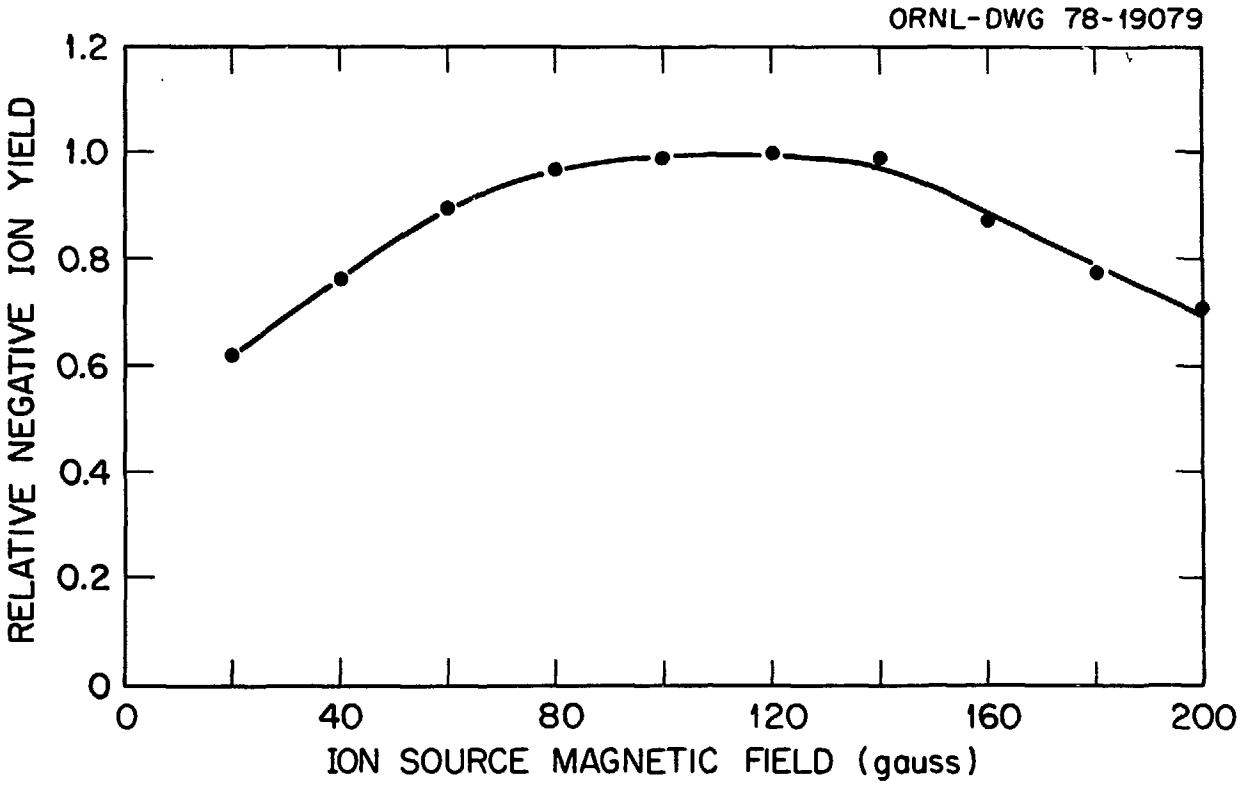


Fig. 5. Typical Relative Negative Ion Yield Behavior with Magnetic Field.

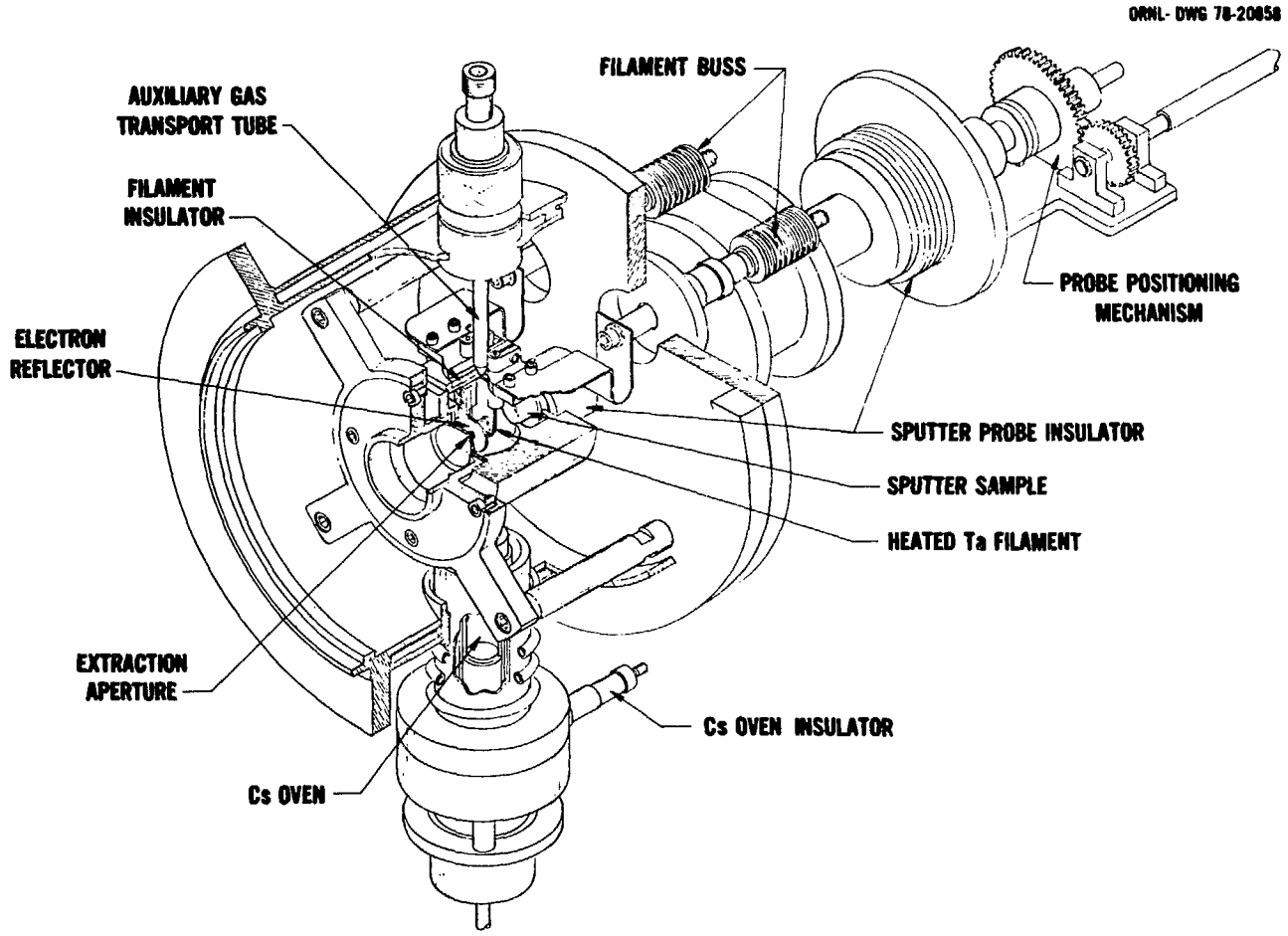


Fig. 6 **Axial Geometry Negative or Positive Ion Source.**

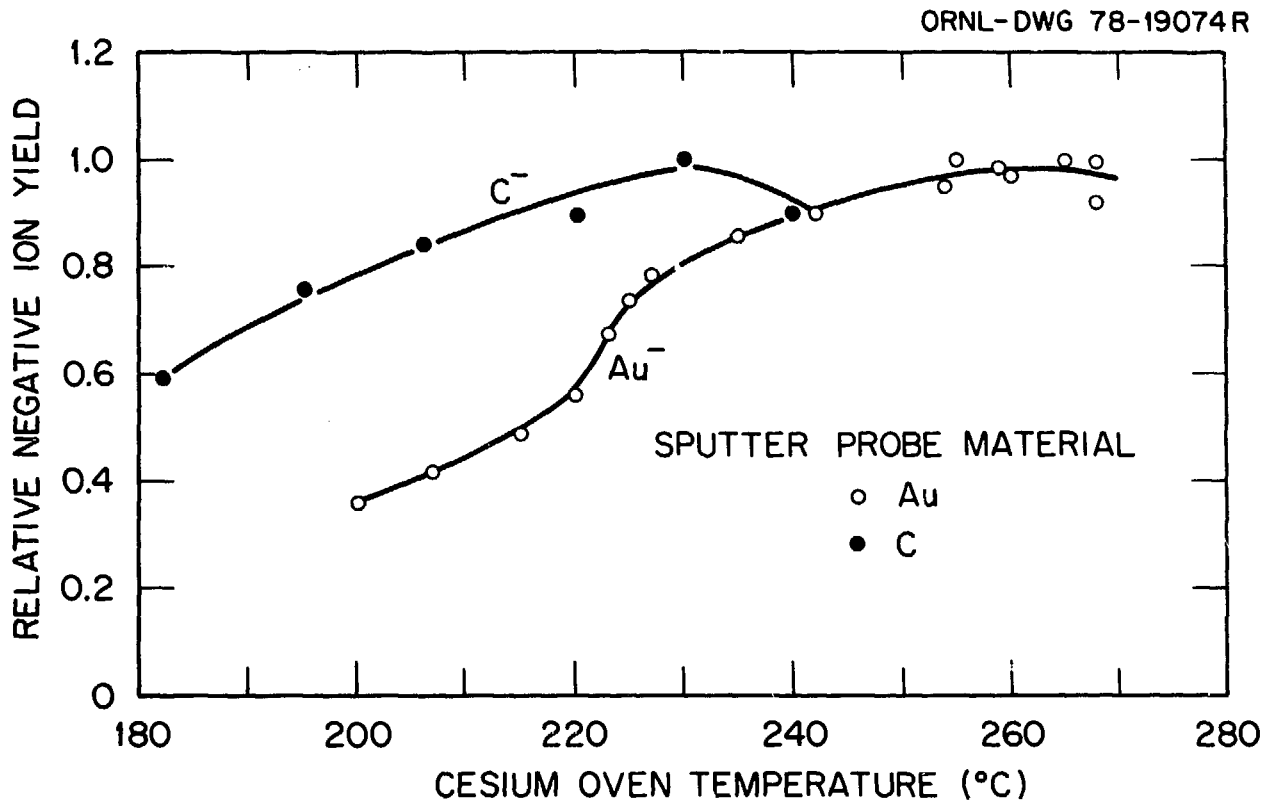


Fig. 7. Relative Negative Ion Yields as a Function of Cesium Oven Temperature (Axial Geometry).

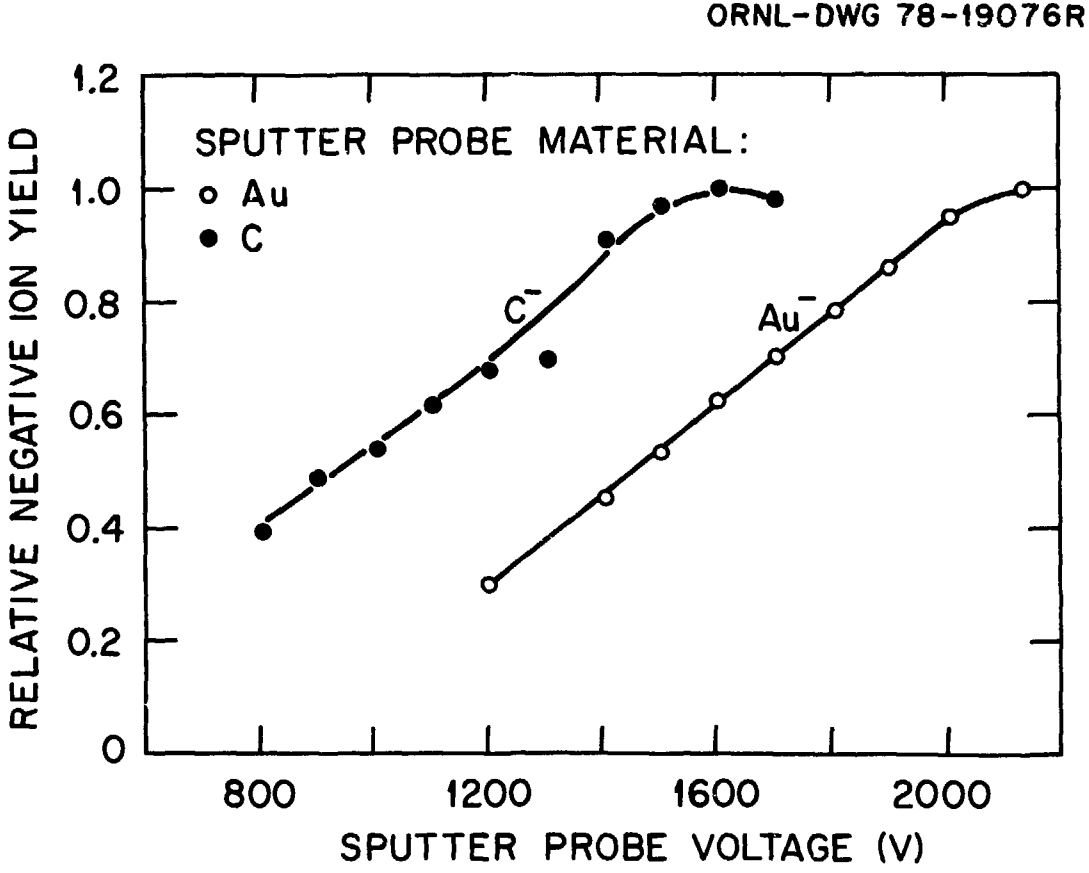


Fig. 8. Relative Negative Ion Yields as a Function of Sputter Probe Voltage (Axial Geometry Source). (C^- with Transverse Magnetic Field, Au^- without Magnetic Field.)

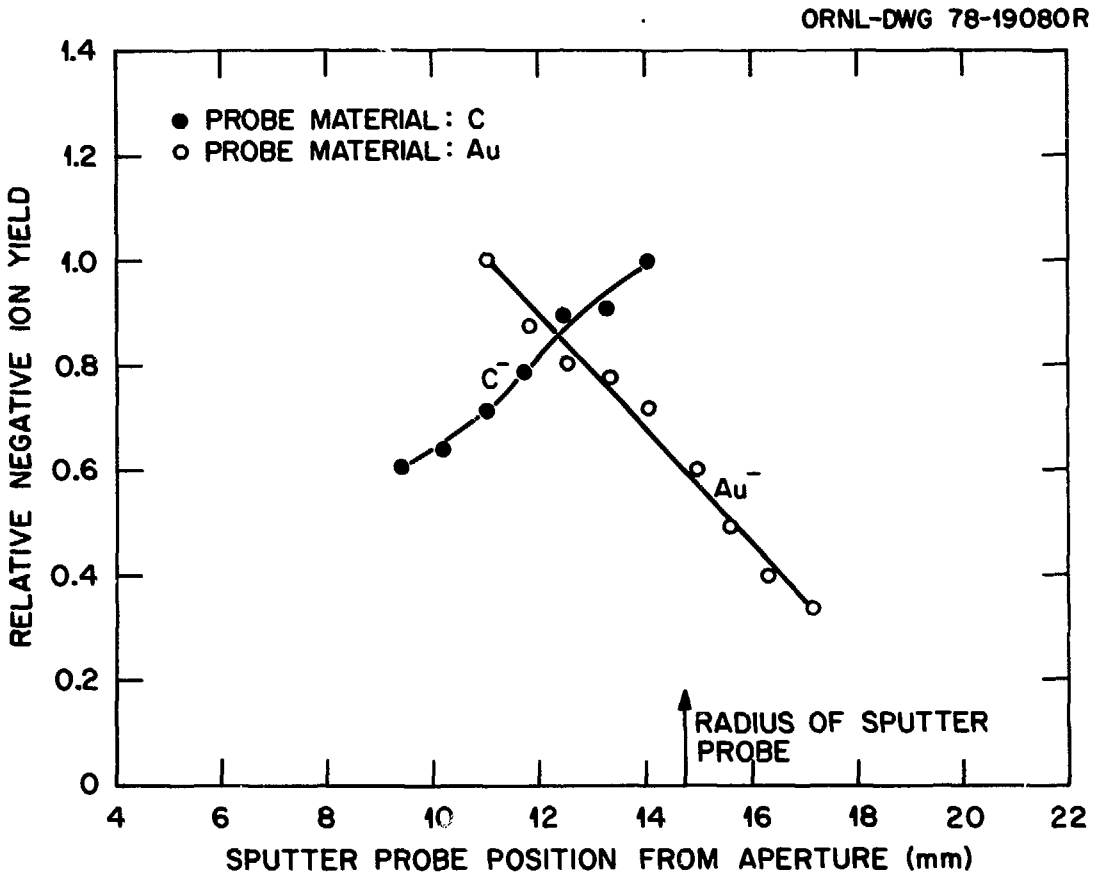


Fig. 9. Relative Negative Ion Yields as a Function of Sputter Probe Position from Aperture (Axial Geometry Source). (C^- with Transverse Magnetic Field, Au^- without Magnetic Field.)

The Production of Negative Heavy Ion Beams through Charge Exchange Processes

P. Tykesson
Institute of Physics, University of Aarhus
DK-8000 Aarhus C, Denmark

1. Introduction

Charge exchange of an initially positive ion beam in an electron-donor target was the dominant method for the production of negative ion beams for the first tandem accelerators. At that time, gas targets were the most common electron donor, but in 1967, Donally and Thoeming[1] pointed out that particularly large negative fractions were to be expected from charge exchange in metal vapours. They regognized the importance of projectile-target combinations with minimum energy defects in the two separate electron-capture processes at the stepwise formation of negative ions from positive ones. These exchange processes have been studied extensively for beams of hydrogen and helium in a number of metal-vapour targets[2,3], mainly for the design for tandem accelerators of polarized ion sources and negative helium-ion sources.

Systematic investigations of the formation of heavy, negative ions by charge exchange in metal vapours were apparently limited to beams of Li, C, O, and Cl[4-6]. Only a few charge-exchange ion sources for negative, heavy ions have been reported. An ion source[7], which produces negative, heavy ions by charge-exchange in potassium vapour from positive ions from a mercury arc pool duoplasmatron, yields μA beams of elements like iron and aluminum. This source has been used for several years in the tandem accelerator at Harwell, England. Freeman et al.[8] combined a slit-type separator ion source with a lithium-vapour, charge-exchange target and at 40 keV charge-exchange energy, obtained high-intensity, negative ion beams of a wide range of elements ($\geq$200 μA Te^-;

≥25 μA Fe^-; ≈ 1 μA U^-). The first systematic investigation of the formation of heavy, negative ions by charge exchange in metal vapours was made by D'yachkov and Zinenko[5], who measured the negative fractions of (10-100 keV) C, O, and Cl beams after passage of Mg and Zn targets. They observed charge-exchange efficiencies as high as 25%.

In view of these encouraging results and the very high yields of negative, heavy ions reported in Ref. 8, Heinemeier and Hvelplund[9,10] at the University of Aarhus started systematic investigations of equilibrium charge-state distributions for a wide range of 15-90 keV ion beams passing through Mg and Na vapours. The results of these investigations confirmed the expectations of high negative fractions, generally far above the 1% range, which has been assumed typical in negative-ion source review articles[11,12], and it was shown by Heinemeier and Tykesson[13,14] that useful intensities for tandem acceleration of a majority of the chemical elements could be produced using standard universal separator ion sources for the production of positive ion beams. During the last year, our systematic investigations of charge exchange in alkali vapour targets have been enlarged by Greenway et al.[15] by including measurements in K and Cs. This means that to-day we have a fairly good knowleuge of how to optimize the charge-excahange conditions, which, together with the discussion and estimation of the expected scattering angles in Ref. 9, are of extremely importance for the design of charge-exchange ion sources for negative, heavy ion beams.

2. Charge-exchange measurements in metal-vapour targets

2.1. Apparaturs and measuring procedure

Figure 1 shows a schematic drawing of the apparatus used for the systematic charge-exchange investigations in metal-vapour targets carried out at Aarhus. A positive ion

beam from a 100 kV heavy-ion accelerator is collimated by a pair of apertures before entering a metal-vapour cell. The first fixed 0.5-mm diam. aperture is 600 mm away from the second variable aperture, usually set at 0.2 × 0.2 mm. After passage through the metal-vapour target and through a second, adjustable aperture, the charge-state components of the emergent beam are electrostatically separated and recorded simultaneously by a position-sensitive detector of secondary-emission, single-particle-counting type[16].

The temperature is measured with a chromel-alumel thermo-couple mounted in the wall of the cell close to the molten metal; the metal-vapour pressure is calculated from a compilation by Nesmeyanov[17].

The fraction F_i of the beam emerging from the cell in charge state i is found by normalizing to the total transmitted beam,

$$F_i = \frac{N_i}{\Sigma_i N_i} \quad ; \; i=-1,\ 0,\ 1,$$

where N_i is the number of particles in charge state i recorded in a charge-state spectrum.

2.2. Charge exchange

The nature of competing electron-capture and -loss processes in charge-changing collisions with the atoms of the target leads to a statistical distribution on charge states of the ions in the emergent beam. At sufficiently high target thickness, equilibrium between the competing processes is attained, and the resulting equilibrium charge-state distribution is independent of the target thickness. Figure 2 shows a typical example of such a charge-state distribution. Most of the reported equilibrium charge-state fractions $F_{-1\infty}$ are taken from the flat portion of the "growth" curves, while μ_∞

is defined as the target thickness required for $F_{-1}(\mu)$ to reach 90% of its equilibrium value $F_{-1\infty}$.

According to Ref. 9, the cross sections σ_{1-1} and σ_{-11} as well as the positive equilibrium fractions $F_{1\infty}$ are sufficiently small for the equilibrated beams to be treated as a two-compnent system,

$$\frac{F_{-1\infty}}{F_{0\infty}} \simeq \frac{\sigma_{0-1}}{\sigma_{-10}} \qquad (1)$$

which makes it possible to explain the energy behaviour of $F_{-1\infty}$ by the Massey adiabatic criterion[18]. Since the loss cross section σ_{-10} varies slowly with energy, a maximum in the energy dependence of the capture cross section σ_{0-1} will be reflected in a maximum in $F_{-1\infty}$. According to the Massey adiabatic criterion, the velocity at the maximum in the capture cross section σ_{0-1} is

$$v_{max} \simeq \frac{a \cdot \Delta E_{0,-1}}{h} \quad , \qquad (2)$$

where

$\Delta E_{0,-1} = E_I$ (target-ionization energy) $- E_A$ (projectile electron affinity)

is the energy defect in the capture process and a is the interaction distance (see Ref. 18) which depends on the projectile-target combination. The expression reveals that the velocity of the projectile determines the maximum in $F_{-1\infty}$, which means that the maximum will appear at a low energy for light projectiles or for elements with high electron affinity (small energy defect).

2.3. Negative equilibrium fractions $F_{-1\infty}$

Figure 3 shows the equilibrium fractions $F_{-1\infty}$ measured in Mg within the energy range of 15 to 90 keV (Ref. 9). The projectiles represent a wide scan in atomic number (4-79) and electron affinity (0.28-3.6 eV). The maximum in negative equilibrium fractions $F_{-1\infty max}$ ranges between ~1% and ~90%, with a nearly monotonically increasing dependence on the projectile electron affinity (given in brackets).

Figure 4 shows the equilibrium fractions $F_{-1\infty}$ measured in Na vapour for the same projectiles and same energy range (Ref.10). Compared to Mg, Na produces higher negative fractions but a lower energies, except for the high-electron-affinity element Cl, which shows a much lower negative yield in Na. Notable are the negative fractions of 3% for Be (E_a=0.24 eV) and 0.5% for Ca (E_a unknown) as negative ions of these elements are assumed to exist in doubly excited metastable states only[19] and are very difficult to produce by other methods such as sputtering or direct-extraction ion sources. Because of problems of obtaining "clean" positive Ca beam from the 100 kV accelerator, the charge-conversion factor at 20 keV for this element was measured on a test bench for ion sources, where a Faraday cup was used for beam detection. The test bench was also used to set an upper limit of 10^{-6} to the conversion efficiency for Mg^- by directing a 150 µA beam of 20-keV Mg^+ through Na vapour.

In Fig. 5, the negative equilibrium fractions $F_{-1\infty}$ are plotted as a function of projectile velocity for a selection of elements charge-equilibrated in Cs. From the table in Fig. 6, it can be seen that the projectiles represent a number of group-I, III, -IV and -V elements in the main body of the periodic table. Many of these elements are difficult to produce with existing negative ion sources as negative-ion beams within the µA range

Group-I elements show an interesting systematic behaviour. For elements, where maxima are obtained within the investigated energy range, these appear at the same velocity and coincide with the velocity corresponding to $F_{-1\infty_{max}}$ for protons taken from Ref. 2. From the figure, the maxima for Li and Cs can be estimated to appear at ~ 4 and ~ 100 keV, respectively. The maximum negative fractions for group-I elements show a monotonically increase with electron affinities.

The group-III elements B, Al, Ga, and In all show maximum negative fractions ~ 10%, which is higher than for most of the group-I elements, in spite of lower electron affinities.

Besides higher negative fractions, there is a shift to lower velocities for group-IV elements compared to group-I and - III elements. This is in good agreement with Eq.2, where a lower projectile velocity for $F_{-1\infty_{max}}$ is predicted if the energy defect is small (high electron affinity).

In spite of lower electron affinities, the investigated group-V elements P and As, show higher negative fractions than do group-IV elements.

For negative-ion-sources applications, it is more relevant to know how $F_{-1\infty}$ varies with projectile energy, and hence in Fig. 7, $F_{-1\infty}$ is plotted as a function of energy for the same projectiles as in Fig. 5.

In order to compare $F_{-1\infty}$ for different metal vapours, $F_{-1\infty}$ is shown in Figs. 8-10 as a function of energy for elements investigated in more than one metal-vapour target. For most of the elements, the largest $F_{-1\infty}$ values are obtained in Cs. Exceptions are found for the elements in Fig. 9, where $F_{-\infty_{max}}$ for B, Al, and Fe are approximately the same in all tested alkali targets, and the metastable Be^- ion actually shows its highest negative fraction in Na.

The investigations of charge exchange in metal vapour have given a picture of the systematic behaviour of the negative equilibrium fraction $F_{-1\infty}$, which can be summed up as follows:

1. $F_{-1\infty_{max}}$ velocity dependence makes that the energy for $F_{-1\infty_{max}}$ increases with increasing mass of the projectile (see Fig. 7).

2. $F_{-1\infty_{max}}$ seems to follow the prediction of the energy-defect dependence in the Massey adiabatic criterion,
   i) for the same charge-exchange target, $F_{-1\infty_{max}}$ appears at lower energies for projectiles with high electron affinities (see Fig. 5).
   ii) for the same projectile, $F_{-1\infty_{max}}$ will shift to lower energies when targets with lower ionization energy are used (see Fig. 8 to 10).

3. For most of the elements, the highest charge conversion is achieved in Cs, and for the investigated elements, the following maximum negative fractions have been obtained:

   0.3 - 9% for group-I elements
   8 -12% for group-III elements
   15 -60% for group-IV elements
   80 -90% for group-V elements.

3. Scattering effects

For accelerator applications, it is important to consider the beam degradation due to scattering of the beam during its passage through the electron donor target. This problem has been extensively discussed in Refs. 9 and 20 and should be only briefly reported on here.

The scattering effects associated with the passage of a beam through matter is strongly dependent on target thickness u, and in

Ref. 9, an expression for the angular halfwidth at half maximum $\alpha_{1/2}$ of the emergent beam is estimated as a function of μ, projectile and target atomic number, and beam energy for an initially parallel beam,

$$\alpha_{1/2} = \chi \frac{\mu^{2.5}}{E} \tag{3}$$

with $\alpha_{1/2}$ in radians, μ in atoms/cm^2, E i keV; χ is given by

$$\chi = 9.7\times10^{-42} Z_1 Z_2 (Z_1^{2/3}+Z_2^{2/3})^{-2} \tag{4}$$

where Z_1 and Z_2 are projectile and target atomic number, respectively. The estimate is based on multiple-scattering calculations by Sigmund and Winterborn[21], and measurements in the relevant μ range are reported in Ref. 20.

In Fig. 11, χ is plotted as a function of the atomic number of the projectile Z_1 for the investigated targets, and from these curves $\alpha_{1/2}$ can easily be calculated if the charge-exchange energy E and the target thickness μ are known. However, the estimated halfwidth is of practical value only in combination with a knowledge of the full multiple-scattering profiles which, in the low target-thickness range in question, are sharply peaked and have wide wings. In Fig. 12, the scattered beam $T(\alpha)$ contained inside a cone of half angle α with respect to the beam axis, has been plotted as a target-thickness independent function α in units of $\alpha_{1/2}$.

In Fig. 11, it can be seen that the scattering effects are only weakly dependent on projectile (or target) atomic number, increasing by a factor of 2 only from Li to Au. This result is important since it means that the charge-exchange technique is nearly equally well suited for light- and heavy-ion beams. The dependence on target thickness μ is strong ($\propto\mu^{2.5}$), but as long as the target thickness does not exceed

2×10^{15} atoms/cm^2, which is a very typical equilibrium target thickness, the scattering effects are very moderate if the charge-exchange energy is $\geq$ 20 keV. For the worst target-projectile combination, the scattering-divergence angle for 90%, a 20-keV beam, charge-converted at a target thickness of 2×10^{15} atoms/cm^2 will be 72 mrad full angle.

4. Negative charge-exchange ion source

When applying the charge-exchange technique to negative-ion sources for tandem accelerators, the combination of a high charge-exchange efficiency with a minimum in degradation of the negative beam emittance should be pursued. A tandem accelerator has a finite acceptance, which sets an upper limit to the emittance of the injected negative-ion beams; hence, when designing a negative charge-exchange ion source, it may be necessary to compromise between charge-exchange efficiency and beam scattering in order to obtain the highest intensity within the specified acceptance (highest brightness).

4.1. Optics

In the existing negative charge-exchange ion sources, two different methods for focusing the positive beam through the electron donor canal are used. By the first method, shown in Fig. 13a, the canal is positioned as close as possible to the extraction gap and sometimes placed in the extraction electrode. The beam is focused by the ion-optical system formed by the concave plasma boundary at the outlet of the positive ion source and the extraction field to a long waist (small beam angle). By the second method, a unipotential lens is placed between the positive ion source and the electron donor canal, which focuses the positive ion beam to a short waist (large beam angle), see Fig. 13b.

The degradation of the negative-beam emittance due to scattering during charge-exchange collisions depends on the relative increase in beam-divergence angle, where the angular-scattering is added to the divergence of the originally positive beam as the root mean square. The effect of the scattering appears most obvious by studying the increase of the emittance areas for the two different methods of beam focusing, and it is unambiguous that a system where the beam is focused to a short waist in the elctron donor canal will give the smallest degradation of the negative-beam emittance (see Fig. 13).

In Fig. 14, a system which is able to satisfy these demands on waist condition is sketched. By operating the extraction electrode and the electron donor canal from different power supplies, the exchange energy can be chosen independently of the extraction voltage. The matching of the ion source implies that the ion optical system formed by the plasma boundary at the outlet of the ion source and the extraction field are adjusted to give a proper filling of the lens. This is of great importance as it defines the length of the beam waist focused in the canal. Special care must be exercised when designing the beam optics following the electron donor canal to ensure that it can handle the large divergence angle of the entering negative beam.

4.2. Electron donor canal (Charge-exchange target)

In order to limit the amount of metal vapour streaming out into the vacuum system and to reduce the degradation of the beam emittance due to scattering, the exchange cell should be operated at lowest possible target thickness, and it is recommended to use the temperature of the cell to control the intensity of the negative ion beam.

From the charge exchange "growth"-curves in Fig. 2 it can be seen that reduction by a factor of 2 of the target thickness required for equilibrium will only reduce $F_{-1}(\mu)$ by ~ 10%, which speaks in favour of operating the charge exchange process below equilibrium. This puts heavy demands upon the control of the target thickness and therefore it is important that the charge exchange cell has low thermal time constants (low mass, good thermal contact between heating element and cell).

When using charge exchange cells of alkalis heavier than lithium, special care must be taken to avoid deposition of target material on the vacuum chamber and the lens system. Condensed target material coating on chamber walls and lens electrodes will, when exposed to air, immediately react with the moisture in the air and be converted into oxides and hydroxides, which are difficult to pump away, and it will be necessary to clean the parts each time the vacuum chamber has been opened up in air. Furthermore, concerning cesium, a heavy deposit of this metal could be dangerous as it can ignite and explode upon contact with air.

By placing condensers on each side of the charge-exchange cell shaped to match the convergence - angle of the beam, the amount of escaping target material can be reduced to a few percent of the total consumption of target material. The escaping metal vapour is formed to beams of neutrals drifting away from the cell in both directions and condensing on the first cold surface hit in the system.

The consumption of target material is determined by the dimensions of the canal, but is typically 1-2 g/day, which determines the intervals between cleaning of the condensers and recharging of target material.

In electron donor canals developed by Bacal and Reichell[22] and by Greenway[23], the condensed alkali metal is

recycled using a heat pipe transport technique, which makes it possible to use the cell for hundreds of hours without recharging. In Fig. 15, the electron donor canal developed at Oxford (Ref. 22) is shown. The ends of the wick-lined tubes are maintained at a suitable temperature for condensing the alkali metal, which is heat transported back to the reservoir by the stainless steel wick.

In the electron donor canals of jet-type the amount of target material excaping into the vacuum chamber is almost totally eliminated, hence this canal type will be very suited for use with Cs. D'yahckov et al.[24] have designed a Cs jet target with a closed circulation system for the cesium, which is capable of producing target thicknesses of up to 5×10^{15} atoms/cm^2 and with very small loss of target material (~0.001 g/h).

4.3. Charge-exchange energy

From the curves in Figs. 8-10, showing the charge-exchange efficiency as a function of energy for a number of projectile-target combinations, it can be seen that the maxima in charge-exchange efficiency mostly appear in the energy range 10-60 keV.

Exceptions are projectiles of Cs charge-exchanged in Cs, with $F_{-1\infty max}$ at about 100 keV and the light elements Li, B, and C, which show high negative fractions when charge-exchanged in Cs at energies of a few keV. From Eq. 3 it appears that the angular scattering becomes very large at these energies, which in combination with the difficuties to control a low energy positive beam of high intensity, makes it unlikely that charge exchange can be made at energies < 10 keV.

5. Test bench measurements

Table I shows some charge-exchange results obtained in Na, Mg, and K on the ion - source test bench at Aarhus. Two different positive ion sources from Danfysik A/S were used. Type 910 is a universal separator source developed by Almén and Nielsen[25], and type 911A is a modified version of the Sidenius hollow-cathode source[26]. The positive ions were extracted at 20 kV and focused by an einzel lens to a waist in the electron donor canal according to the method described in Sec. 4.1 and schematically shown in Fig. 14. However, the electron donor canal was not electrically insulated from the vacuum chamber, so the exchange energy was determined by the extraction voltage. The exchange cell used had a very low thermal time constant, which ensured a fast regulation of the temperature. The design of the exchange cell is described in detail in Ref. 13.

During the runs, the ion source and the lens were first adjusted to give maximum, analyzed, positive beam current; then the exchange cell was heated and its temperature adjusted to give maximum negative beam. The measured currents and the corresponding charge-conversion efficiencies are shown in the table. For comparison the equilibrium fractions $F_{-1\infty}$ at 20 keV are included where available. Also listed are the target thickness values μ_∞ and the maximum scattering angle α for 50% of the beam, estimated as described in Sec. 3. With the exception of Al and Fe, the charge-exchange efficiencies obtained on the test bench are in good agreement with $F_{-1\infty}$ (20 keV) as long as they are low, whereas an increasing disagreement is found at larger values. This is explained by the pole gap of the analyzing magnet limiting negative beams of large angular divergences, i.e., beams which are scattered strongly in the exchange target. The Al^+ and Fe^+ beams were produced by the CCL_4-method[27], and a low transmission due to

space-charge effects from the strong chlorine component in the extracted and charge-converted beam may explain the low conversion efficiencies observed for Al and Fe.

The test bench is equipped with an emittance-measuring device and the emittances of the anlyzed negative ion beams were measured in the vertical plane. With the emittance defined as the area enclosed by the 10% brightness contour in the two-dimensional emittance diagramme, the negative-ion-beam emittances were found to vary from 1.6-2.8 π mm mrad $MeV^{1/2}$ for the measured beams.

6. Conclusion

The systematic investigations of charge exchange in metal vapour targets are a valuable contribution to the development of negative-ion-source technology, and it has been demonstrated that standard, universal, positive ion sources, in combination with charge exchange in a metal vapour, are capable of producing low emittance negative ion beams in the μA range of a majority of the chemical elements. The charge-exchange method has shown to be particularly suited for the production of beams of elemental negative ions with low electron affinity, and for such ions generally absolute beam currents, in excess of those achieved with the best of the existing negative ions sources[28,29], are obtained.

The scattering effects in the charge-exchange process are in the relevant exchange-target thickness range only weakly dependent on projectile (or target) atomic number and inversely proportional to the charge-exchange energy. However, the dependence of target thickness μ is strong ($\propto \mu^{2.5}$), which can make it necessary to operate the charge-exchange process below equilibrium and compromise between charge-exchange efficiency and beam scattering to obtain the highest intensity within a specified acceptance. The degradation of the beam emittance due to scattering can

be minimized by a proper design of the optics of the charge-exchange ion source, i.e., the positive ion beam should be focused to a short waist at the position of the electron donor canal.

The energy spread in the charge-exchanged negative beams due to energy straggling has in Ref. 9 for moderate target thicknesses been estimated to be similar to or less than that of negative beams delivered from existing negative sputter or plasma sources, provided the energy spread of the positive ion source is low (~5 eV).

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DISCUSSION

Moak: On the alternative scheme where you use a short high density canal and focus the beam to a rather large angle, small diameter, you have the disadvantage that you must now use a focussing element with its associated spherical abberations and must follow this system with an additional focussing element because the beam divergence is not suitable for injection into an accelerator. So you have accepted the disadvantage of spherical abberations of two rather bad lenses at rather low energy. Do you think this would almost destroy the advantages of using a short high density canal?

Tykesson: I completely agree with you. It is very important, especially, that the following optical elements can take the largest divergence angle of the beam. But the worse case, we get an increase in divergence of about 100 mrad full angle. So this means, lets say, that the scattering angle is added as the root mean square. So if the scattering angle is 100 mrad – lets say we focus the beam to 200 mrad convergence angle, this will give up a beam diameter of 20 mm at the cell if we place the charge change cell 100 mm from the center of the lens. So it depends on how close you can place the lenses relative to the cell. If you have a lens diameter of perhaps 100 mm it should not give you serious abberations.

Moak: But without those two lenses you have no abberations and it seems to me that these lenses are always a serious problem. In fact, this is one of the advantages of having the canal directly behind the extractor so that you can avoid the use of two offensive lens elements which spoil the beam emittance by themselves without any spoilage from the cell itself.

Alton: I think this particular technique may be the way to go for Group I and possibly Group II elements. There seems to be adequate means for generating most everything else in the periodic chart with the exclusion of these. Would you comment about the intensities that you get from cesium? I'm talking about negative ion intensities rather than percentages. The intensity that you observed in your Faraday cup when you measured the negative ion yield from the cell.

Tykesson: We have not made any measurements on the test bench where we are looking for the maximum intensities. For cesium, we have only studied the charge exchange efficiency. This is the next step. So we can't say anything about this. This is very interesting for the alkali metals because they have very low charge exchange efficiencies, but on the other hand, it is very easy to produce intense positive ion beams from these elements in surface ionization sources that we use on our sputter sources.

Alton: One additional comment, I think this is perhaps the only way to go with the Group I elements with the exclusion, of course, of those from which we can prepare solid samples. I am speaking specifically of rubidium, potassium, and cesium. This method must be the only way for producing these elements so far.

Tykesson: Yes, I agree. I can't see any other way.

Middleton: At Strassburg you reported, I believe, that of the Group II elements, the only one you had no success with was magnesium. It would appear, I'm asking really, have you repeated measurements on magnesium, possibly with cesium vapor? Has indeed magnesium even got a metastable negative ion?

Tykesson: There should be a metastable negative ion for magnesium but we haven't observed it. We haven't measured magnesium charge exchange in cesium but we have measured using potassium and sodium and we found that the fraction must be smaller than 10^{-6}. This metastable state may be too short for us to measure.

Middleton: I'm very very curious to know if magnesium is similar to nitrogen, has a lifetime that is very, very short?

Tykesson: For all other alkali earth elements we have observed negative ions. For calcium, we have observed 1.5% for charge exchange in sodium. For strontium we have not measured the charge exchange efficiency but we have made rough tests on the test bench and we observed negative ions for strontium and it was surprisingly high, about 20%. For barium we also observed a low negative beam which indicated a charge exchange efficiency of 0.2% but on the test bench we are very low in charge exchange energy so for barium, since it is a heavy element, the efficiency could be higher at higher energies. And, of course, beryllium 3% in sodium and we also have produced 3.5 μA on the test bench and have also accelerated in the tandem.

Clegg: I would like to point out the technology of alkali metal vapor wick canals, since it was developed in Orsay in 1971, has been used in the Lamb shift polarized ion sources rather routinely. Primarily at Los Alamos, but also at Seattle, and some at the University of Michigan. The technique is such that you can run with 20 grams of cesium very, very easily for a month of continuous operation. You have to be careful, however, that this reservoir region not be exposed to air, as you indicated. If you want to turn off your ion source and not use it for several days and start it up again, you should have some way to

valve off the region of cesium if you have to go into the other part of the ion source for any reason because if the air contacts any of the cesium coated surfaces you have to essentially open up the entire system and do a maintenance. The wick becomes contaminated with air and then you have to rewet the wick before you can really get satisfactory performance again. But it is a very stable source, the alkali metal canal wicking system works very well and it has been used successfully now for a long time. There is a study of this wicking canal and actual measurements of cesium flowing from the canal in the latest University of Washington Progress Report made by Risler there. He actually made a little cesium detector and measured cesium atom fluxes from his wick canal. If anyone is really interested you could look at that.

Larson: I would like to respond to Charlie Moak's comment. The figures that you showed for changing the shape of the waist from a rather broad one to a narrow one by a short crossover were perhaps a little too extreme. If the scattering generated in the canal is at larger angles than originally existed in the beam, then I suspect that despite the fact, the lenses that you use there are probably going to abberate. You would be ahead to use a lens before the canal and raise the angles in the original beam, to angles comparable to those that are going to be generated by scattering. Surely, you must lose otherwise. If the scattering angles are going to be larger after the canal then you're forced to use the second lens no matter what. So you can't get away from that lens. But I think extreme angles, as were suggested by the figure, might not be appropriate and indeed, as Charlie says, you might suffer more from the abberation through the lens than you gain by reducing the contribution to the scattering into the phase-space. The design is not quite trivial and one must consider what the beam is that you're dealing with and all aspects of the components that are going to contribute to abberation, ultimately.

Tykesson: You always have to play with the target thickness and the charge exchange energy and the scattering factors when you try to optimize these things.

TABLE I

Charge exchange results obtained on the test bench for different negative ion beams. $F_{-1\infty}$ is the equilibrium fraction at 20 keV and μ_∞ is the equilibrium exchange target thickness α is the maximum scattering angle of 50% of the beam.

Ion	Electron affinity (eV)	Charge exchange material	Ion source	Positive Beam (μA)	Negative Beam (μA)	Conversion efficiency (%)	$F_{-1\infty}$ (%)	Emittance Negative Beam mm mrad MeV$^{\frac{1}{2}}$	μ_∞ 10^{15} atoms/cm^2	α(50%) mrad
$^{7}Li^-$	0.62	Na	911A	170	2.0	1.2	1.3		0.8	0.1
		Na	910	130	1.6	1.2	1.3	1.6 π	0.8	0.1
		K	911A	180	2.5	1.4	1.7		0.9	0.1
		K	910	70	1.0	1.4	1.7	1.9 π	0.9	0.1
$^{9}Be^-$	+0.24	Na	911A	125	3.4	2.7	2.8		3.2	4
$^{11}B^-$	0.28	Mg	911A	50	3.0	6.0	7.0		0.8	0.2
		Na	911A	30	2.8	9.3	11.5		1.2	0.4
$^{12}C^-$	1.3	Mg	910	24	3.2	13.3	14.4		1.5	1
		Na	910	28	4.5	16.1	22.0	2.2 π	1.5	1
$^{16}O^-$	1.46	Mg	910	16	3.0	18.7	40.0		7.5	60
		Na	910	14	2.3	16.4	33.0	2.8 π		
$^{23}Na^-$	0.54	Na	911A	110	0.4	0.36	0.4		0.5	0.1
$^{24}Mg^-$	+0.32	Na	910	120	$\leq 10^{-4}$	$\leq 10^{-6}$				
		K	910	150	$\leq 10^{-4}$	$\leq 10^{-6}$				
$^{27}Al^-$	0.46	Na	910	25	1.1	4.4	7.8	2.1 π	0.6	0.1
		K	910	10	0.6	6.0	13.5	2.5 π	1.4	0.9
$^{28}Si^-$	1.39	Na	911A	65	15.5	23.8				
		Na	910	13	3.2	25.0		2.1 π		
$^{35}Cl^-$	3.6	Mg	910	15	7.0	47.0	77.0		7.5	70
$^{40}Ca^-$	+unknown	Na	911A	100	0.5	0.5				
		K	911A	40	0.5	1.2				
$^{56}Fe^-$	0.25	Na	910	30	0.7	2.3	5.5	1.8 π	0.8	0.3
$^{197}Au^-$	2.3	Na	911A	29	10.0	34.5	68		3.0	8

+ metastable

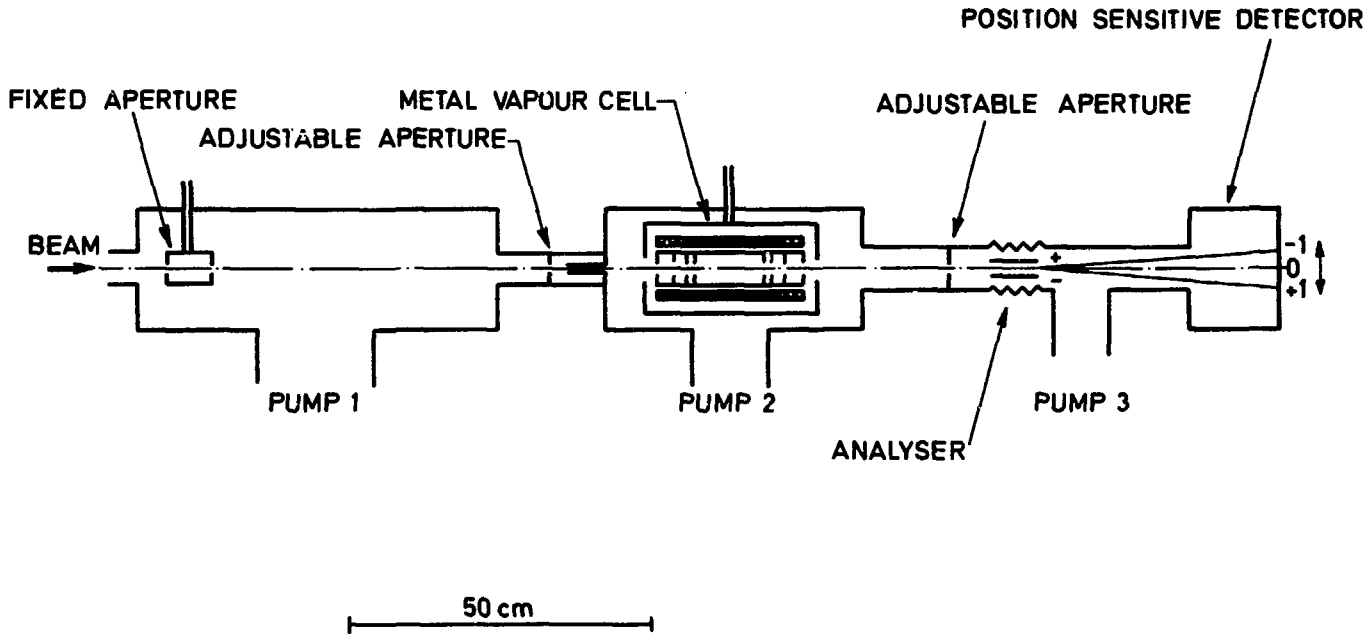


Fig. 1: Schematic drawing of the apparatus used for the charge-exchange investigations.

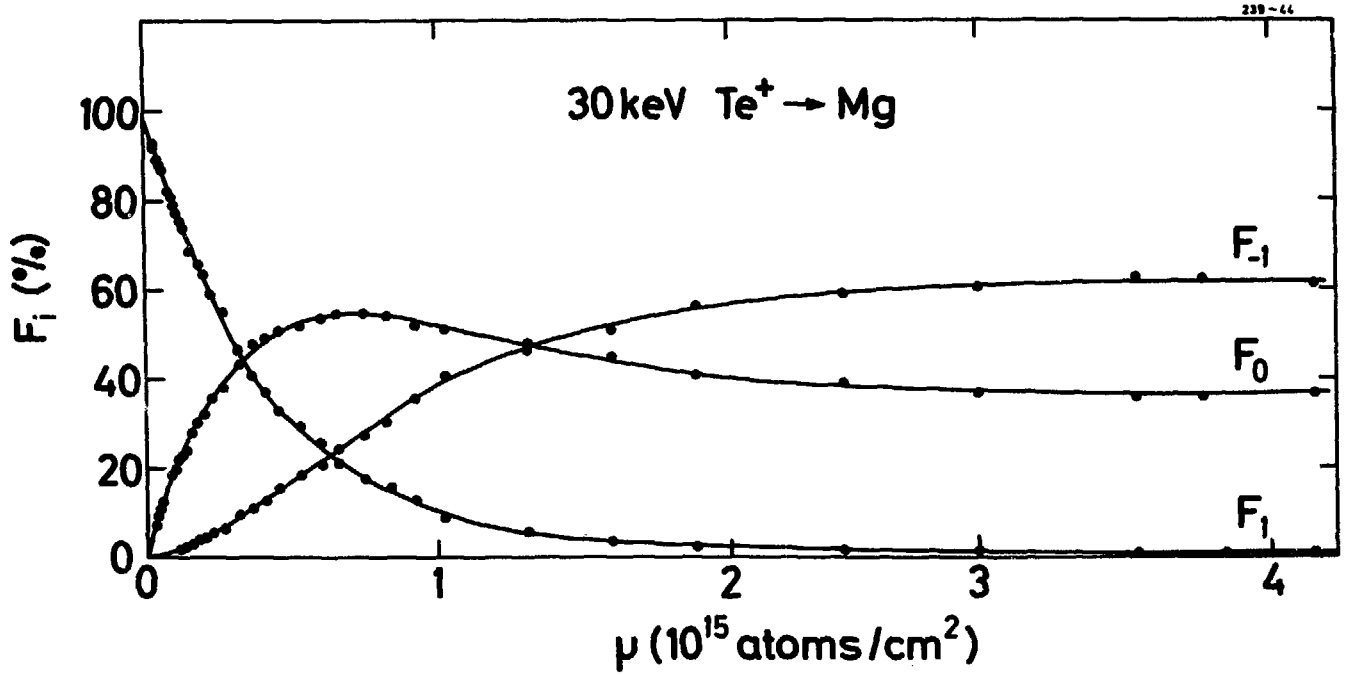


Fig. 2: Typical example of measured variation of charge-state fractions with target thickness:30 keV Te^{+}→Mg.

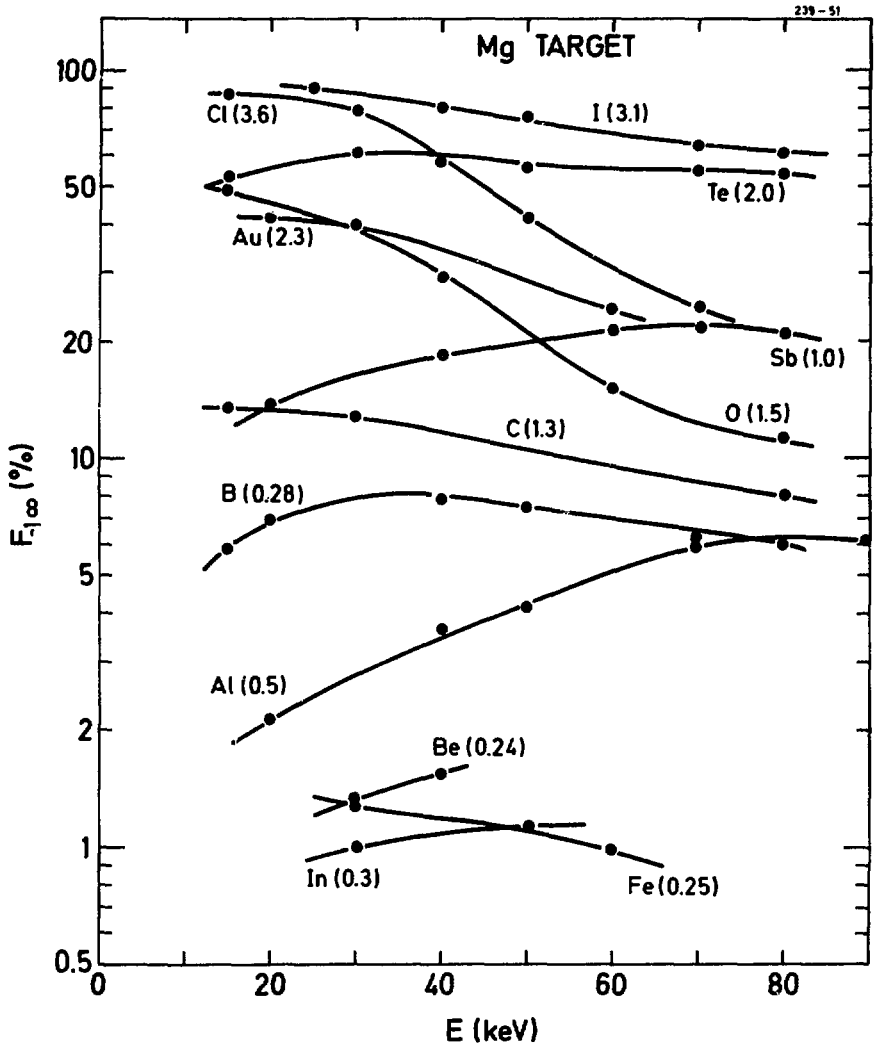


Fig. 3: Negative equilibrium fractions versus energy for charge-exchange in Mg, taken from Ref. 9. The projectile electron affinity (eV) is given in brackets.

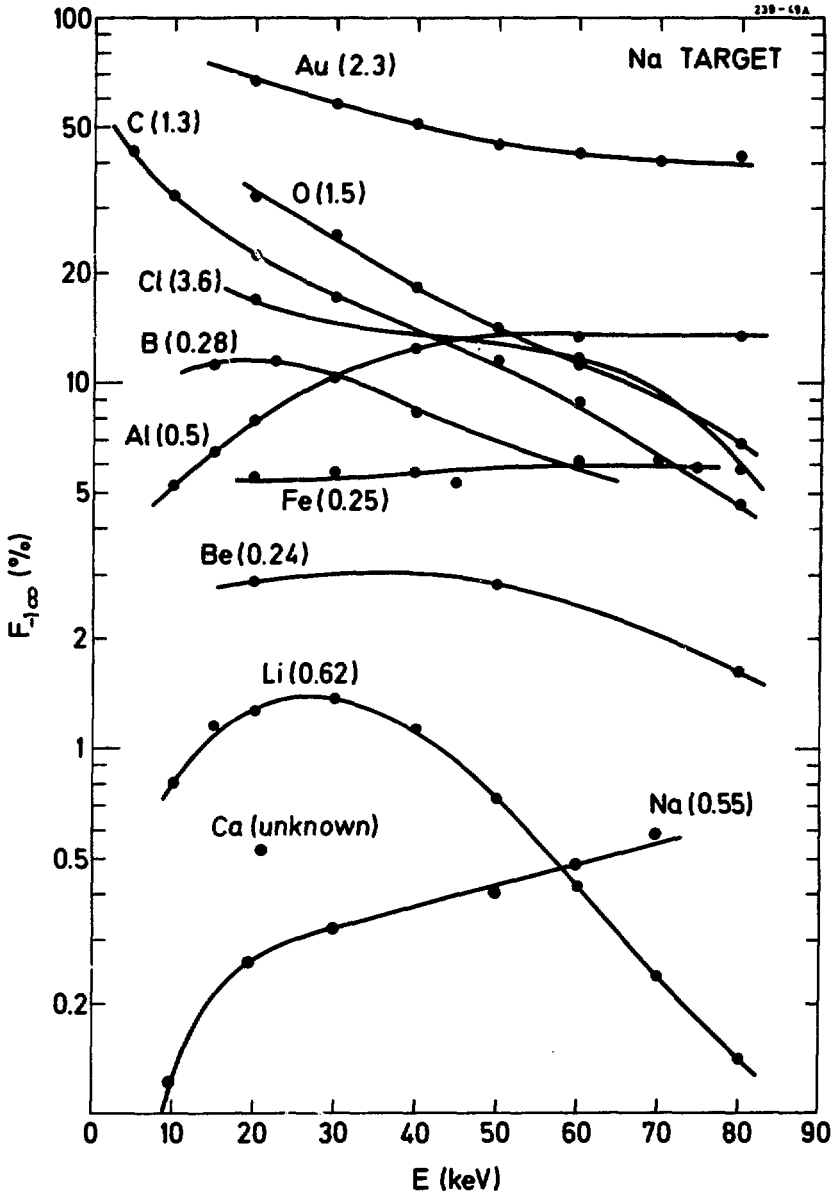


Fig. 4: Negative equilibrium fractions versus energy for charge-exchange in Na, taken from Ref. 10. The projectile electron affinity (eV) is given in brackets.

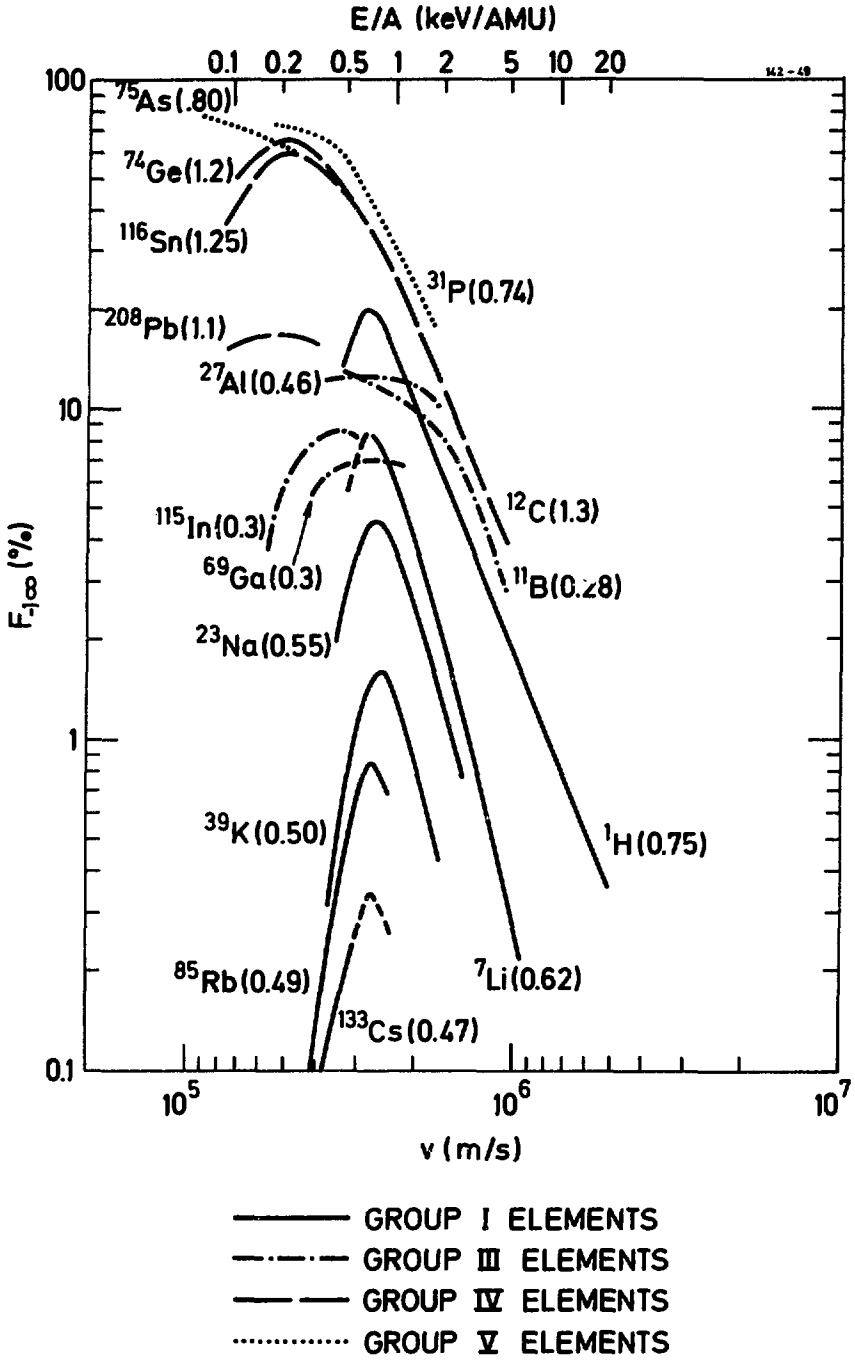


Fig. 5: Negative equilibrium fractions versus projectile velocity for charge-exchange in Cs. The projectile electron affinity (eV) is given in brackets.

1 H 13.595 0.7542							2 He 24.58 0.078 *
3 Li 5.39 0.620	4 Be 9.32 0.24 *	5 B 8.30 0.28	6 C 11.26 1.268	7 N 14.54 ≤ 0	8 O 13.61 1.462	9 F 17.42 3.399	10 Ne 21.56 < 0
11 Na 5.14 0.546	12 Mg 7.64 0.32 *	13 Al 5.98 0.46	14 Si 8.15 1.385	15 P 10.55 0.743	16 S 10.36 2.0772	17 Cl 13.01 3.615	18 Ar 15.76 < 0
19 K 4.34 0.5012	20 Ca 6.11 ?	31 Ga 6.00 0.3	32 Ge 7.88 1.2	33 As 9.81 0.80	34 Se 9.75 2.0206	35 Br 11.84 3.364	36 Kr 14.00 < 0
37 Rb 4.18 0.4860	38 Sr 5.69 ?	49 In 5.78 0.3	50 Sn 7.34 1.25	51 Sb 8.64 1.05	52 Te 9.01 1.9708	53 I 10.45 3.061	54 Xe 12.13 < 0
55 Cs 3.89 0.4715	56 Ba 5.21 ?	81 Tl 6.11 0.3	82 Pb 7.41 1.1	83 Bi 7.29 1.1	84 Po 8.43 1.9	85 At 9.5 2.8	86 Rn 10.74 < 0

* METASTABLE

UPPER FIGURE : IONIZATION POTENTIAL IN eV

LOWER FIGURE : ELECTRON AFFINITY IN eV

Fig. 6: Periodic table of the elements with ionization potential and electron affinity in eV.

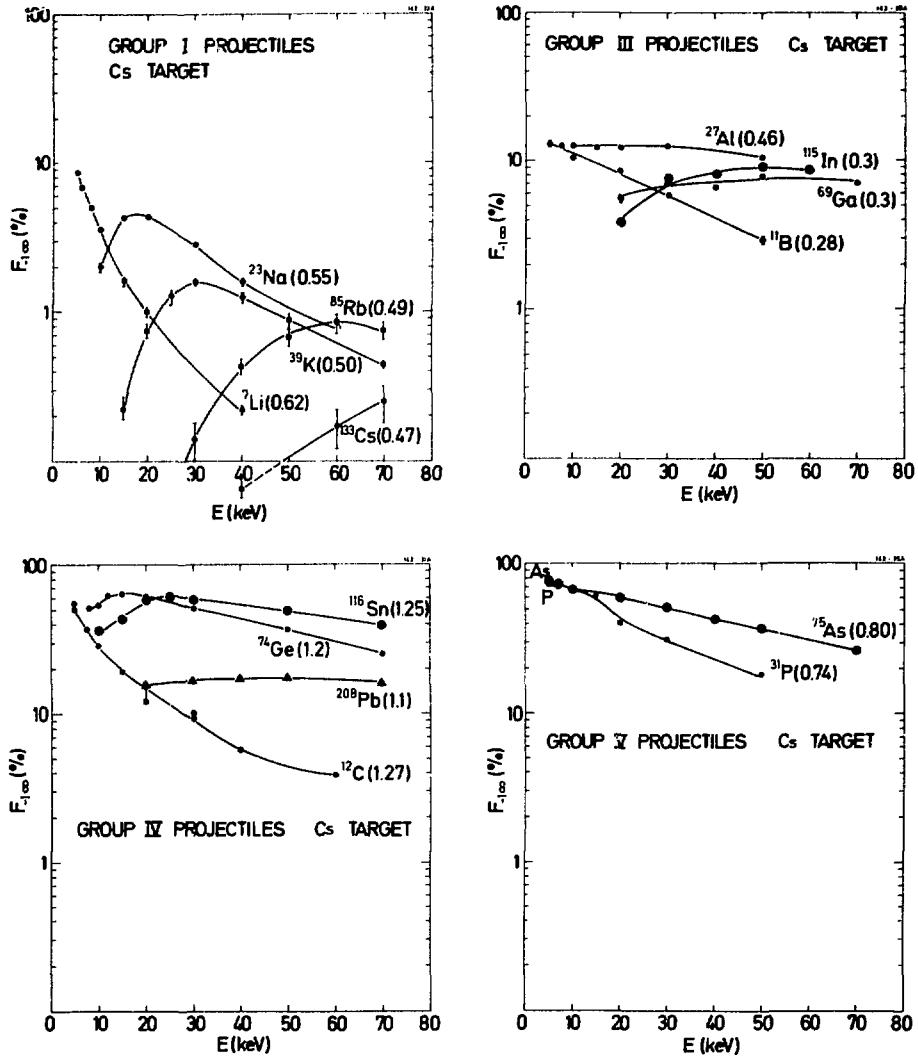


Fig. 7: Negative equilibrium fractions versus energy for charge-exchange in Cs. The projectile electron affinity (eV) is given in brackets.

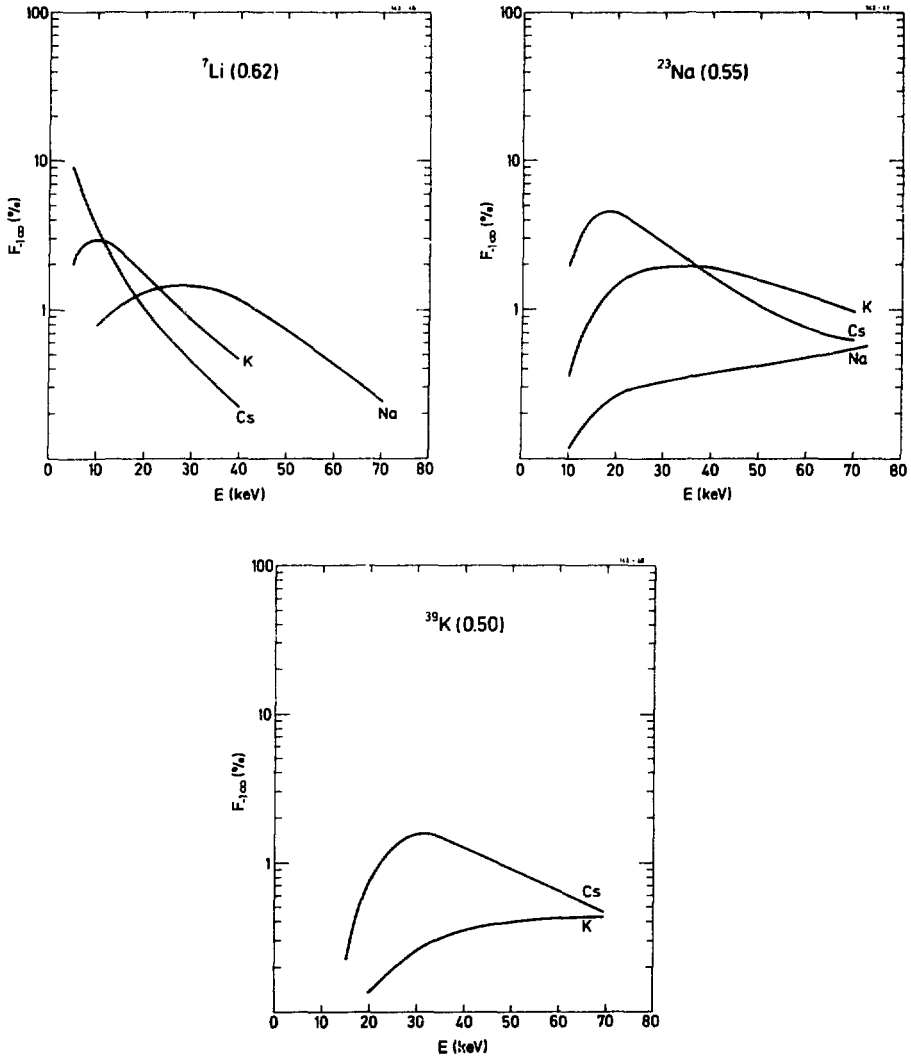


Fig. 8: Negative equilibrium fractions versus energy for Li, Na and K charge-exchanged in different metal vapour targets.

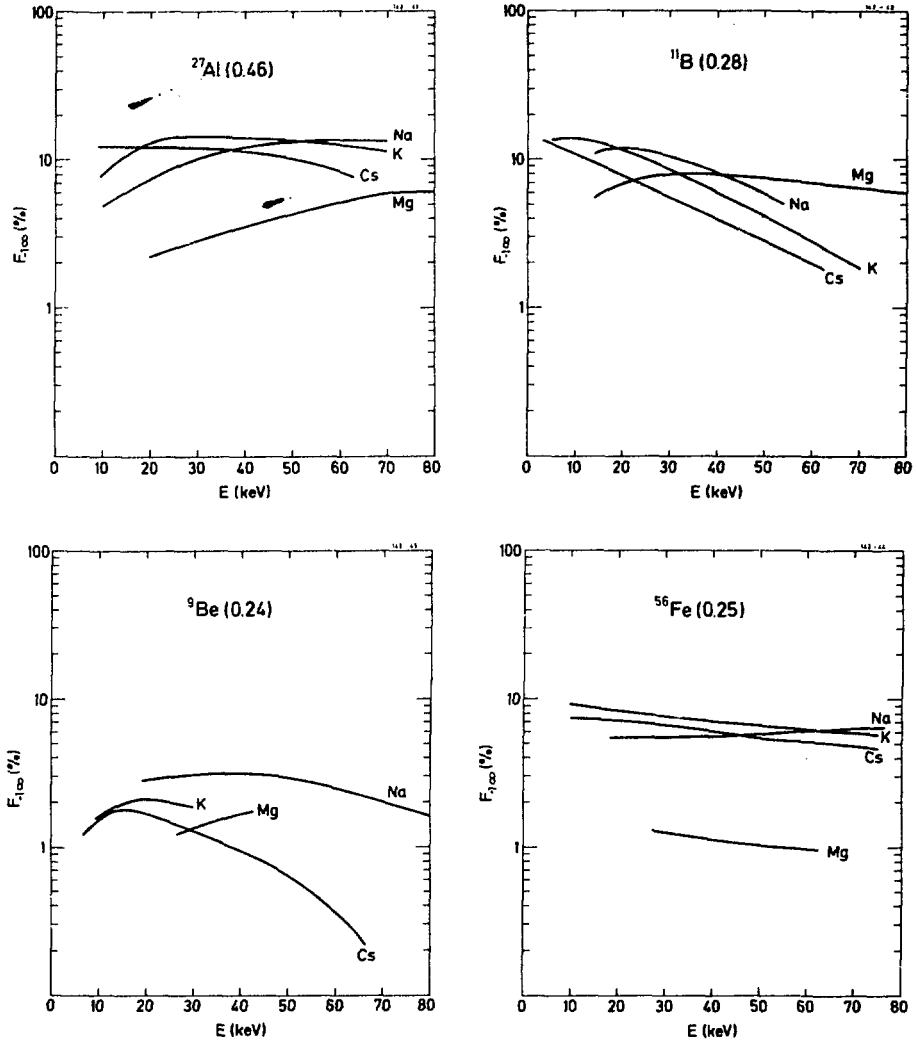


Fig. 9: Negative equilibrium fractions versus energy for B, Al, Be and Fe charge-exchanged in different metal vapour targets.

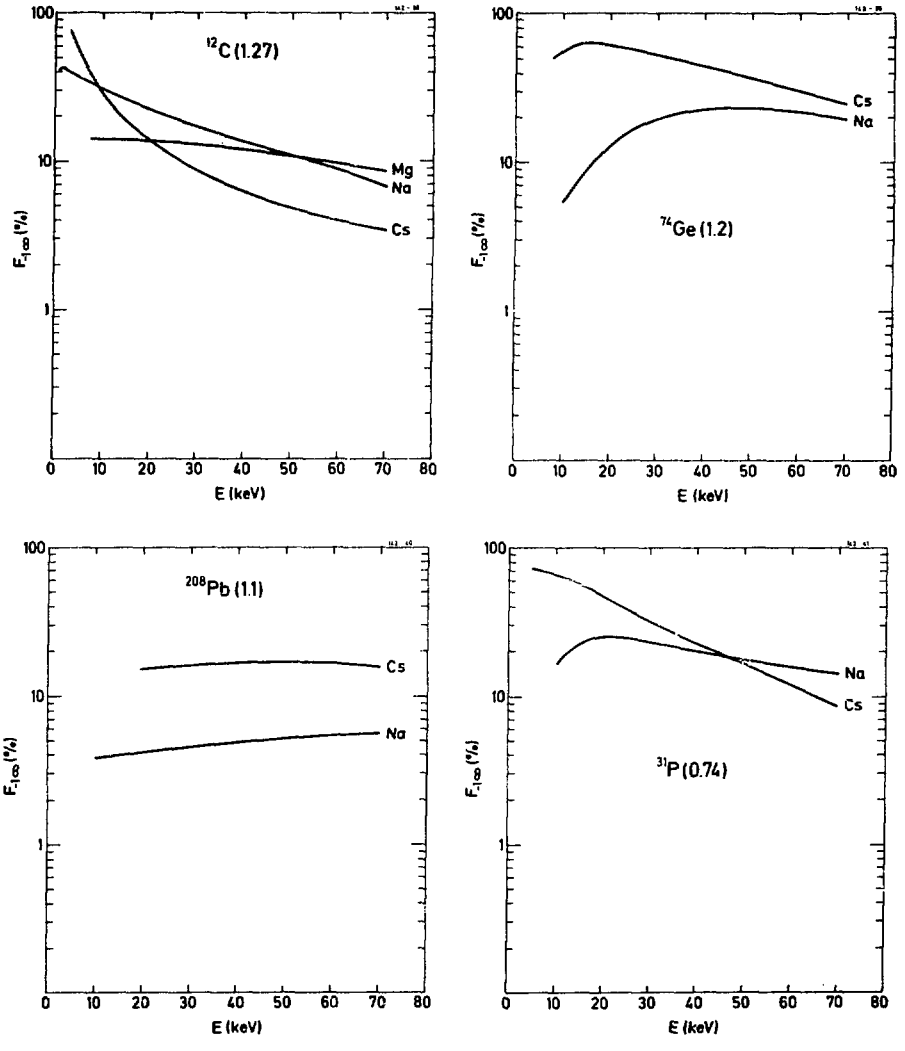


Fig. 10: Negative equilibrium fractions versus energy for C, Ge, Pb and P charge-exchanged in different metal vapours targets.

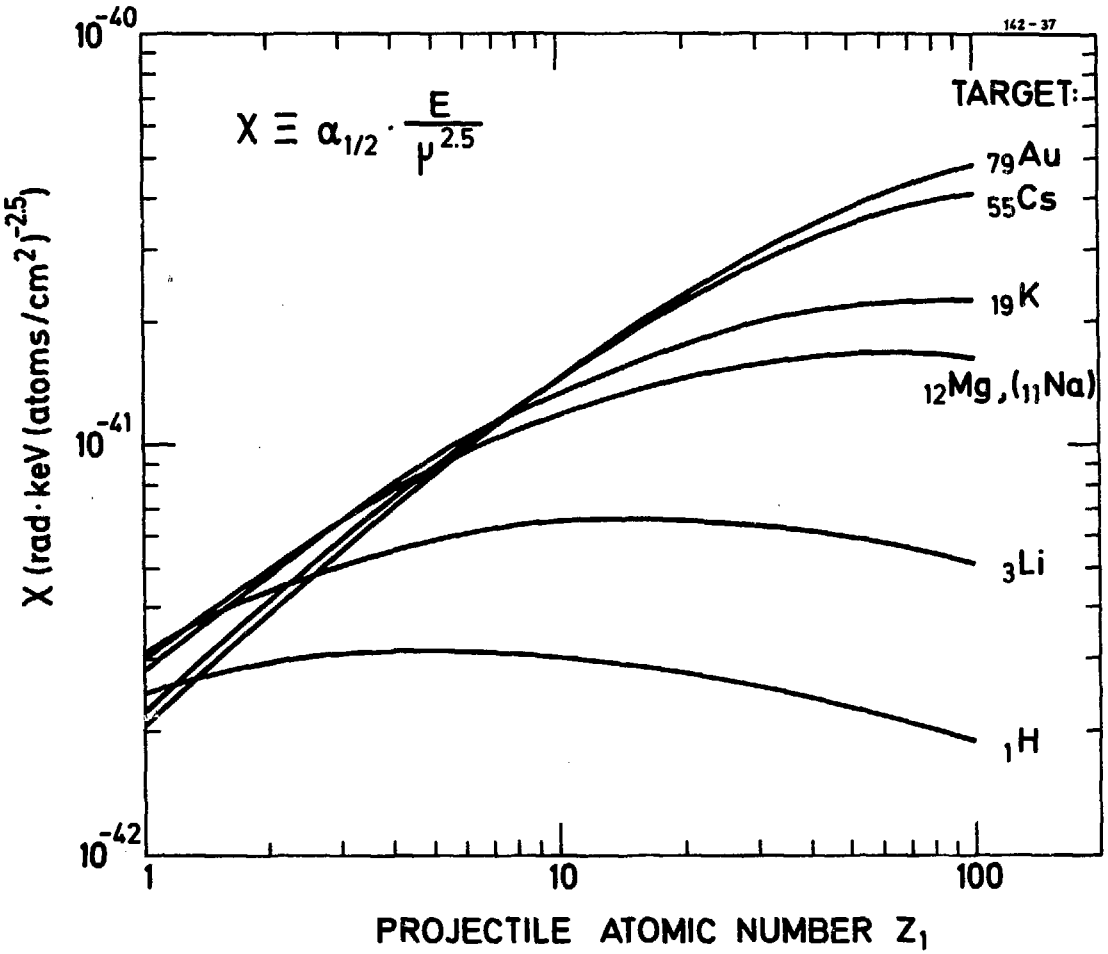


Fig. 11: The factor χ (Eq.3) calculated for different targets as a function of projectile atomic number.

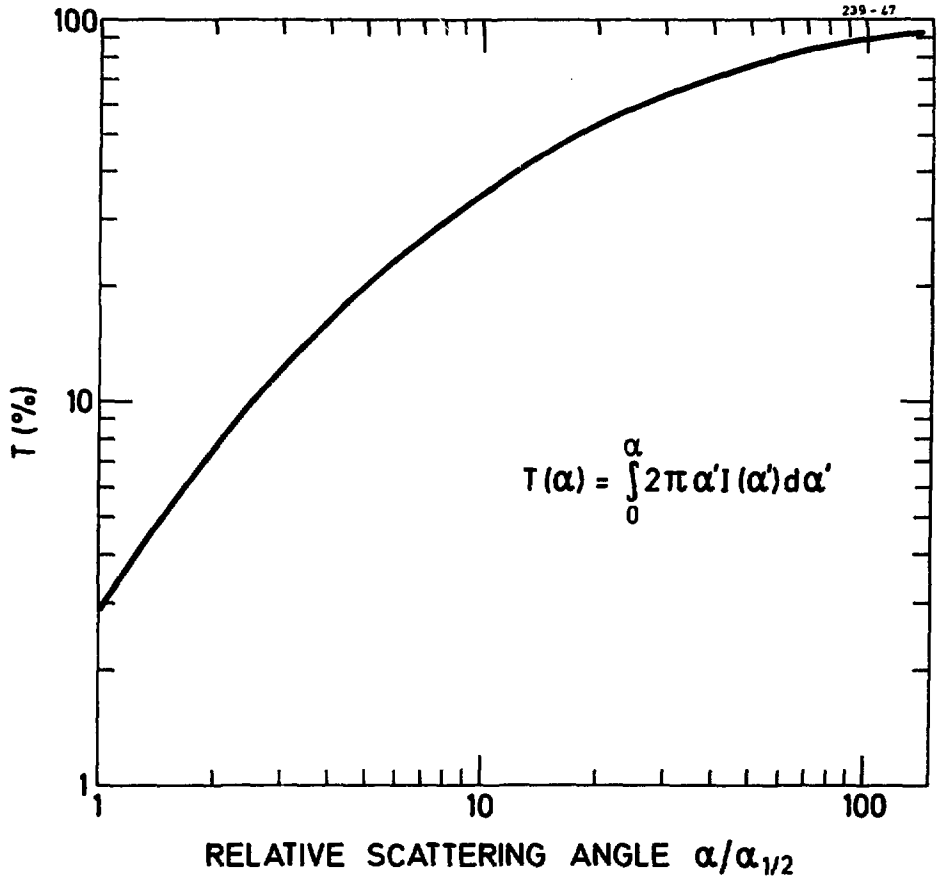


Fig. 12: The fraction $T(\alpha)$ of the scattered beam contained inside a cone of half angle α with respect to the beam axis.

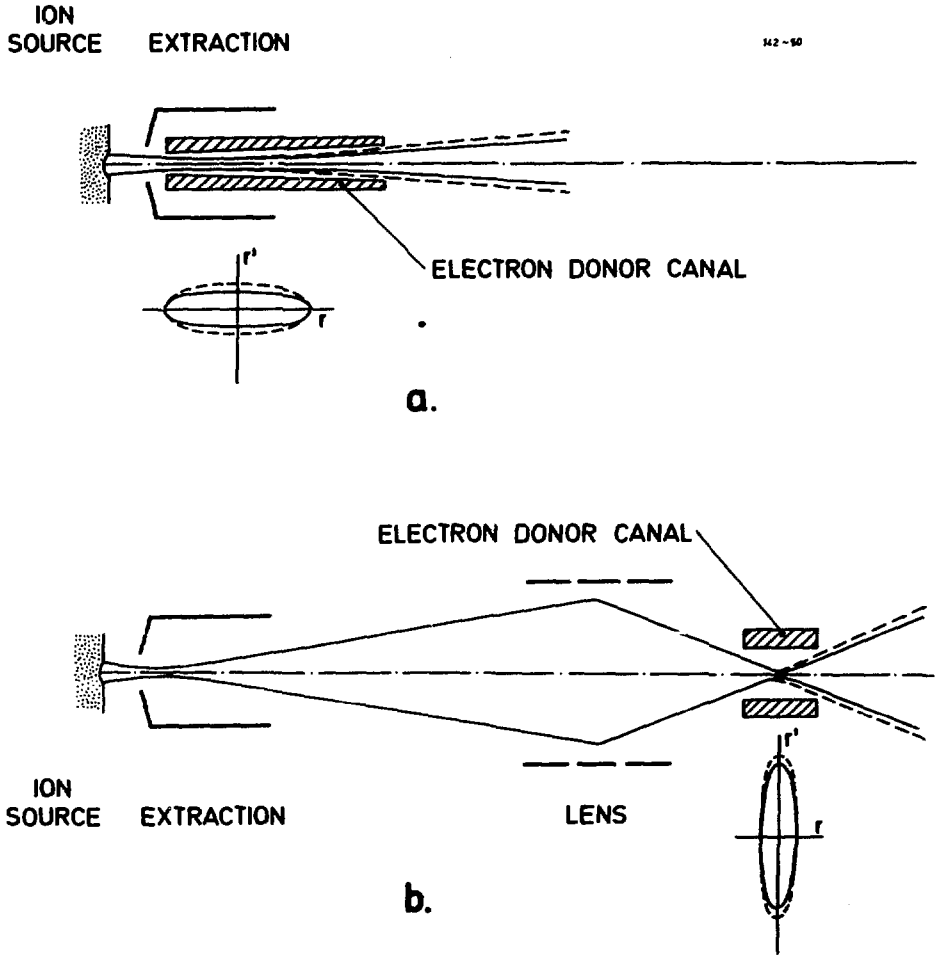


Fig. 13: Different methods for focusing the positive beam through the electron donor canal.

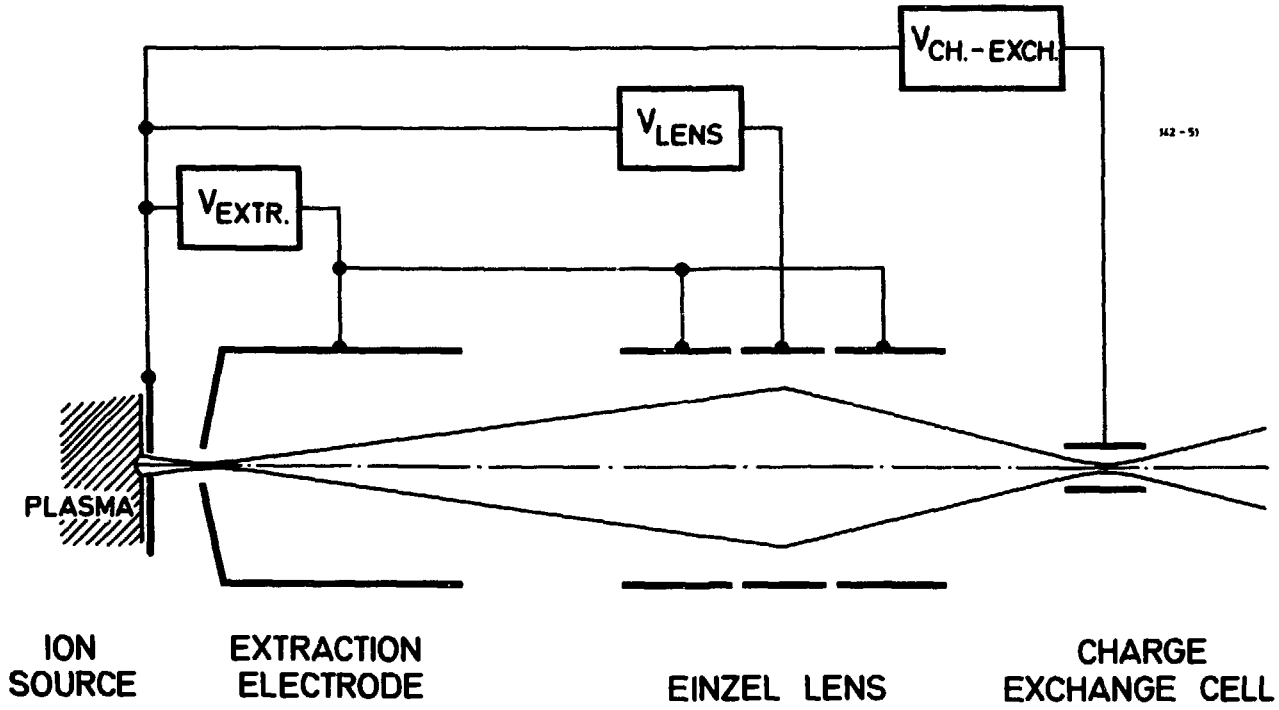
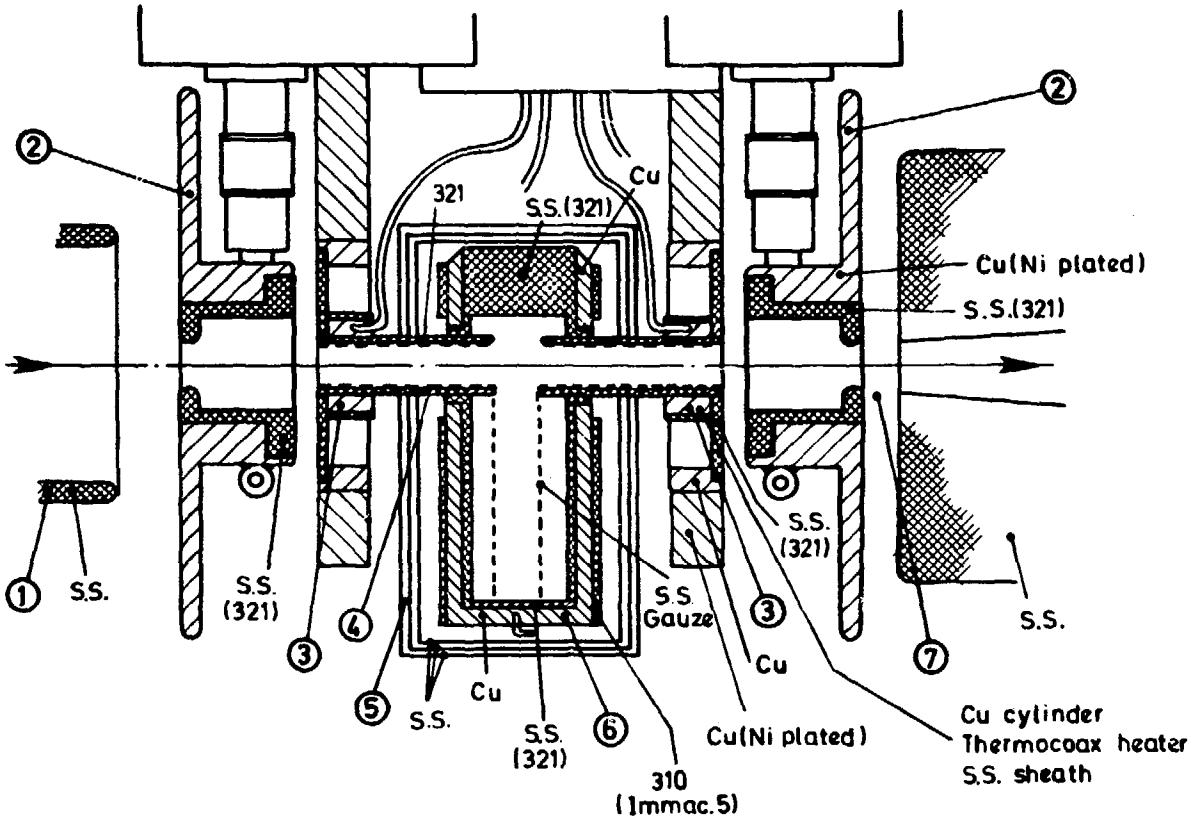


Fig. 14: Sketched charge-exchange ion source.



1 end of einzel-lens; 2 cooled apertures; 3 condensers with heaters and thermocouples; 4 wick lined tube; 5 triple layer heat shield; 6 alkali metal reservoir with heater and thermocouple; 7 accelerating gap.

Fig. 15: The Oxford recycling electron donor canal.

Session VII, Chairman, P. Thieberger
Editor: C. M. Jones

OVERVIEW OF THE VICKSI-FACILITY

VICKSI-Group, presented by B. Spellmeyer
Hahn-Meitner-Institut fur Kernforschung Berlin GmbH
Bereich Kern- und Strahlenphysik
Berlin, Germany

Introduction

The accelerator combination VICKSI (Van de Graaff Isochron Cyclotron Kombination für Schwere Ionen) is nearly completed after five years of design, construction and assembly. It consists of a converted 6 MV Van de Graaff as an injector and a four sector split pole cyclotron. A general layout of facility is shown in fig. 1.

Positive ions of masses $1 \leq A \leq 86$ and a charge state q_i (typically $1^+ - 4^+$) are produced in an axial penning ion source in the terminal of the Van de Graaff. After acceleration in the Van de Graaff the ions are stripped to higher charge states q_s and injected into the cyclotron. In the cyclotron the ions are accelerated to about 17 times the injection energy. Thus the final energy of the ions is given by $E = q_i \times 6 \times 17\ MeV \approx q_i \times 100\ MeV$. The energy limit given by the design of the cyclotron magnets is $K \approx 128\ MeV \cdot q^2/A$. Beam intensities of about 100 pnA at the exit of the cyclotron with energies of $E \approx 200$ MeV are reached using a 2^+ beam from the ion source. For higher charge states out of the ion source the intensity drops by about factor of 10 for every charge state increase. The adaption of the dc-beam from the ion source to the pulse requirements of the cyclotron is provided by three bunchers. The first buncher is located in the terminal of the Van de Graaff, the second after the stabilizing slits and the third in front of the cyclotron. By the three bunchers about 60 % of the dc-beam are compressed into a 6° phase interval of the cyclotron radio frequency. The stripper located half way in the injection beam line is combined gas and foil stripper. In principle the losses due to bunching and stripping are the only one

that have to be anticipated and about 15 % of the dc out put of the ion source can be accelerated in the cyclotron and extracted with an energy resolution of $\Delta E/E = 10^{-3}$.

Three target areas, the one for hyperfine interactions, for time of flight spectrometry and the one in front of the Q3D spectrograph are completed and have received beam for first experiments.

In the following the converted Van de Graaff, the cyclotron and the operating experience with both machines are described. The computer control system as well as the beam handling system are presented in other contributions to this symposium.

Van de Graaff Injector

Ion source

As an ion source a penning ion source with axial extraction is used. The maximum power consumption is 150 W, the gas flow in the source about 0.5 - 3 cm^3/h. Due to some improvements and optimalization of ion source parameters the lifetime of the source is well above 200 h. Table 1 gives the beams and the intensities for different charge states accelerated so far. Fig. 2 shows the whole terminal.

Beam handling

Fig. 3 shows the beam guiding elements of the injector. Three einzel lenses in the terminal focus the beam into the Wienfilter, the buncher and the tube, respectively. In addition two preacceleration gaps in the terminal provide optimum adaption of the beam for the buncher and the tube, respectively. Parameters of these optical elements are given in table 2.

The beam line below the injector is assembled with the standard VICKSI beam diagnostic elements. A quadrupole triplett near the base plate focuses the beam to the object slits of a double focussing analyzing magnet. Stabilizing slits are positioned at the focus at the exit of the analyzing magnet.

Vacuum system

The differential pumping system in the terminal consists of a turbo molecular pump with a fore pump and three ion getter pumps. The turbo pump pumps directly after the ion source ond is a standard Leybold pump with a reinforced chamber (450 l/sec). The fore pump is a stondard 4.5 m^3/h Alcatel pump with a special pressure tight chamber. The outlet of the pump is stored in a pressure tight reservoir bottle ($\sim$3 l) which is also connected to the motor chamber of the fore pump. A 400 Hz motor has been built in order to obtain the 1500 rpm for the pump. If the turbo pump is switched off a battery powered valve is closed between the turbo and ion getter pumped vacuum region of the terminal beam line. The turbo pump is not vented with N_2 to prevent backstreaming of oil vapor from the fore pump region (the fore pump is separated by a return valve). An analysis of the residual gas showed that the partial pressure of hydro carbons at the high vacuum side of the turbo pump is less than 1 % and after having run the system for several hundred hours no oil was found above the turbo pump. The ion getter pumps in the terminal are housed in especially built vacuum chambers. They provides pumping speeds of 90 and 180 l/s. Inside the ion getter pumps # 1 and # 2 einzel lenses are installed. The third pump is directly connected to the tube. Flow speeds between the different regions of the terminal vacuum system are between 0.1 and 1 l/s. The vacuum measured at the entrance of the tube is $\leq 1 \times 10^{-8}$ Torr.

The differential pumping system below the tube consists of six ion getter pumps. Two of them are installed inside the tank of the injector (fig. 4).

Generators and power supplies

The power for the components of the terminal has been increased from 4 —→ 7 kW by inserting a second generator at a higher terminal potential deck. The generator is driven by an isolated V-belt from the upper pulley generator.

In the terminal unregulated power supplies are used. The experience with the injector shows that only during the first 2 - 3 hours very little retuning of some settings has to be done because of thermal drifts. To prevent damage to the power supplies spark protection circuits are installed (fig. 5) and the shielded cables are run inside metal hoses. A good electrical contact between the spinning shell and the spinning is provided by about 150 springs. The terminal base plate is copper-plated and all holes in the plate are covered with metal grids.

Cooling of the terminal power supplies and the mechanical pumps is provided by cooled tank gas which is guided to the terminal by a plexiglas cooling tube from the base plate of the injector. In the terminal two freon cooling systems are installed (for the turbo and the fore pump). A schematic view of the cooling system is given in fig. 6. In addition a number of blowers have been installed in the terminal for good ventilation of the isolating gas in the closed spinning. Temperatures measured near the different power supplies are about 30 - 40° C.

Tube

The tube has been made from 20 sections of standard NEC-tube modules. First experiments with a potential distributions by an open corona system (1 needle and 3 needle failed). We replaced the corona system by a resistor chain of 3 x 200 M MOX-resistors in a shielding cave (fig. 7). The experience with this voltage distribution is very encouraging. No resistor failed since the initial

installation. Electrically and mechanically the tube is connected every five sections to the column. Fig. 8 shows the lowering of the tube into the column.

Voltage stability of the injector is given by the voltage stability of the tube. After few days of conditioning stable operation is reached at voltages up to 5.8 MV. During conditioning the maximum voltage was 6.4 MV. A good vacuum seems to be necessary for good voltage stability. Therefore the whole vacuum system connected with the tube is heated every time it is possible. Vacuum in the tube measured near the ion getter pumps at the baseplate of the injector is $\sim 4 \times 10^{-9}$ Torr.

Column

The CN-column has been elongated by 2 additional rings to meet the length of the NEC-tube. A cross-section of the column at the mid plate is given in fig. 9. Power cables can be drawn to the terminal to provide power for the terminal pumps in case the drive motor is shut off. For monitoring the belt position in the terminal up and down light pipes are used as light barriers. They are monitored outside the tank. The gradiented cooling tube is made from plexiglas and Al-rings and is connected to the column by springs. The resistors for the column voltage distribution are the same as used in the CN before (upgraded version).

Electrostatic changes

The diameter of the spinning has been increased by 20 cm, the length by 1.5 m. The shape of the spinning is the result of extensive field calculations. Fig. 10 shows that with the larger diameter a reduction of the field strength at the change over of the hoops to the spinning is reduced by about 20 %. High voltage test

without the tube showed that this region as well as the spinnings head is the most critical area for tank sparks. According to the enlarged spinning the length of the tank was increased by inserting a ring of 1.5 m height between tank and base plate.

At the tank a radiation source (Ir, ~4 Cu), the generating voltmeter, a NaJ(Tl) detector system and the corona system are installed.

Gashandling

For a fast service of components inside the pressure vessel a fast gashandling system has been installed. Fig. 11 shows the layout of the system. Turn around time with this system is about 3 h. As all components of the injector also the gashandling system is connected to the VICKSI control system which requires quite a lot of hardware interlocks between the different components of the gas-handling system.

Cyclotron

Fig. 12 shows the cyclotron in the new built cyclotron hall. It was completely assembled at the beginning of October 77 and after some difficulties with the last extraction element we succeeded in extracting a first beam from the cyclotron on 4th December 1977. The acceptance tests were completed in the middle of March 78.

In fig. 13 a cross section of the cyclotron with the main elements is shown. The diameter of the vacuum chamber is ~4 m. The maximum field between the pole tips is 1.57 T (gap ~6 cm). Each pole tip has 12 trim coils for the proper setting of the field. The resonator has a length of ~3 m, a diameter of 1.5 m. The

frequency range is 10 - 20 MHz. The frequency is set by a moving short, fine tuning is done by capacitive plates. The maximum Dee voltage is 100 kV. For injection three elements (2 magnetic - 1 electrostatic) are used. Extraction of the beam is done by an electrostatic deflector, the electromagnetic septum and an extraction magnet. Fig. 14 shows turn potterns of the beam inside the cyclotron taken by the movable differential probes in the injection and extraction valley of the cyclotron. For additional beam diagnostic a number of probes (positioned in front of the injection and extraction elements) and a set of phase probes are installed.

Calculation and setting of the cyclotron as far as the magnets are concerned is done by computer programms which is based on extensive field measurements. Furthermore a number of programms exist which help the operator to center and to isochronize the beam in the cyclotron. A comparison of the design aims and measured values are given in table 3.

Operating experience with VICKSI

At the beginning of this year few preliminary experiments have been started even before the cyclotron was accepted. Between March and July we have scheduled 1160 h of beam time. We were able to deliver 727 h of beam to experiments (60%). Down times in this period were due to the injector (44 %) beam handling and buncher system (11 %), cyclotron (43 %) and computer system (2 %). Down time at the injector is so high because any repair which requires an opening of the tank has the time for opening and closing the tank added on and furthermore during this period a necessary service at the compressor of the gashandling system has to be done while the tank was opened. The down times of the cyclotron were mainly due to problems with the main power supply, which we think we have solved now.

Fig. 15 shows a summary of beams accelerated so far. The final energy is plotted versus nucleon number A. The vertical lines indicate ions which have been tested and can be produced. The dots are beams which have been accelerated. On the right hand side the limit due to the charge state of the ions out of the source is shown. The limit of the cyclotron magnets for charge to mass ratios 1/2 to 1/6 are also indicated. The particle transmission from the ion source to the faraday cup behind the cyclotron is routinely between 5 % and 10 %. For an ^{16}O-beam a transmission of 12.5 % was reached. For the heavier ions (Ar, Kr) the transmission is only about 5 %. We attribute this to the necessary use of a foil stripper which gives a larger energy straggling than the gas stripper leading to a higher losses at the extraction of the beam out of the cyclotron. However with the second buncher operating and the proper time focus in the center of the cyclotron this transmission data for the heavier ions should be improved.

To end this overview I would like to point out that the idea for a VICKSI-like accelerator combination is not the intuition of todays physicists. A picture of Wassily Kandinsky from 1930 (fig. 16) clearly shows a single ended machine, a beam line system, high frequency and an old fashioned data transfer system (flags).

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Discussion:

Purser: Could you tell us something about the differential pumping that you said you had in the vertical machine?

Spellmeyer: That is a good point. Just under the ion source we have built in a 400 liter/sec pumping speed Leybold turbomolecular pump with an Alcatel 4 ft^3/hr forepump which pumps into a reservoir bottle.

Wegner: Could you tell us something about the cost?

Spellmeyer: The costs are really high but I think we have paid about 40 million marks for the system. Is that right, Heniz?

Clegg: I'm interested a little bit more in the cooling, I saw a lucite pipe going up to the terminal, is that right? What is the cooling fluid and cooling mechanism? How much power can you cool? I'm also interested a little bit more, if you have time, in your experience with the control system. You said infrared diode. What sort of protection circuits are needed to keep from blowing things out? Have you had any problems with that?

Spellmeyer: I will postpone the infrared light links to the other talk in the afternoon session. The cooling circuit; what we just do is bring cold water through a heat exchanger at the base plate of the Van der Graaff, and take tank gas, bring it through a rotary pump, through a filter after the lucite cooling tube where two other heat exchangers are placed at the terminal base which are air-freon heat exchangers and we then go with this cooled freon to the turbo pump and to the forepump. We have installed the old cooling from the CN and the cooling seems to be sufficient, especially if you have some air flow inside the terminal which was a problem, and we bring some ventilators in to it and now it seems to be good. We have made no special calculations, we just have to remove 7 kilowatts.

Clegg: So I understand your cooling medium is the actual cooling tank gas which goes up this pipe, is that right?

Spellmeyer: The cooling medium is the tank gas specially cooled in a heat exchanger before entering the cooling tube.

Wegner: Could you characterize the nature of the apparent voltage limitation problem on the NEC tubes and say a word or two as to whether or not you've experienced any problems similar to Munich's and also could you contrast the performance of the NEC tubes with your previous HVEC tubes in the machine?

Spellmeyer: Well, I think its unfair to compare the old tube with the new tube because we have changed quite a lot of things in the machine. For example, the larger spinning means bigger energy in the machine. So I don't like to compare both. What we had in former times was a good running CN, even at 7.3 MV, and we heard from the Freiburg people who have our old tubes that they are running the Van der Graaff at 8.5 MV and above. The problems with our tubes so far; we have not observed the same thing as the Munich people have with their colored surfaces and so. We have opened our tubes about a quarter of a year ago and looked in and haven't seen anything. We disassembled it enough just to look at it, we haven't observed such things. Now our situation in the last year was this: from the injector we had to deliver a beam to the cyclotron, whether it was high energy or low energy, it doesn't matter, they want to have a beam for the acceptance tests and so we didn't go really much into the details. We have had one longer conditioning period, the last 10 days, just before the conference, and we heated the whole system very extensively before beginning the conditioning process and it seems to me that this heating improved the conditioning process very much. With bad heating in our system, the machine starts tube conditioning at 4.5 MV or something like that. With better heating now the machine starts to condition now at about 5.0 MV and we bring it up within 4 or 5 days to 5.7, 5.8 MV. What we really have is some trouble about 5.7 mV. So I think we have to attack this problem but there has been no time so far.

Ion	Charge state 1^+	2^+	3^+	4^+	5^+
p	2				
^3He	6	.6			
^4He	1	.5			
^{12}C	10	.1			
^{14}N	1	.8			
^{16}O	10	1			
^{20}Ne	10	10	1.5		
^{40}Ar	10	10	3	.5	.05
$^{84}Kr^x$	5	5	1	.2	.05

^{x}from natural isotop gas mixture in the ion source

Table 1: Ion beams intensities available from the injector (Nov. 78)

Einzel lenses	inner diameter D (mm)	inner length li (mm)	max. voltage (kV)
EL 1	64	50	30
EL 2	60	70	30
EL 3	60	80	60

Wienfilter	
length (mid entrance to mid exit gap)	136 mm
electrical field gap	25 mm
electric field (variable)	4 kV/cm
magnetic field gap	35 mm
magnetic field	2.1 KG
Buncher	
tube diameter	3 mm
amplitude	0.3 - 1.5 kV
Acceleration gaps	
Extraction	0 - 30 kV
Preacceleration for bunching	0 - 20 kV
Preacceleration for tube	0 - 60 kV
Steerer	
STX B1	-500 - +500 V
STY B1	-500 - +500 V
STX B2	-500 - +500 V

Table 2: Parameters of the optical elements in the terminal.

	Design Aim	Measured Value
Energyfactor $K = \frac{E \cdot A}{q^2}$	120	127 for heavy Ions from field 144 for light Ions measurement 128 $^{20}Ne^{5+}$ accelerated to 160 MeV
Energyspread $\frac{\Delta E}{E}$	$5 \cdot 10^{-3}$	$2 \cdot 10^{-3}$ full intensity $0.5 \cdot 10^{-3}$ at 50 % intensity through analyzing slit
Transmission through cyclotron	100 % (30 % guarantee)	35 % for all extracted beams
Emittance	10 π mm mrad	2 π mm mrad
Time structure	1 % intensity outside $\pm$ 30° RF phase around beam pulse	In a scattered beam no particles between pulses with 10^4 particles in pulse
Pulse width	6°	9° (1.5 ns at 16 MHz)
RF-Amplitude stability		10^{-3}, long term 2×10^{-4}
RF-Phase stability		0.02° (12 h)

Table 3: Comparison of design specifications and acutually measured values for several machine parameters.

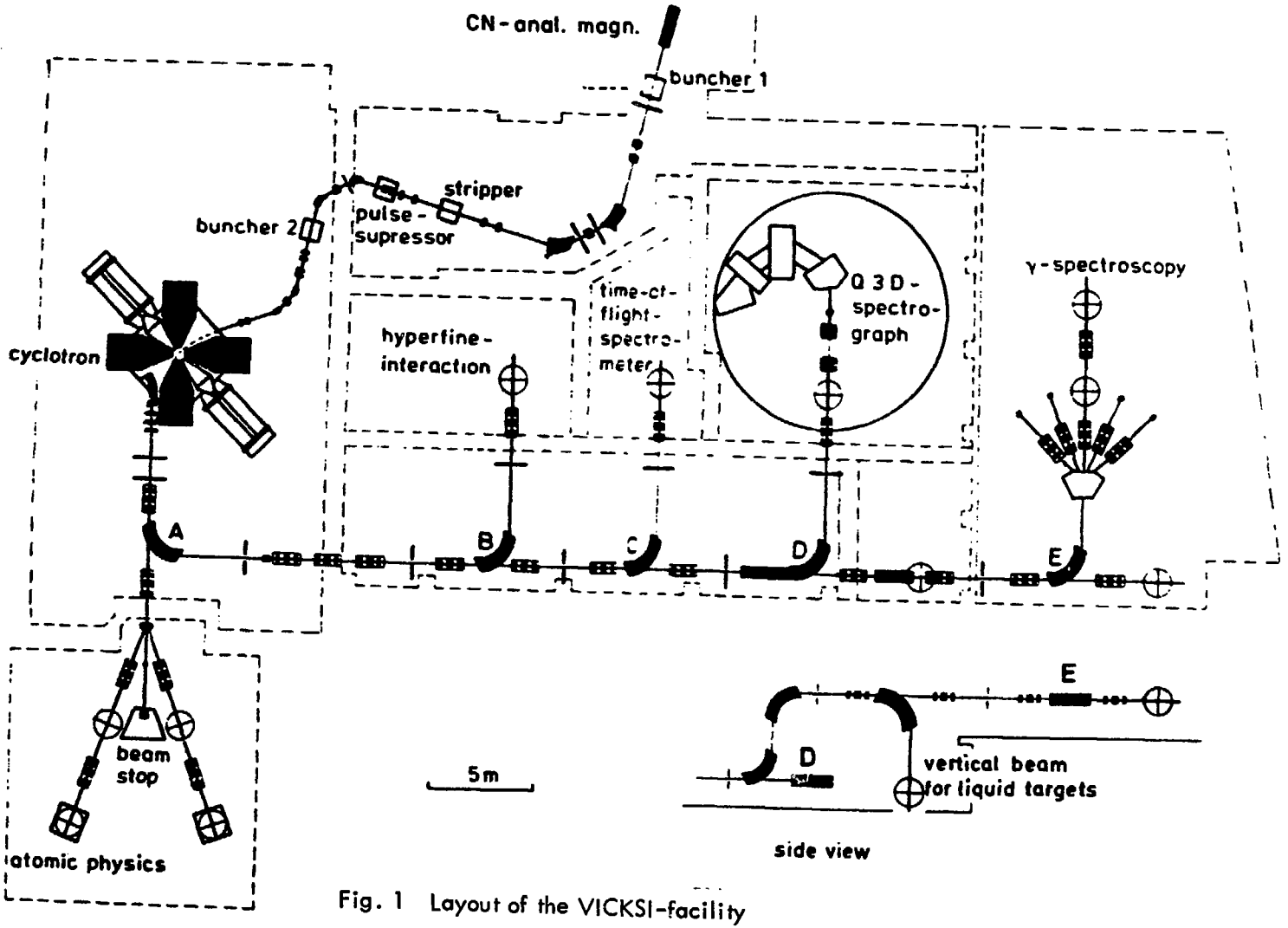


Fig. 1 Layout of the VICKSI-facility

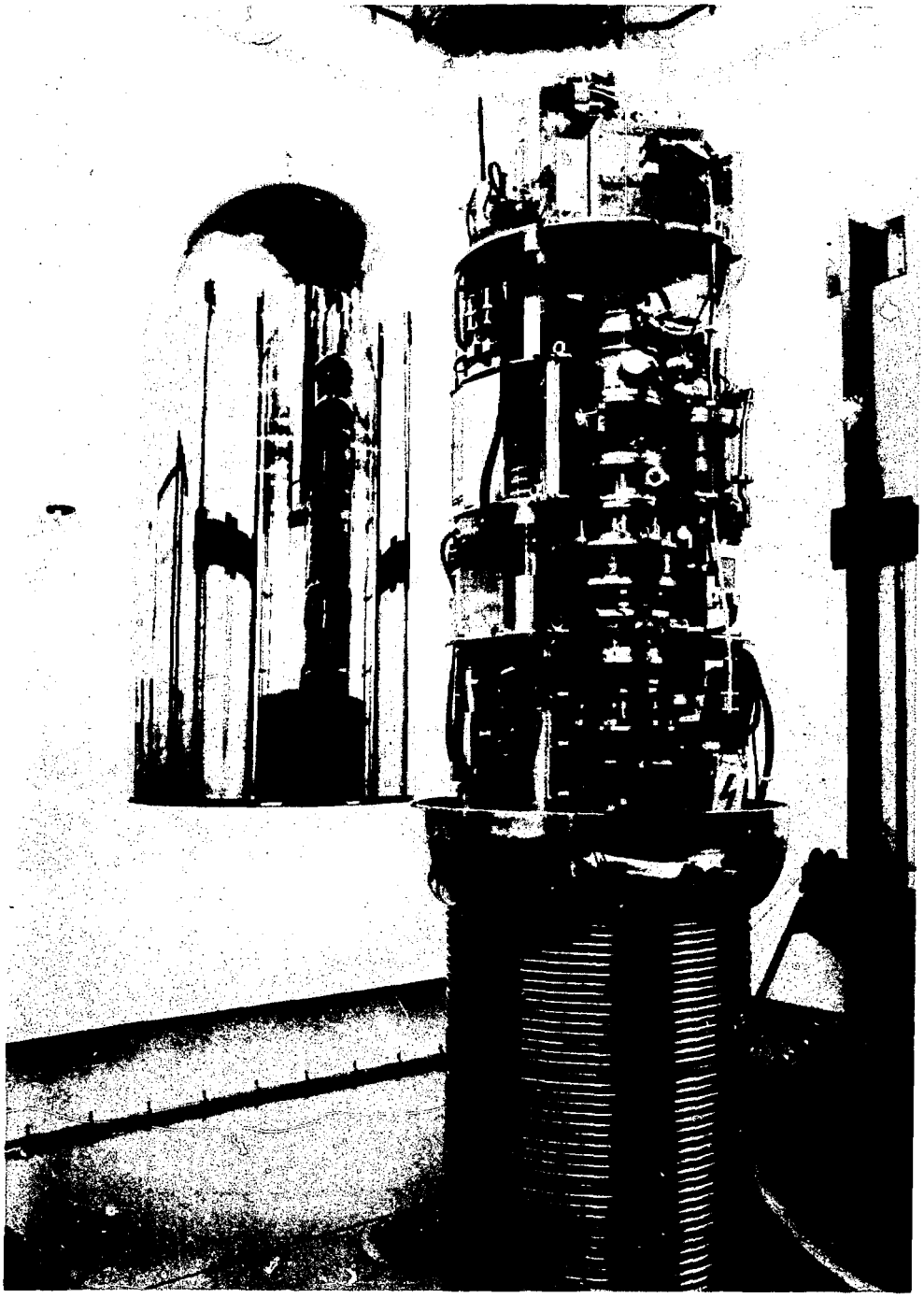


Fig. 2: The new terminal for the Van de Graaff Injector.

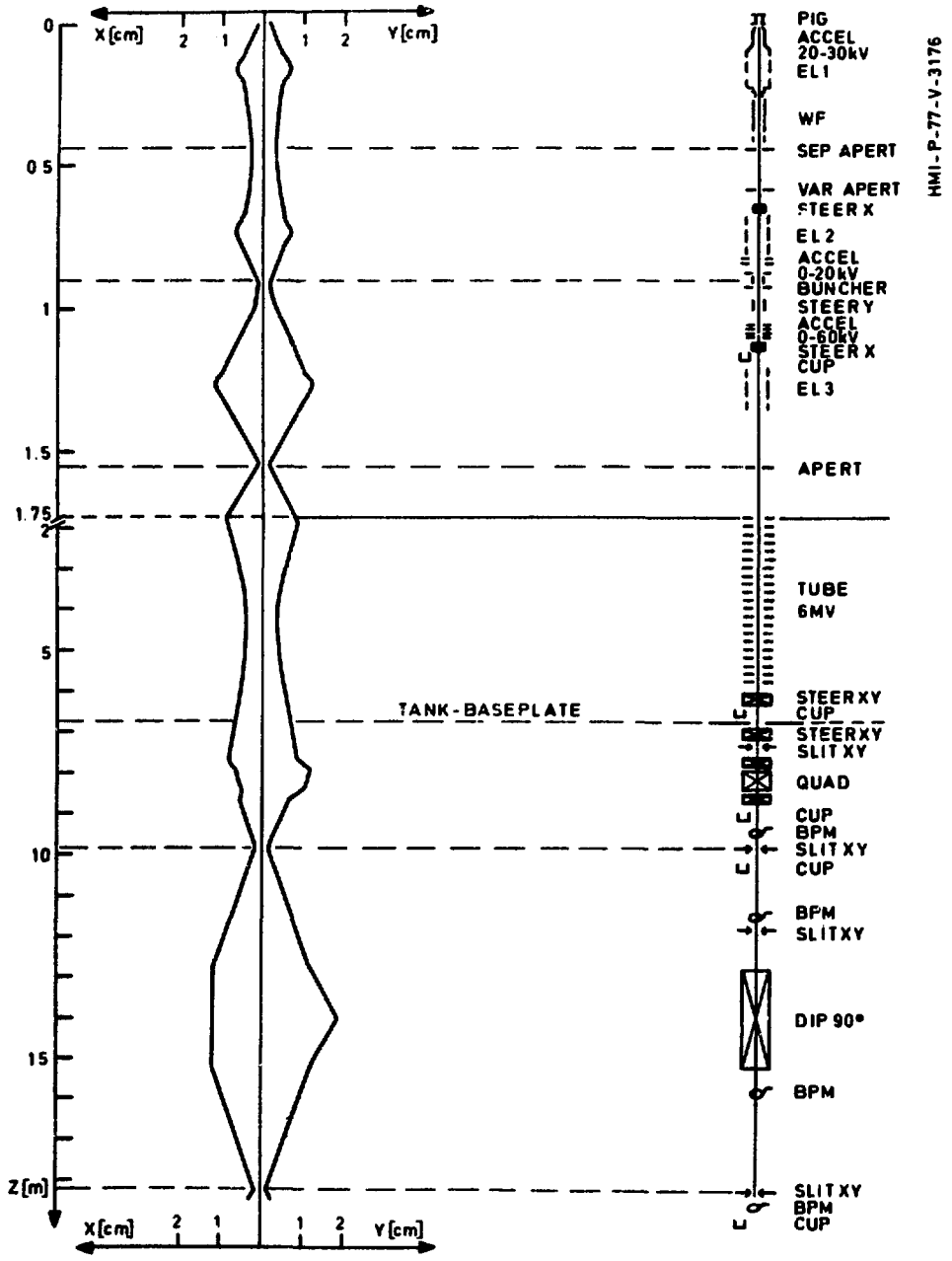


Fig. 3: Beam line elements and beam envelope from the ion source to the analyzing slits of the injector.

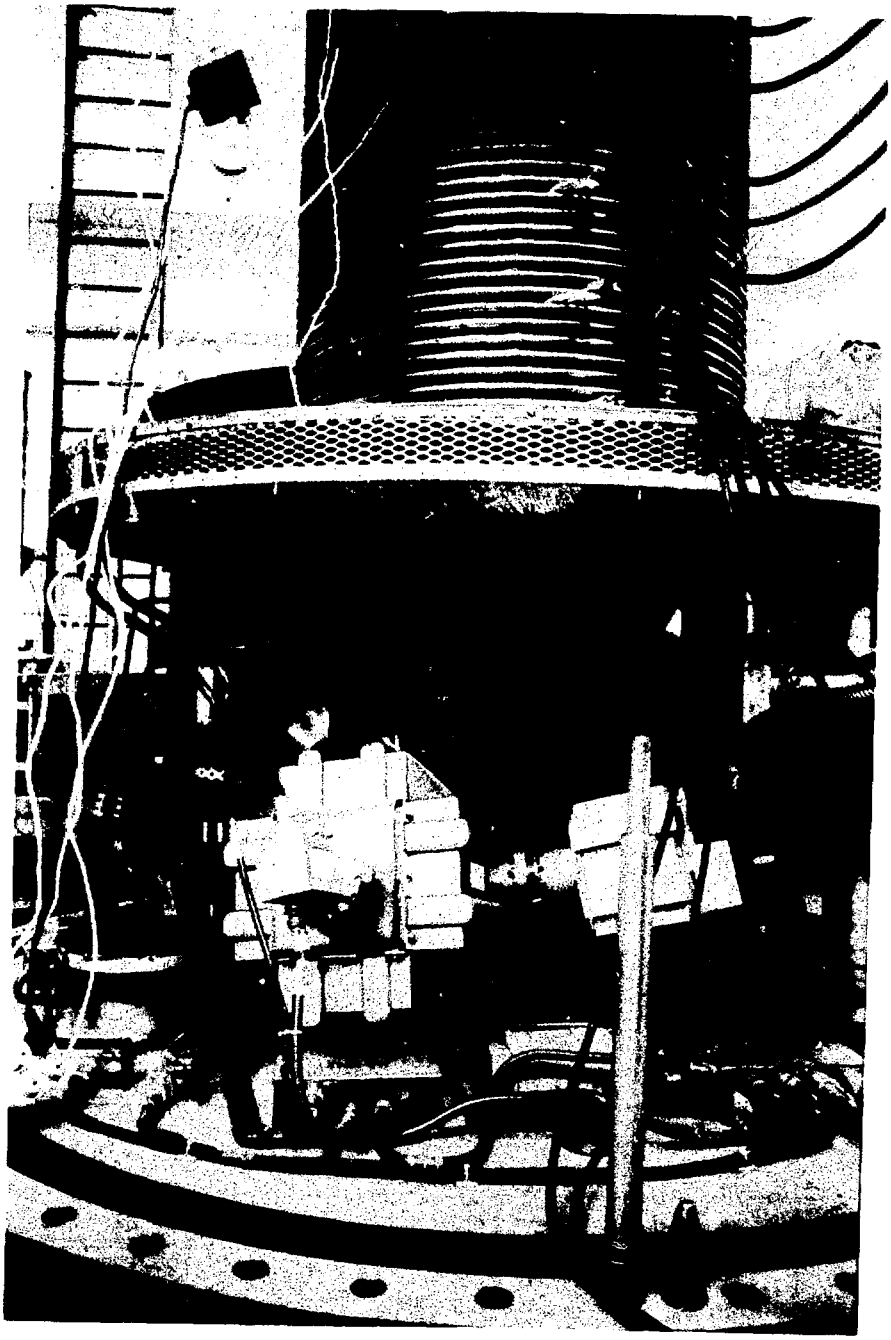
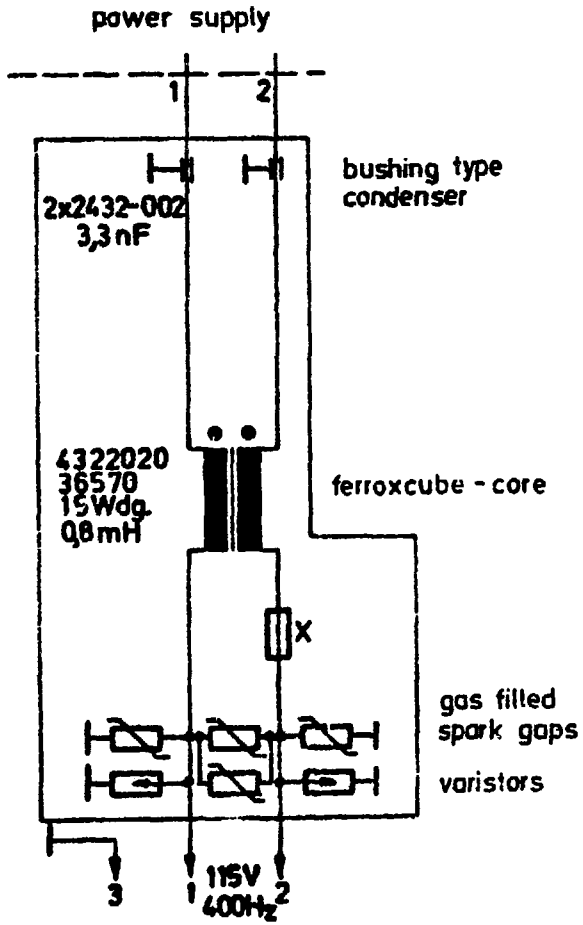
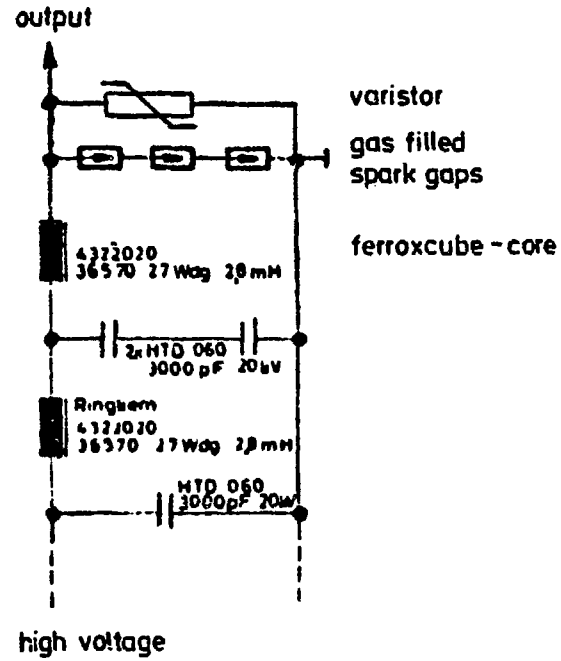


Fig. 4: Ion getter pumps and tube support at the base plate of the injector. The power cables for the heating of the apertures in the tube are connected. At the right hand side the heat exchanger (water-tank gas) and the support for the cooling tube can be seen.





Output voltage protection
of a high voltage power supply

Fig. 5: Power supply protection circuits in the terminal.

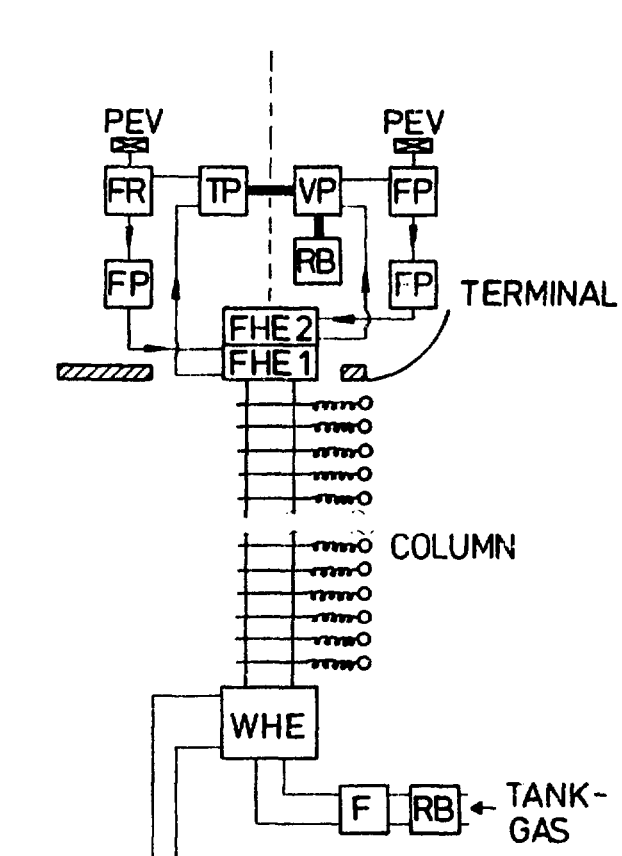


Fig. 6: Terminal cooling system.

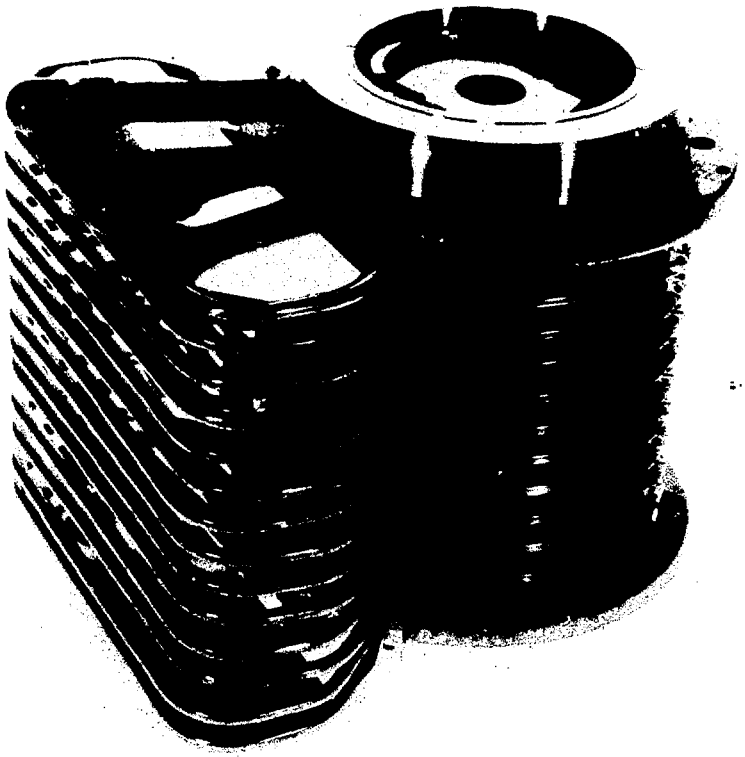


Fig. 7: NEC-tube module with resistor chain.

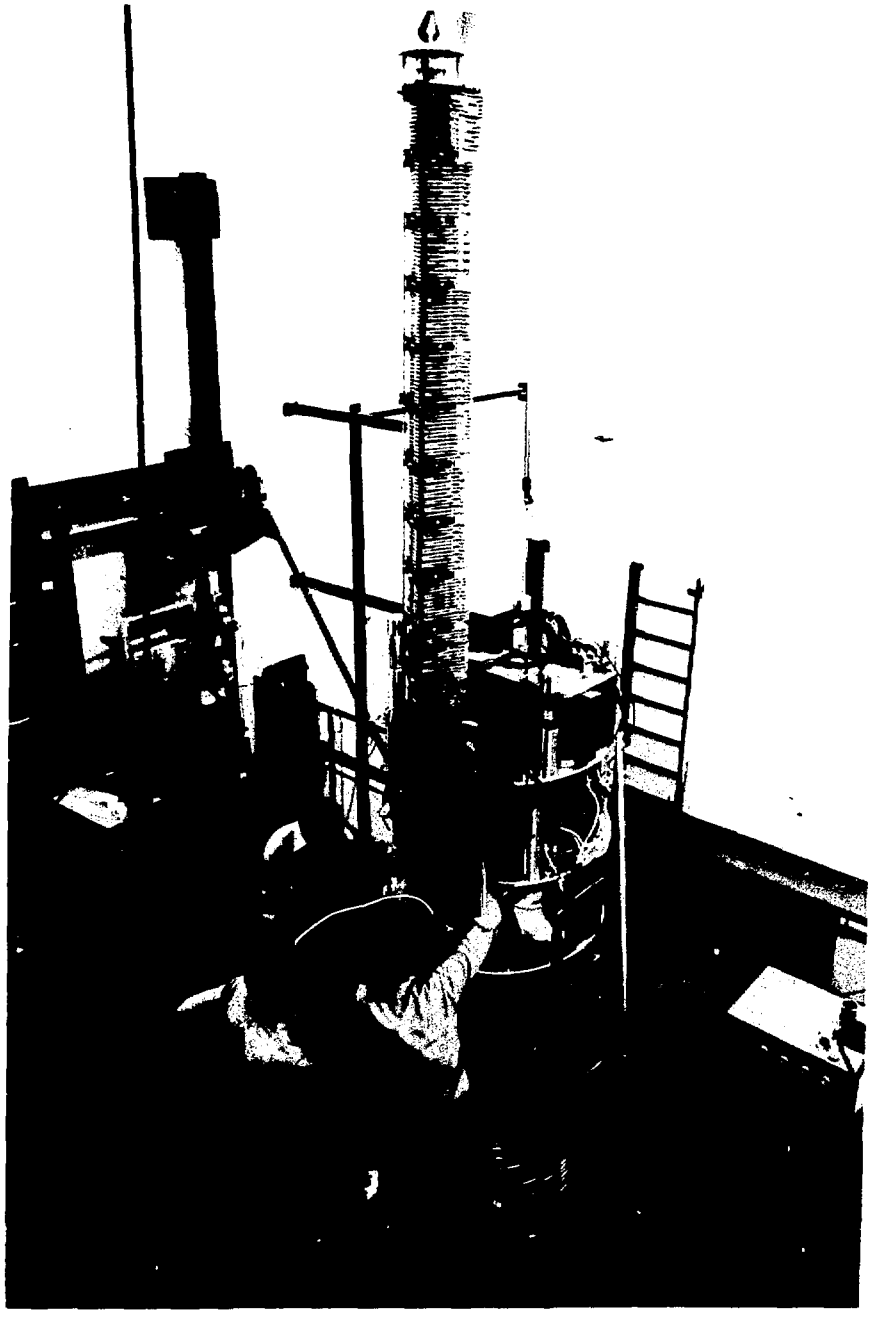


Fig. 8: Lowering the tube in the column.

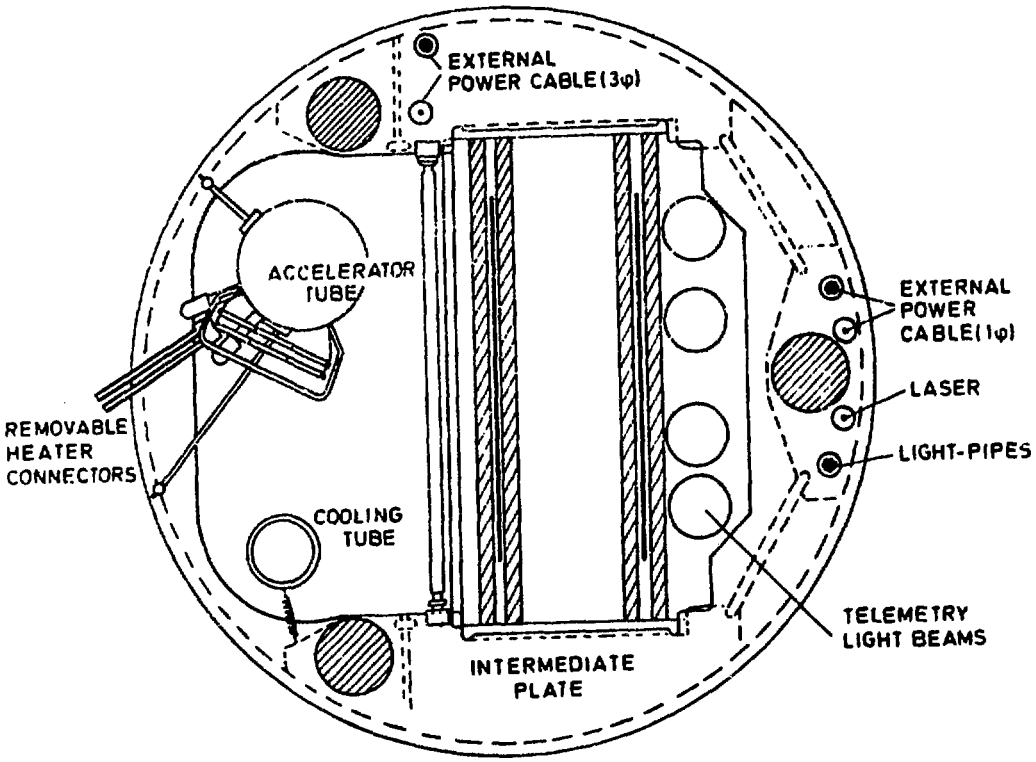


Fig. 9: cross section of the column structure

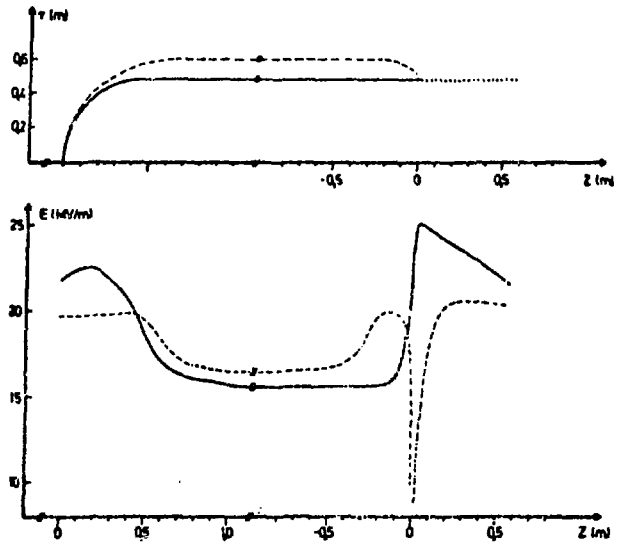


Fig. 10: Results of the field calculations for the new spinning.

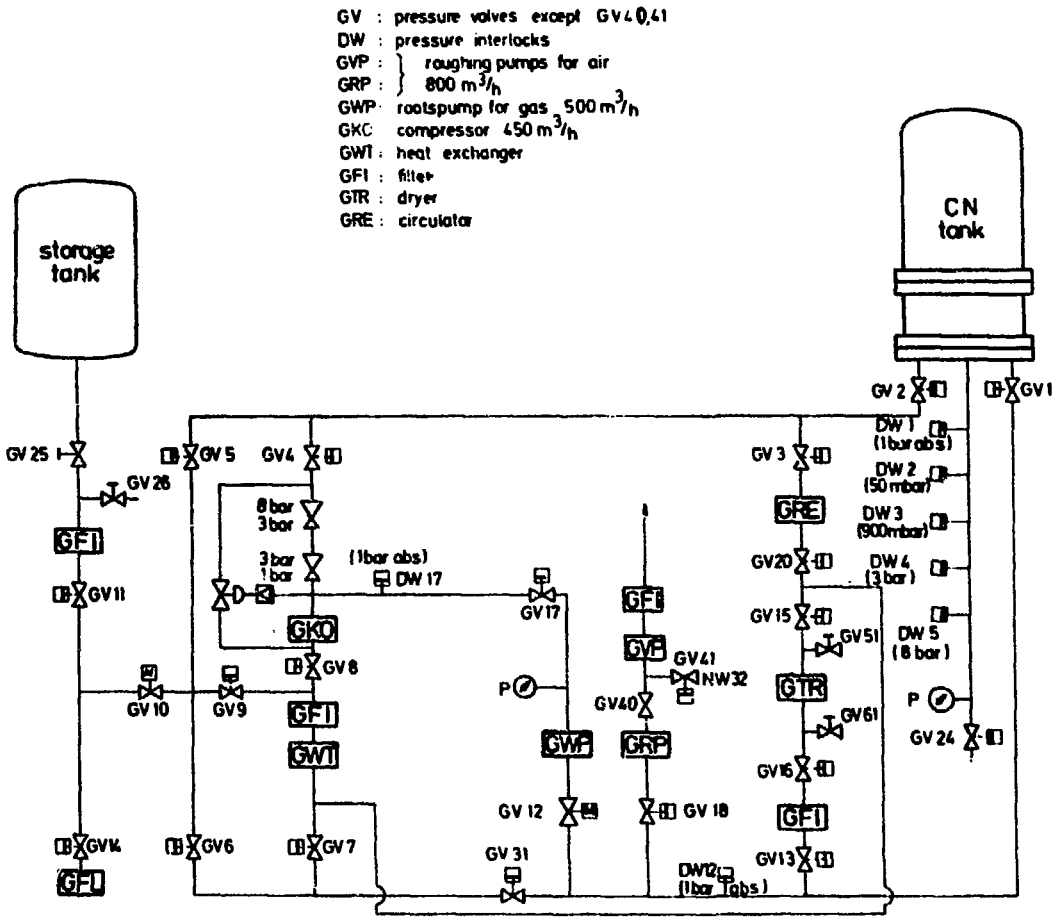


Fig. 11: Layout of the Gashandling system.

Fig. 12: The VICKSI-Cyclotron.

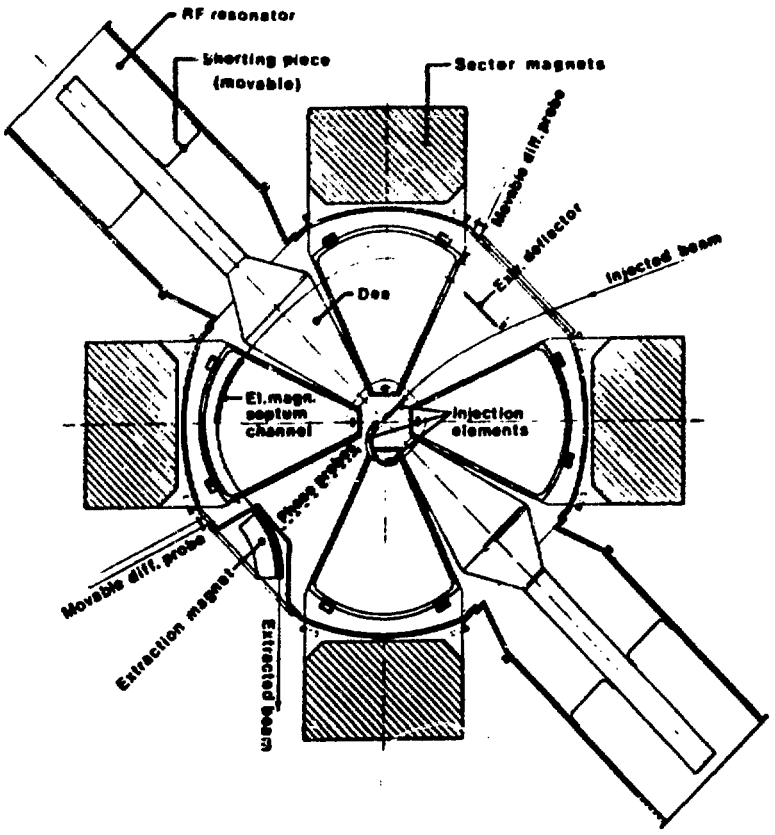


Fig. 13: Cross section of the cyclotron.

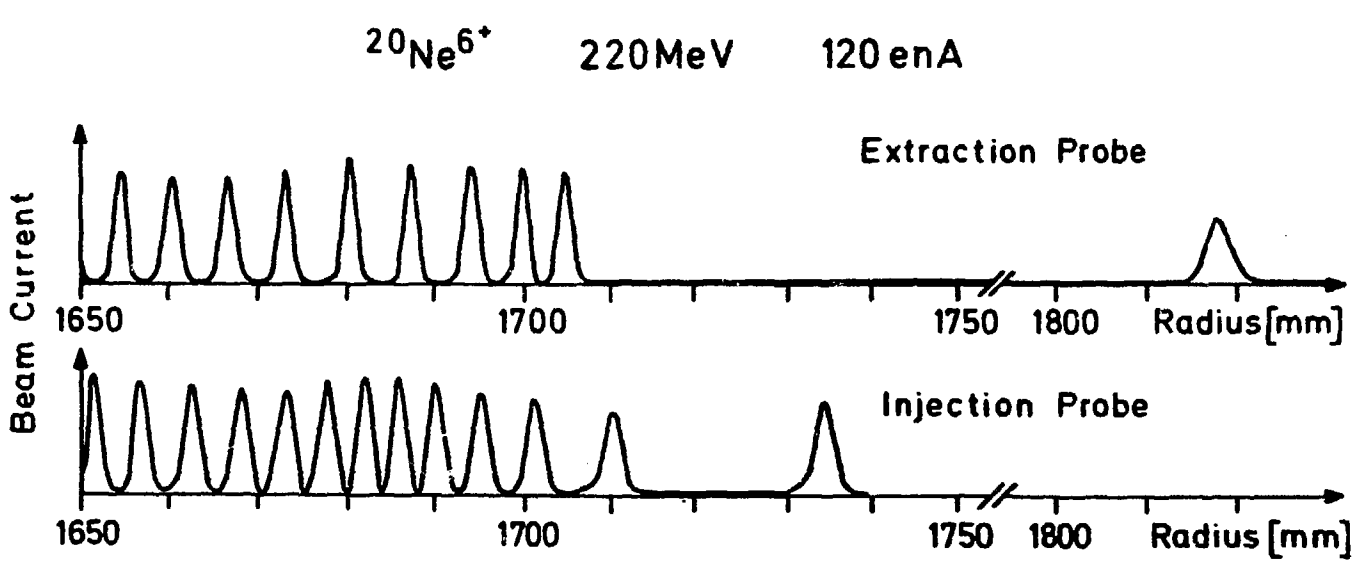


Fig. 14: Turn pattern near the extraction radius taken with the radial propes in the injection valley and the extraction valley. The turn at 1733 mm radius of the injection probe is behind the electrostatic deflector E1. On the extraction probe the turn at 1820 is in front of the extraction magnet.

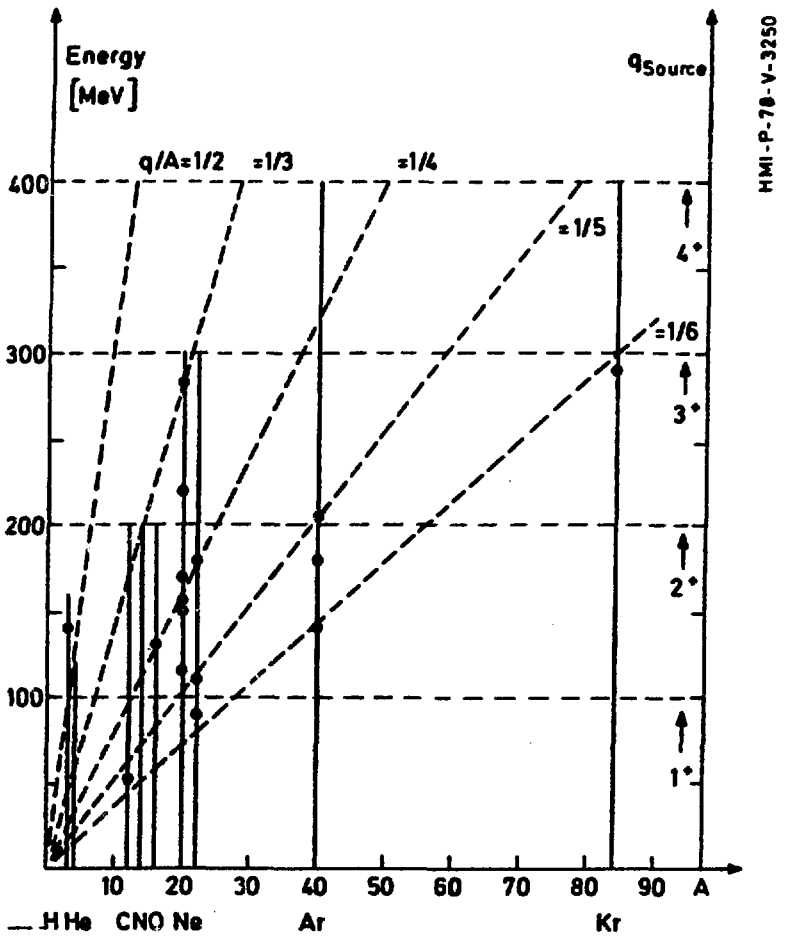


Fig. 15: Summary of beams produced so far.

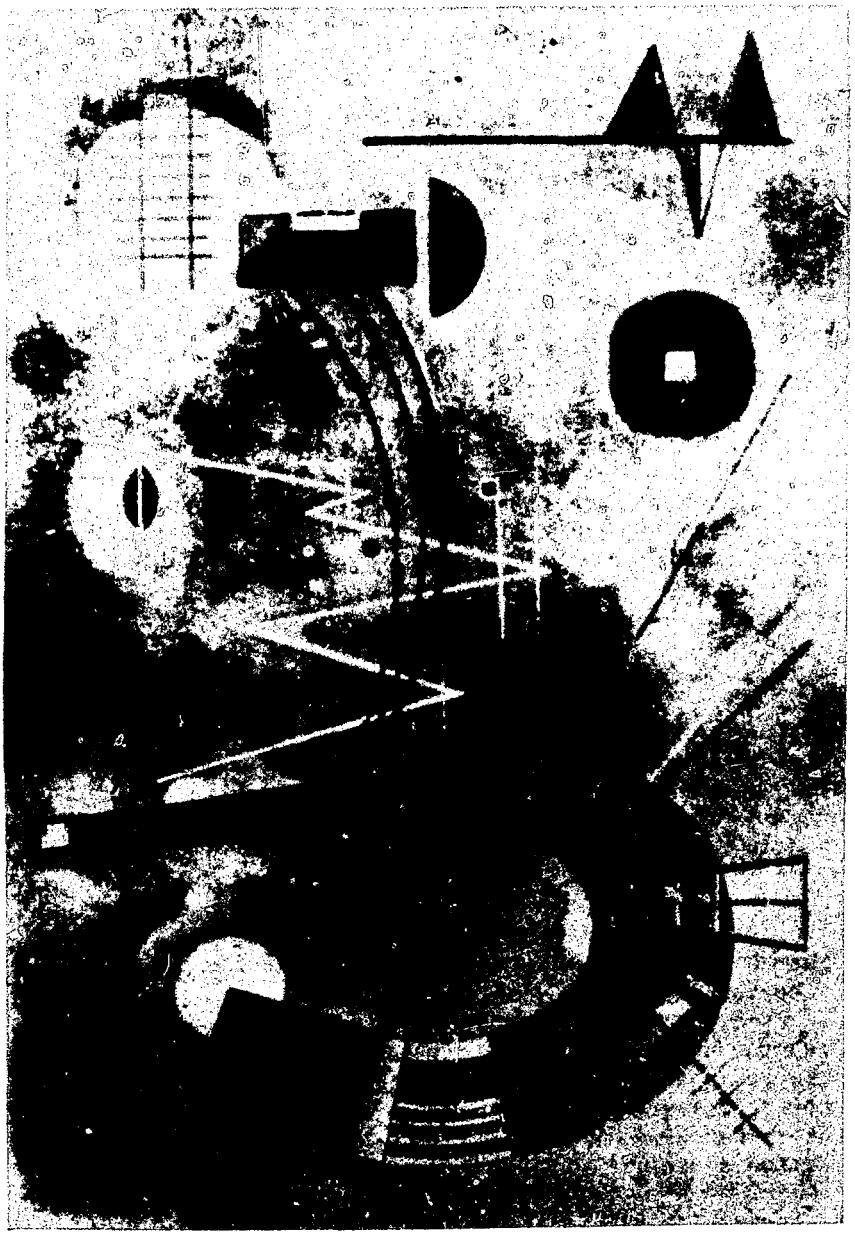


Fig. 16: Picture painted by Wassily Kandinsky (1930).

Dup

MODIFICATION OF THE ARGONNE TANDEM

J. L. Yntema

Argonne National Laboratory, Argonne, Illinois 60439

If one wishes to do nuclear structure experiments with heavy ions, it is necessary to have ion energies in excess of 5 MeV per nucleon. Obviously, a FN tandem by itself cannot produce such energies for ions heavier than ^{19}F. It is therefore necessary to post accelerate the tandem heavy ion beam to attain the energies necessary for the experimental program. A Superconducting Linac post accelerator has been under development at Argonne for several years. The modification of the Argonne tandem was designed to make it a suitable ion source for the post accelerator.

The installation was directed by Peter Billquist, who also took care of the ion source development work. The terminal design and vacuum system modifications were the responsibility of Floyd Munson and Charles Heath. The installation of the 150 kV injector was largely done by Pat Den Hartog. The ion beam optics calculations were done by W. Mourad of NEC and John Erskine of ANL. We also had six technicians working on the installation and about 20 man days of assistance from National Electrostatic Corporation. The modification interrupted the research program for four months.

The linear accelerator is a pulsed machine operating at a frequency of about 97 MHz. It is clear that if one wishes to have an adequate beam of heavy ions to carry out an experimental program, it is necessary to bunch the heavy ion beam prior to injection into the FN tandem. This is immediately evident if one considers the intensity of Ca, Mg and Ti beams produced by present ion sources. The quality of the experimental data obtained in nuclear structure experiments with the tandem-linac system depends on the longitudinal emittance $\Delta E.\Delta t$ of the beam at the output of the system. If $\Delta E.\Delta t$ is small enough at its input, the linac will not deteriorate the longitudinal emittance. Therefore, the emittance and thereby the quality of the experimental program are determined essentially by the tandem and the purpose of the modifications was to achieve as good a preservation of the longitudinal $\Delta E.\Delta t$ and transverse (x,x',y,y') emittance during acceleration through the tandem as possible. According to Liouville's theorem, one cannot improve the emittance but it is certainly possible to deteriorate it. The emittance of the tandem beam is affected by the acceleration and by the ion source injection system.

The factors which affect the transit time through the accelerator are the terminal ripple, the voltage distributions along the accelerator and fluctuations in beam trajectory. The charging system of the Argonne tandem was changed from belt charging to Pelletron chains about 4 years ago. The terminal ripple is now normally ±200 volts with a main

component of 0.6 Hz. The chains have been essentially trouble free for the past 4 years. Of course, the total upcharge to the terminal is only about 160 μa. The limited upcharge made it necessary to change the voltage distribution along the column structure from resistors to an independently pressurized closed corona system. This system has also worked very well after some initial teething problems. The lifetime of the corona needles is in excess of 10,000 hours. The total downcharge needed to run the machine is about 70 μa which leaves about 90 μa for the beam.

The transit time of heavy ions through the accelerator is of the order of a few microseconds. In order to successfuly operate the tandem-linac system, it is desirable to keep transit time fluctuations well below 1 nanosecond. Such transit time fluctuations are primarily caused by fluctuations of the voltage distribution along the accelerator. To keep such fluctuations to a minimum, it is necessary to match the injection to the acceptance of the accelerator, to minimize losses due to ionization of the insulating gas and to prevent ions from hitting the electrodes in the high energy section of the machine. It was our opinion that one could achieve these conditions more readily in an axially symmetric acceleration structure and this was one of the reasons why the National Electrostatic accelerator tubes were selected. The guaranteed terminal voltage for the NEC tubes was 8 MV. For a tandem-linac system, the terminal voltage is not very important as long as the linac has resonators with the appropriate values of β. It is however quite important that the accelerator be very stable at high heavy ion currents. In our case, we can readily produce 4 μa of Cl^{8+}, O^{6+}, C^{4+} and 1.6 μa of $^{58}Ni^{10+}$ with the necessary stability. This is about an order of magnitude more current than targets used in nuclear structure experiments will usually be able to withstand without serious deterioration. The machine will operate with the needed stability at a terminal voltage of 8.5 MV and we expect eventually to achieve routine operations at 9 MV.

The arrangement of the accelerator tubes is shown in Fig. 1. It should be noted that tubes connecting the accelerator tube corona system are very close to the column electrodes and equipotential hoops. The potential difference between the column and the corona system can be as high as 100 kV and some care has to be exercised in the installation. The machine at this time is only equipped with foil stripping. The foil changer is a 230 foil NEC model. In order not to vent the accelerator tubes when changing foils, terminal valves similar to the ones developed by Weiser at ANU have been built and installed. This valve is shown in Fig. 2. These valves have worked very well and allow us to reload the foil changer and return to 8 MV operation in less than 24 hours. A view of the terminal is shown in Fig. 3. There are two mini Ti-Ball sublimator pumps in the terminal. However, since the pressure in the terminal is about the same as the pressure at the high energy end of the machine, approximately 2×10^{-8} torr, these pumps are not used in normal operation.

The ion source arrangement is shown in Fig. 4. There are three ion source positions — one is occupied by the direct extraction duoplasmatron, one by the Li exchange source, and one by the 150 kV preacceleration system, which normally uses the Chapman source. The ion optics for the 150 kV injection is shown in Fig. 5. The source is followed by einzellens L_1. The three preacceleration tubes A_1 A_2 A_3 are NEC multi-purpose tubes. These tubes can operate at 75 kV each. In our arrangement, the tube A_1 is at 1/3 the voltage gradient of A_2 and A_3 to allow formation of an object with 0.7 mm radius at W_1. The magnet M has a mass energy product of 20 for 40° injection. The buncher B is the single gap pretandem buncher developed by Bollinger, Lewis, Lynch and Henning. The beam is focussed by an electrostatic quadrupole to form the object for the accelerator tube with a radius of 1 mm at W_2, about 43 cm. from the accelerator tubes entrance. The corona tube of the first section of accelerator tube in the machine is arranged to give half the voltage gradient of the subsequent accelerator tube sections and the column structure corona in its first 8 sections from the low energy base has a similar arrangement. This is done to move the cross over point further away from the machine in order to produce a small enough beam spot, at the terminal. The beam diameter at the terminal is about 4 mm. The system was intended to be a constant Q system. However, in practice, it is not operated that way. It is much easier to achieve good matching of the ion source emittance with the accelerator tube acceptance with the axially symmetric accelerator tube than with the previous inclined field tube. For 150 kV injection at 9 MV on the terminal, the inclined field vertical waist would have been at 49.3 cm and the horizontal waist at 91.5 cm from the accelerator tube entrance. The layout of the elements between the injection magnet and the accelerator tube is shown in Fig. 6. The Ultek cryopump is attached to the housing of the first steerer. In normal operation, it pumps the magnet chamber to pressures in the low 10^{-8} torr range. The 1 inch valve is an all-metal NEC straight through valve. The einzellens located just before the tank wall was installed to allow injection of the 40 kV He^- beam. The quadrupole triplet would produce too large an object for the accelerator tubes for the lower voltage. The einzellens housing is connected to a 400 ℓ. Ultek DI pump through a 4 inch right angle all-metal valve. To reduce contamination problems, the DI pump will be replaced by a Granville Philips pump. The main purpose of the 4-jaw slit is to improve the mass resolution of the injection system. We expect that a mass resolution of about 1 part in 75 will be achieved. The waists of the ion beam are approximately in the center of the second section of the deflection plates X_2 Y_2 and this does reduce the effectiveness of the 4-jaw slit somewhat.

The 150 kV injection system is shown, with the source developed by Ken Chapman in Fig. 7. The beam line between the preaccelerator tubes and the injection magnet is shown in Fig. 8. Besides the einzellens L_2, it shows the location of the selectable aperture. Four apertures ranging from 3 mm diameter to 19 mm diameter are available. They are used primarily to attenuate the beam from the ion source when the experimentally desired beam intensity changes. The ion source einzellens and extraction supplies are similar to the ones of O'Dacre at Chalk River and allow operation from the control room.

To allow successful operation of the tandem-linac system, the energy spread of the beam at the buncher should be less than about 30 eV out of 132 keV. The 150 kV acceleration supply is a HVEC Deltatron with very low ripple and high stability.

Some of the problems which may occur in a tandem-linac system are best illustrated by some results obtained with the bunching system. Figure 9 shows the time spectrum from the 56 MeV O^{6+} beam from the sputter source with the prebuncher and the chopper using the phase stabilization system described in the paper by Lewis. This is obviously a very good bunched beam. If one now looks at the time spectrum obtained with this bunched beam after rebunching by the superconducting buncher at a point far past the time cross over point, one obtains the spectra labelled $\phi \neq 20°$ and $\phi = 0°$ in Fig. 10. Obviously there is some fine structure in the beam. Such a fine structure can be caused by a component of the 132 kV O^- beam with perhaps 100 eV less energy. Such components are readily introduced in O beam from sputter sources because one gets O^- beams from everywhere. Such a lower energy component cannot be resolved by the 90° analyzing magnet because the energy difference is only about one part in 500,000. However, the slow component arrives too late at the superconducting buncher and therefore gets a net energy gain greater than its energy deficiency and gives a high energy tail to the pulsed beam.

The adjustments made by Billquist to the ion source were an increase in the oxygen gas flow and selection of a smaller aperture in the variable aperture box. The low energy and retuning is essentially a touching up of the parameters required by the change in aperture. The improvements are obvious in the third spectrum. The energy spread of ions from the direct extraction source is too large to permit use of this source for the tandem-linac system as shown in Fig. 11.

In summary, the Argonne tandem has been improved as an injector for the superconducting linear post accelerator and is now the determining factor for the quality of data obtainable in nuclear structure experiments with the tandem-linac system. The intensity and stability of heavy ion beams has been substantially improved. The transmission of Ni beams is improved by a factor of 8. As a stand alone tandem, it has now a somewhat lower terminal voltage. However, it is quite likely that with increased operating experience, the terminal voltage will exceed 9 MV.

Discussion:

Miller: As I understand it, on your last slide, those were of order 25 picoseconds in width?

Yntema: I don't know what each channel was, it is marked on the slide someplace, we can look at that. The total width of the pulse should be about 25 picoseconds going into the linac because you have to stay within 3° of the 97 megahertz frequency.

Miller: How did you make that measurement? What was the instrumentation?

Yntema: The instrumentation here was a scattering chamber with a thin gold foil at the end of the linac and surface barrier detectors. The previous one was taken at the proper time focus for the pre-buncher and that was also done with surface barrier detectors some time ago.

Purser: I think its important to make the comment that at Rochester the average pressure in the tubes is probably about 10^{-6}. We have transmitted beams of 4 microamperes of gold through with an attenuation of about a factor of 2. The expectation is that if the pressure was reduced by pump in the middle of the tube, the transmission of gold would be essentially unity. The loss of the other particles would be essentially zero too. It looks as though 10^{-7} is the right pressure range on average for the acceleration tube to transmit particles without charge exchange losses. My first question is, what do you measure the pressure to be in your machine? The second point refers to the fluctuations of inclined field tubes. If you design them correctly and appropriately, and certainly there are designs that can be derived to do this, the movement of the beam is essentially zero. I wonder if you would like to comment on that point too?

Yntema: Far be it for me to argue with inclined field people. I only know what happened to the machine, to the way the machine was before and after, and I think that we had substantial trajectory fluctuations. Of course, we have the corona system with some conditioning going on in the accelerator tubes normally at any time otherwise they wouldn't accelerate, and if there is some voltage fluctuation, if this happens to be at the point where there is a strong focusing action in the inclined field tubes, at least in our case, it seems to have done something. Our pressure at the terminal is normally probably about 3×10^{-8}, our pressure at the high energy end is of the same order of magnitude, at the low energy end, it depends somewhat more on the source. It varies between about 2×10^{-7} and less. One of the problems we have is that if we start to inject more than two microamperes of

chlorine through the machine we get less through. The reason for that is that you get a lot of junk off a sputter source and this starts hitting our carefully baked magnet chamber and produces local pockets of gas and that apparently deteriorates the beam sufficiently so you can't get it through. So we really can't run very high injection currents, not because we can't make them or because the machine can't handle them, simply because the emittance doesn't seem to be so good. So I think that the normal pressure is in the order of 3-4 x 10^{-8} and that's because the accelerator tubes want to run there. We don't make any effort ourselves to be better or worse.

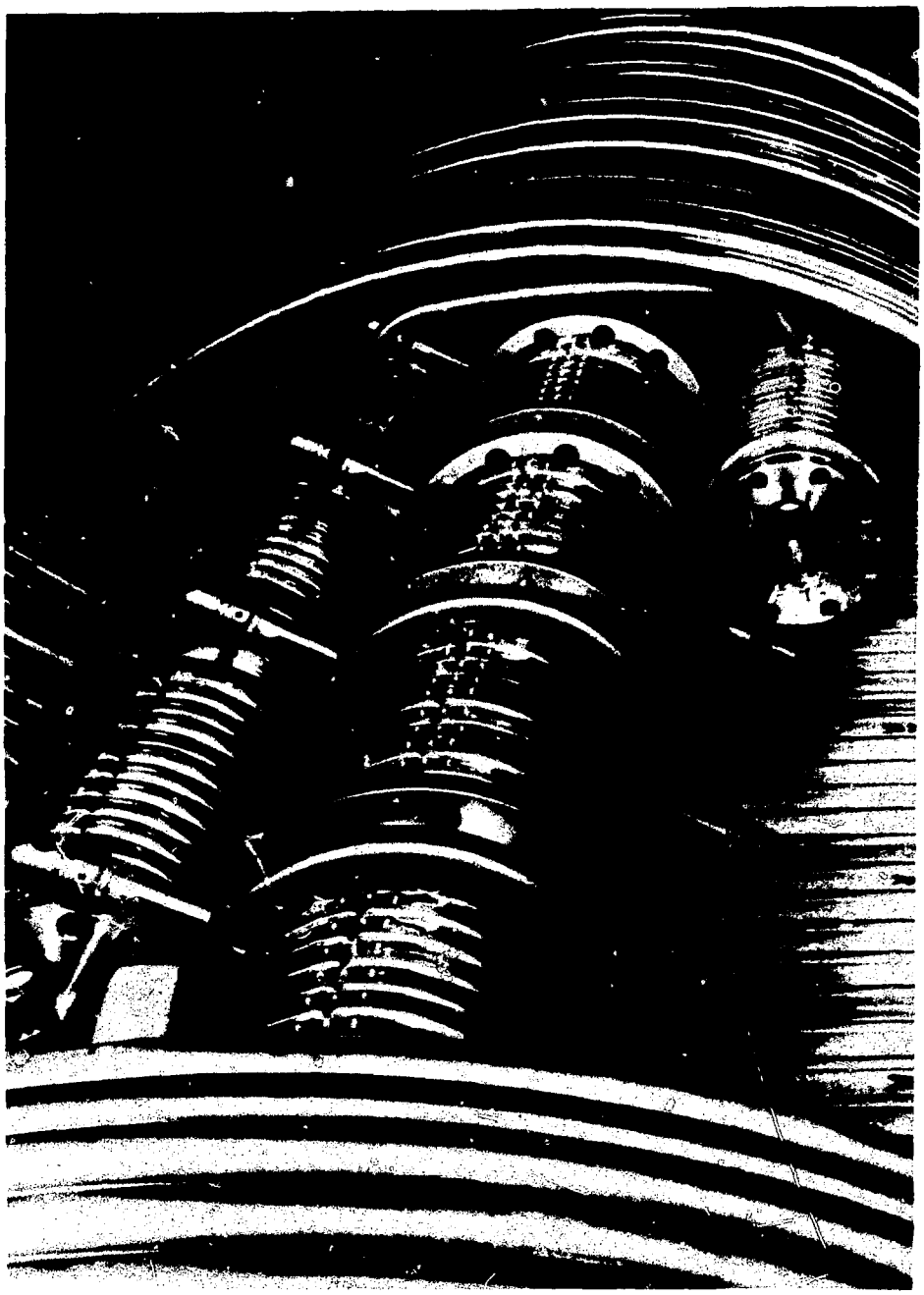


Fig. 1. NEC Accelerator tubes and corona systems mounted in the FN tandem.

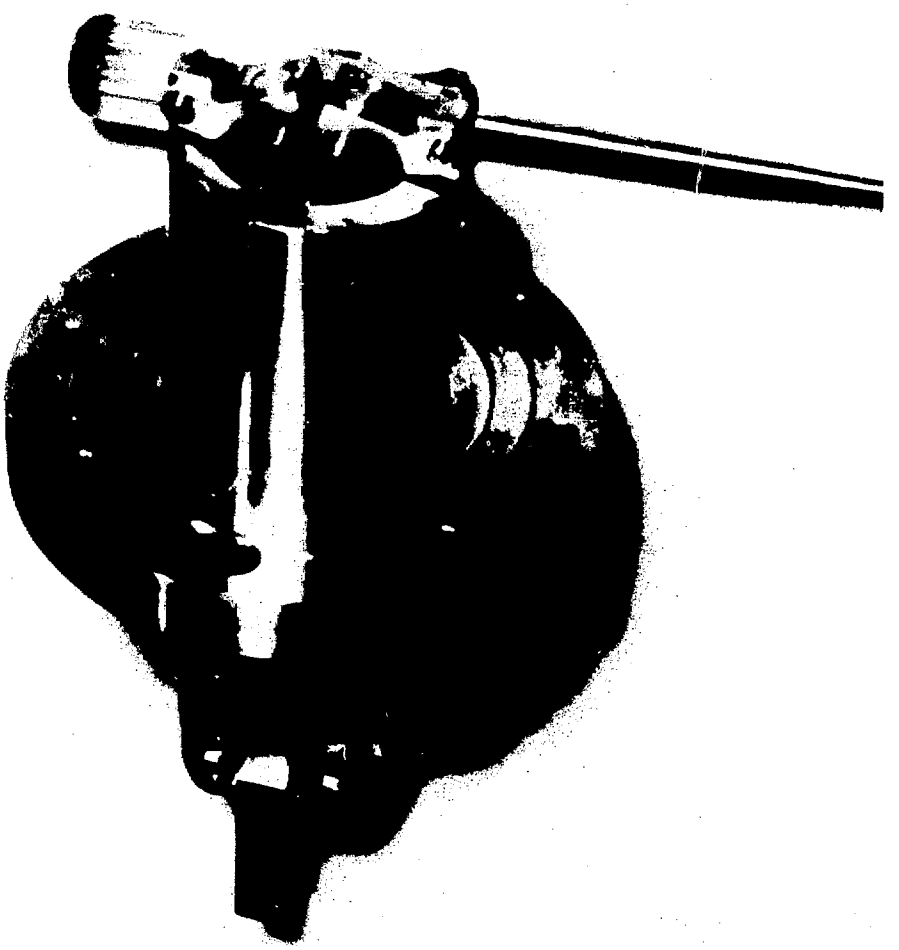


Fig. 2. Terminal all metal valve.

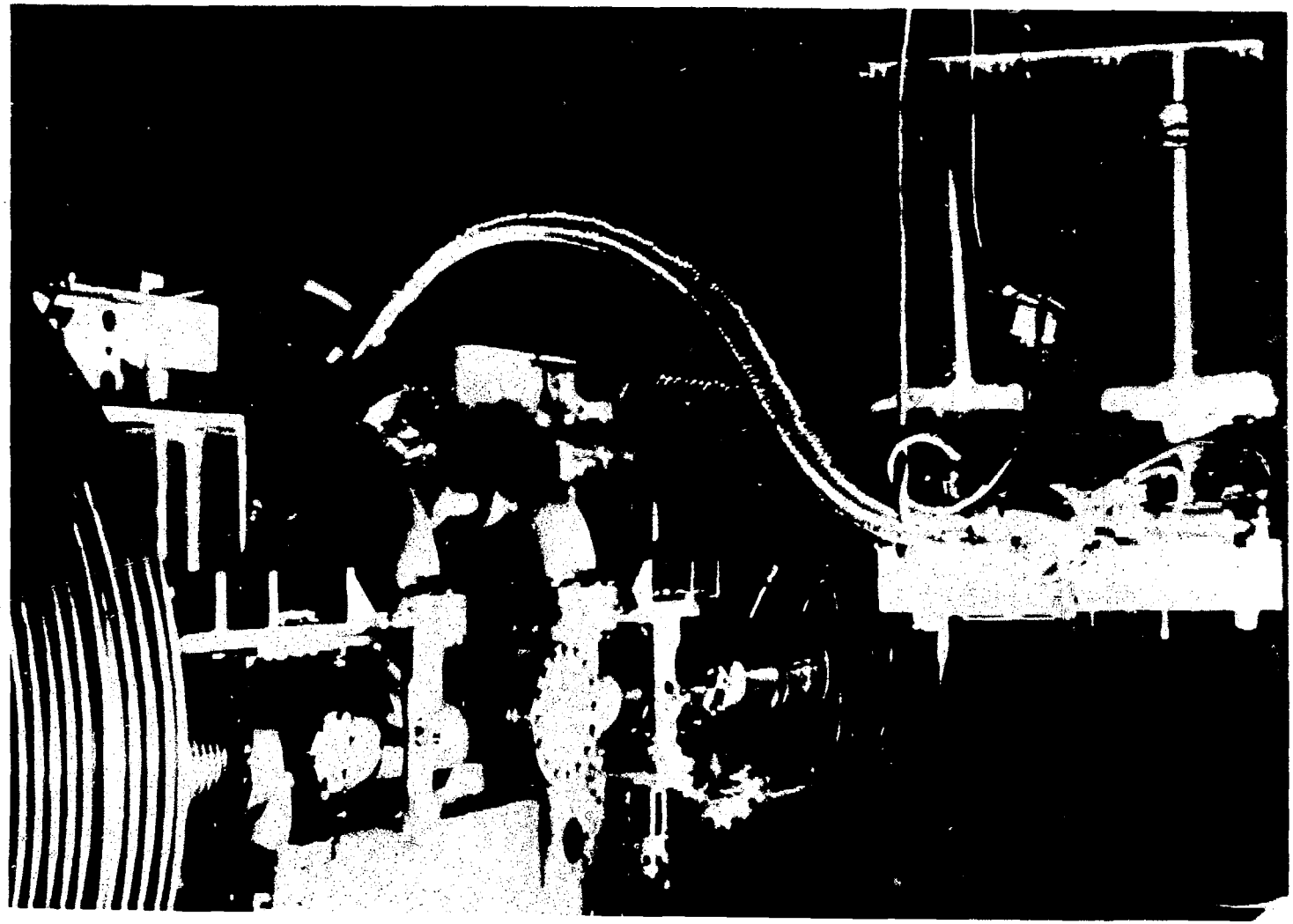


Fig. 3. View of the terminal with portable vacuum unit.

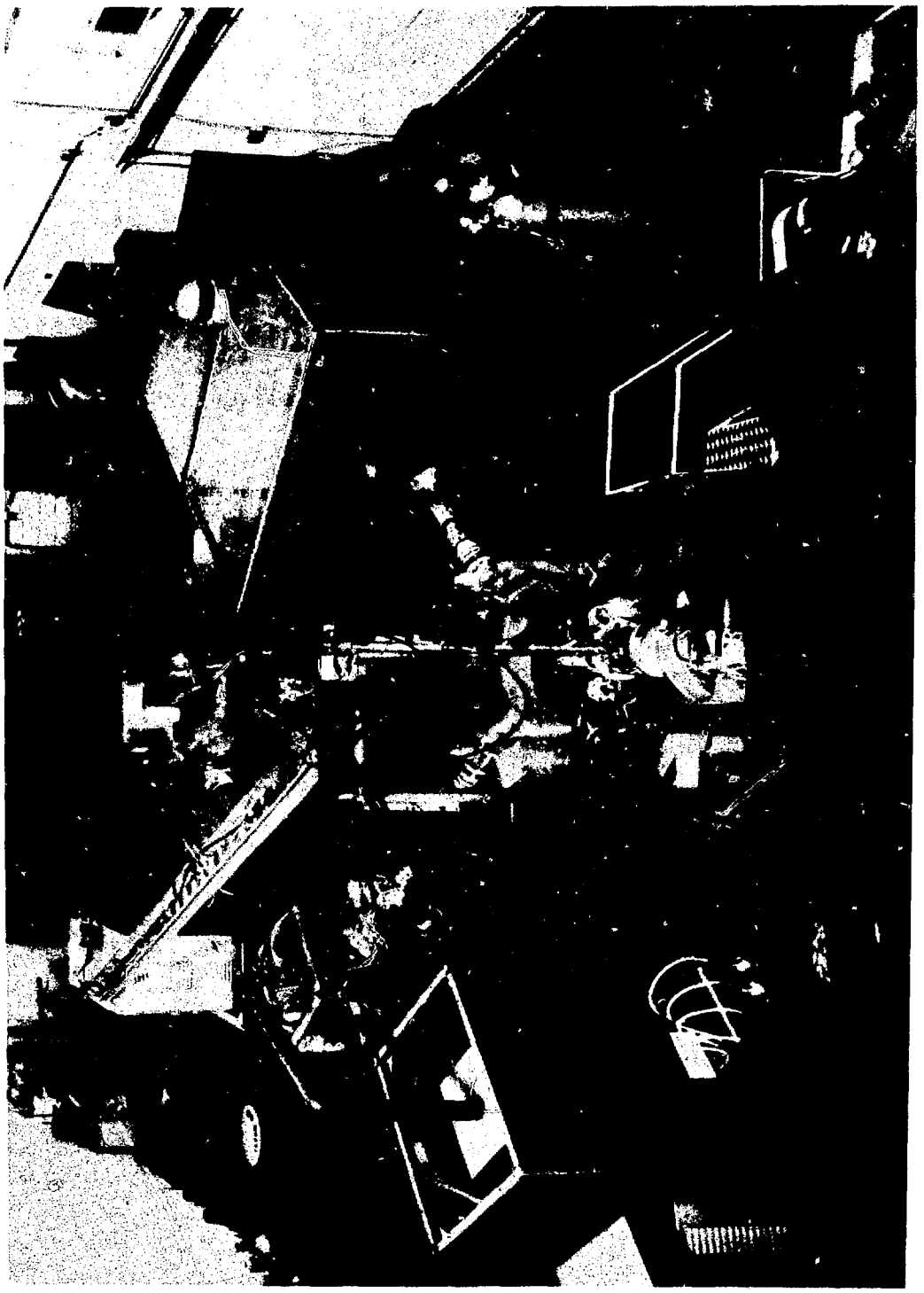


Fig. 4. Ion source area.

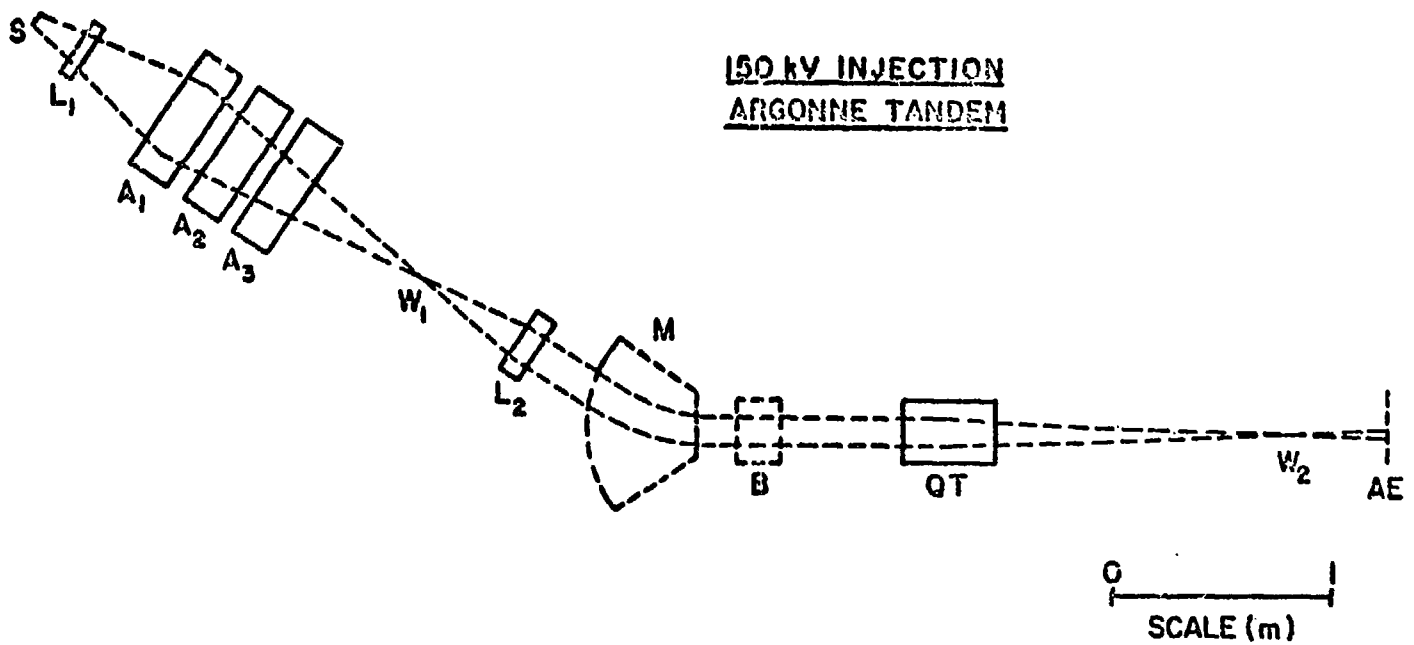


Fig. 5. Ion optics for 150 kv injection system.

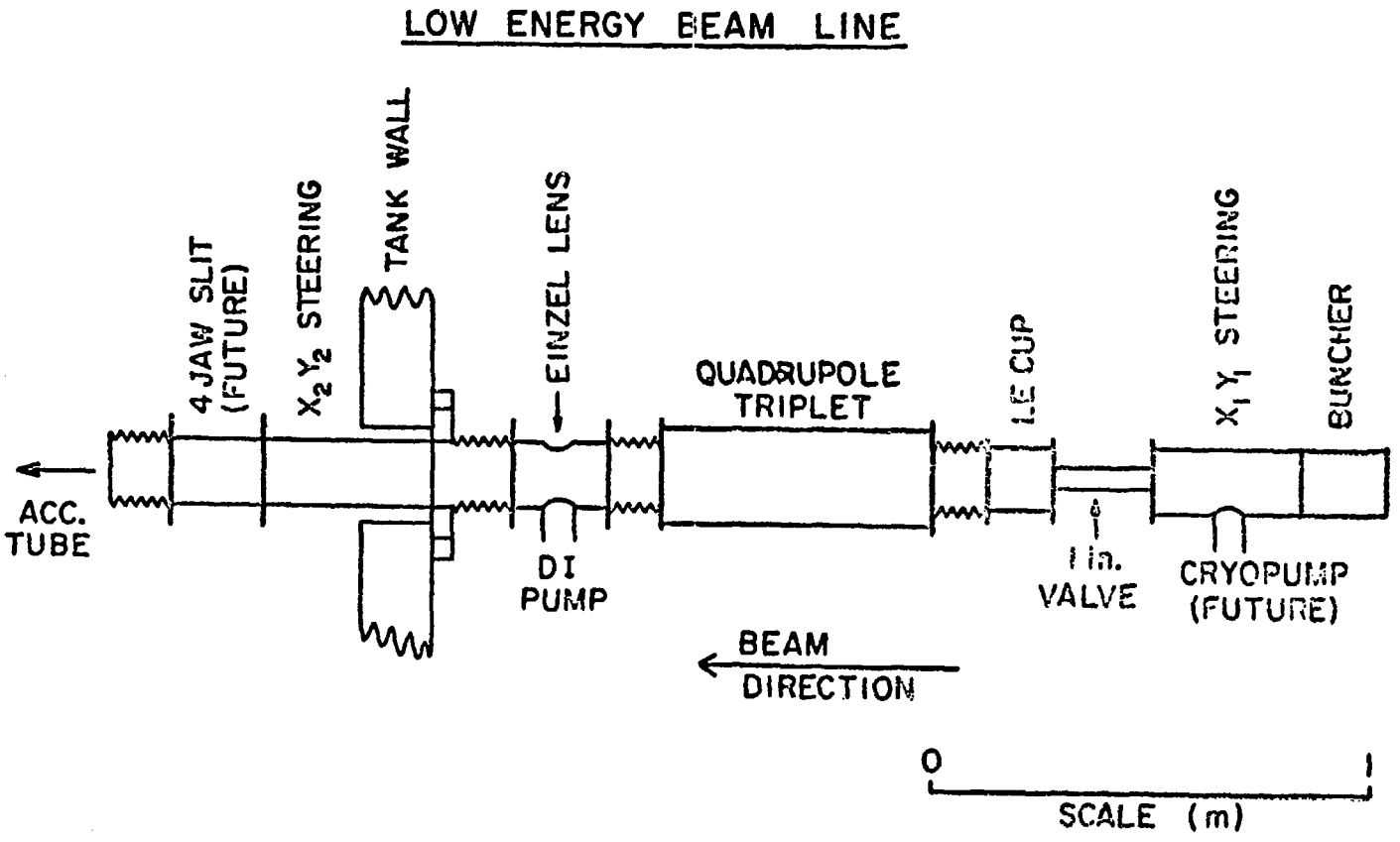


Fig. 6. Low energy beam line elements.

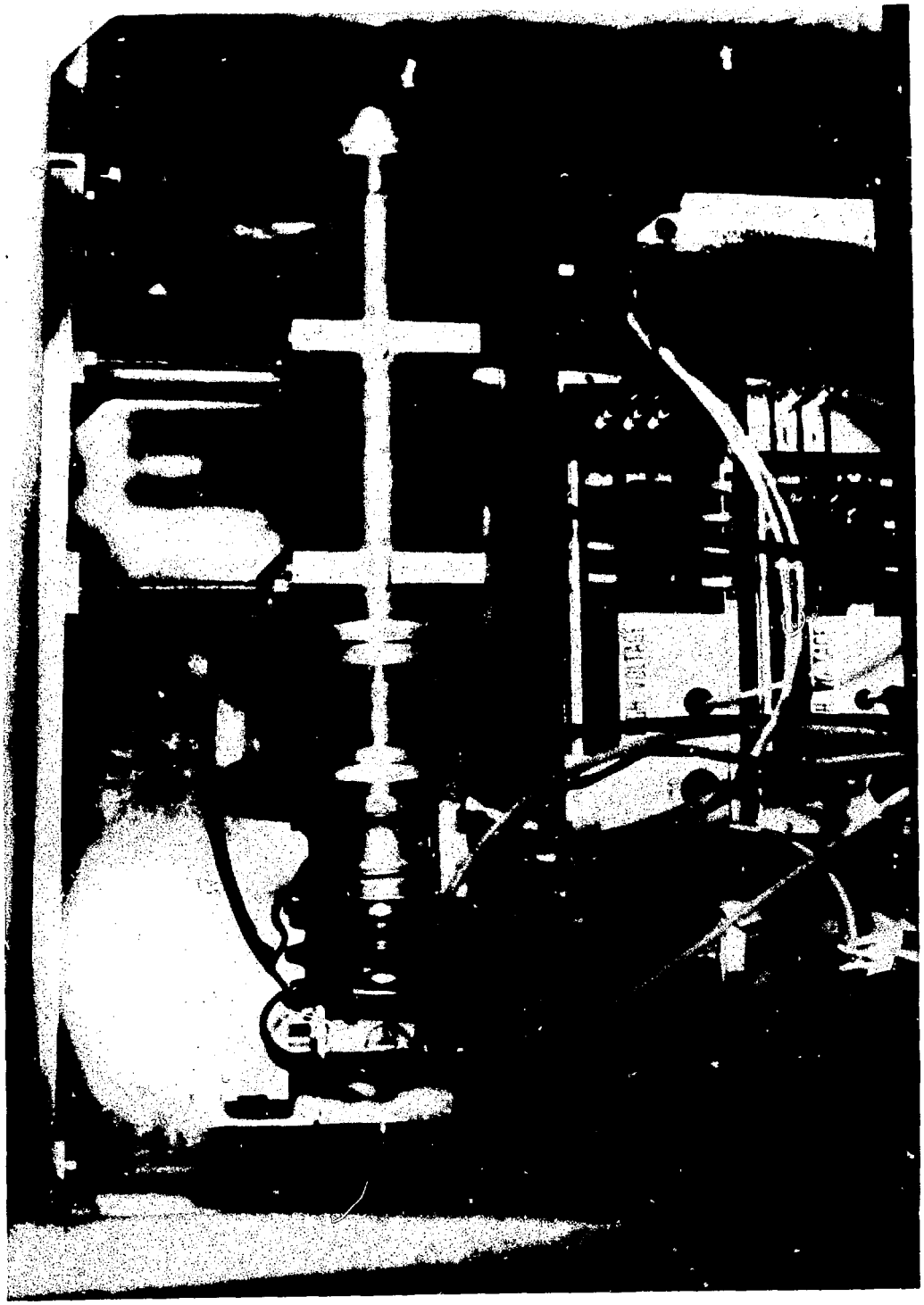


Fig. 7. 150 kv injection system with FSU source.

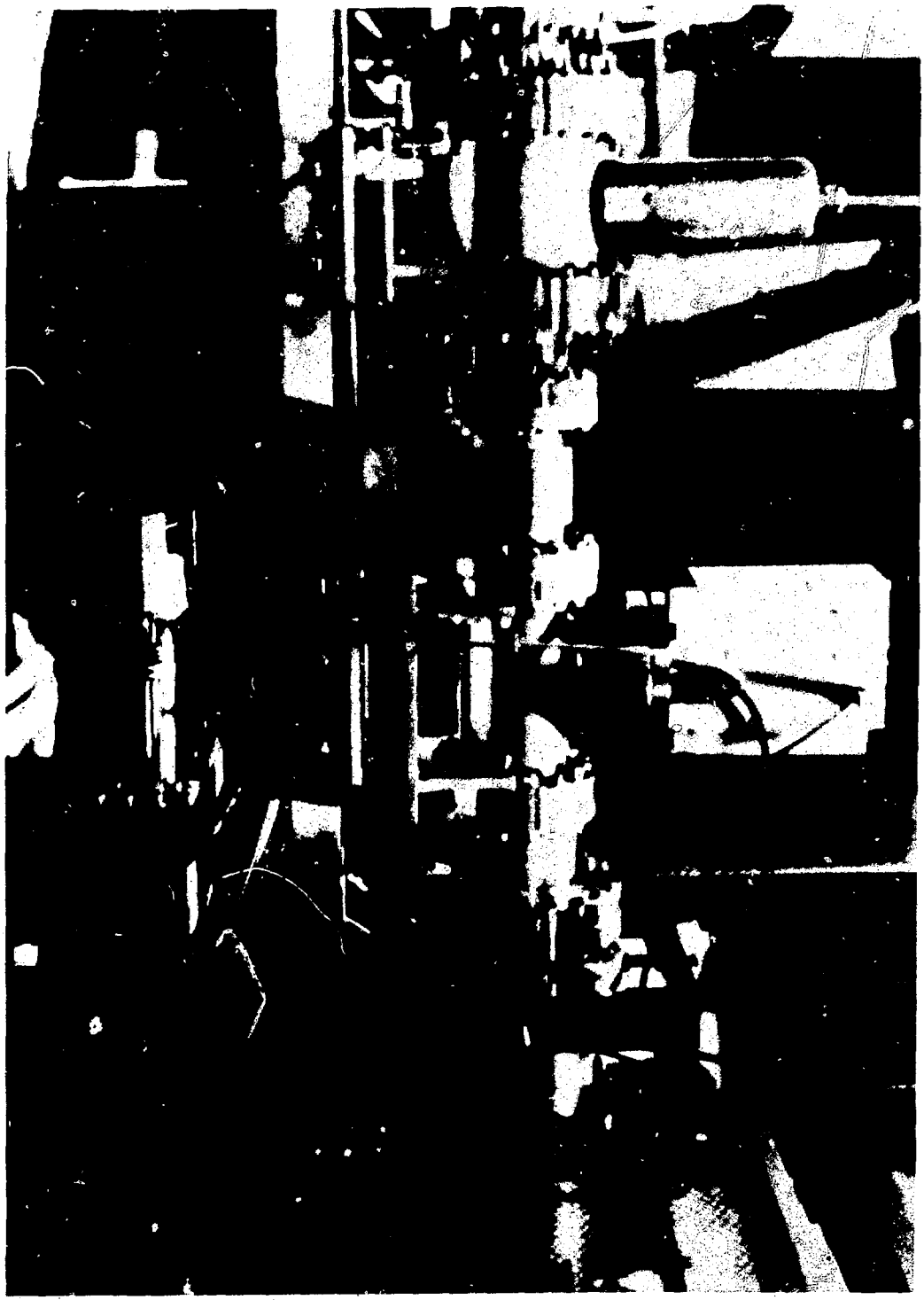


Fig. 8. 150 kv beam line between source and injection magnet.

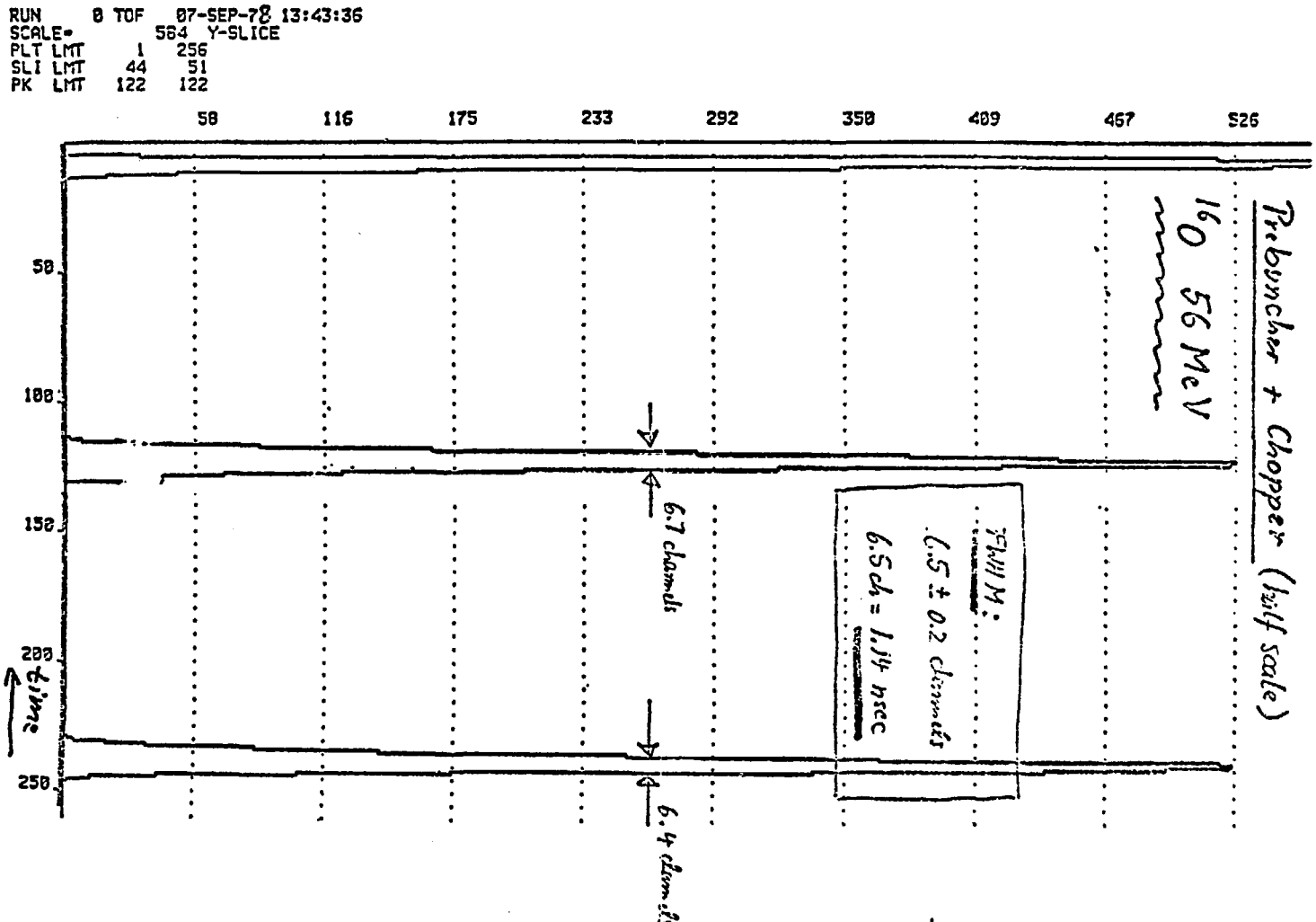


Fig. 9. Time spectrum of bunched tandem beam for 56 MeV O^{6+} with phase detector stabilization using sputtering source.

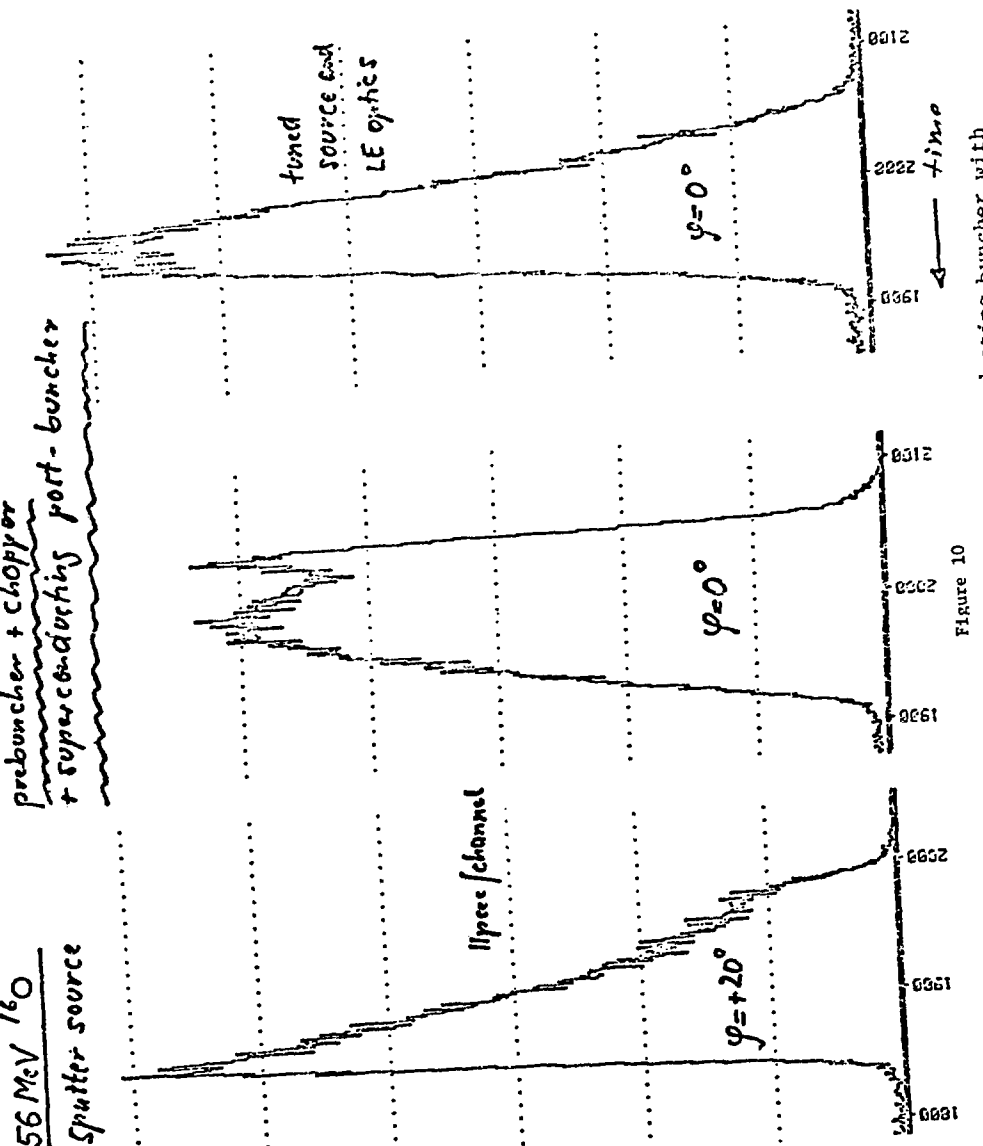


Figure 10

Fig. 10. The beam of fig. 9 rebunched by the superconducting buncher with the spectra taken far away from the time cross over point.

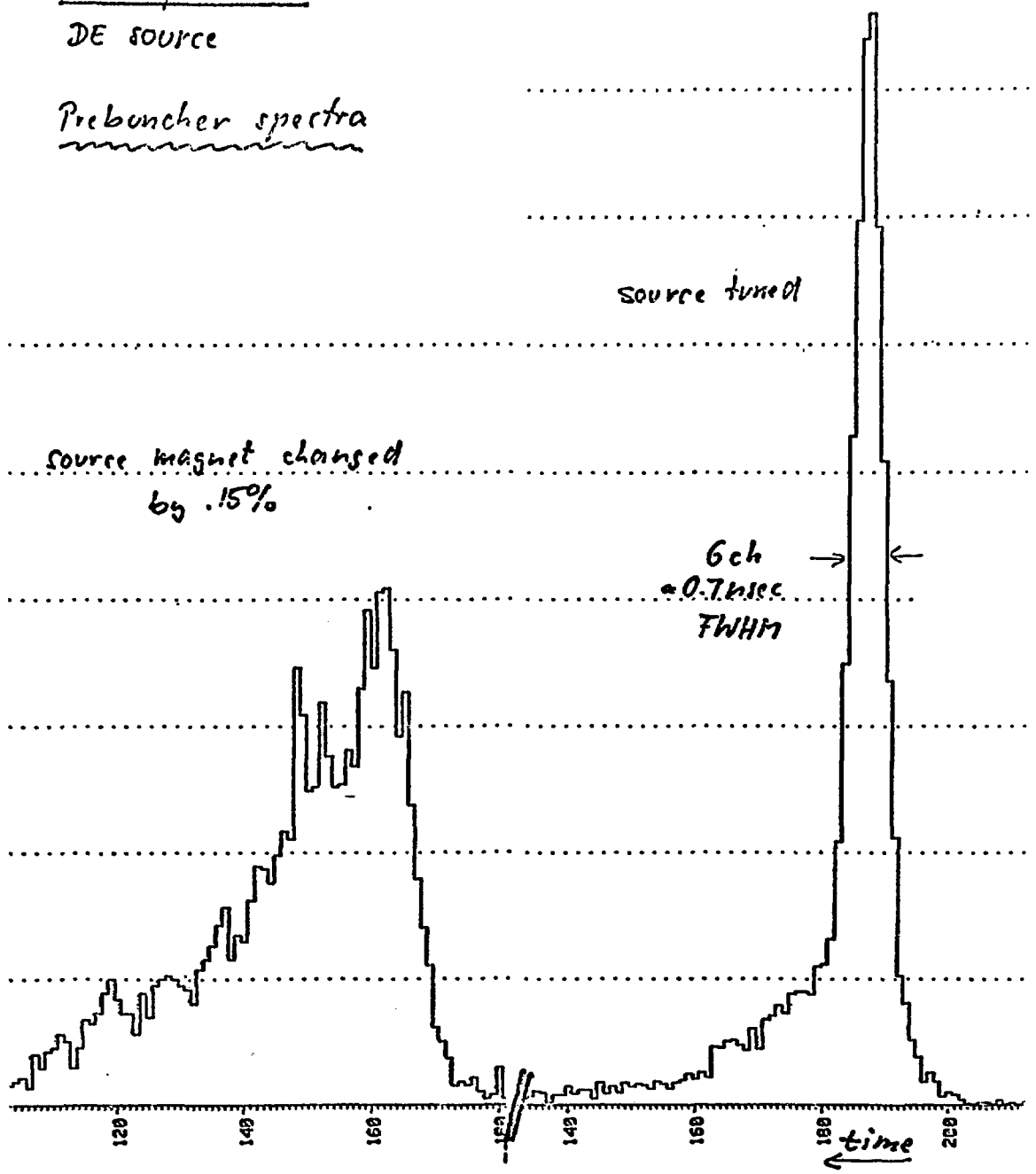


Fig. 11. Bunched beam from direct extraction duoplasmotron.

THE FN TANDEM PROJECT AT THE UNIVERSITY OF IFE

George Oso
University of Ife, Nigeria

I would briefly like to take this opportunity to inform you about the program at the University of Ife. I don't have all the information at hand but I can give you a brief account of the program and tell you how far we have gone with the program.

The people at the University of Ife, especially in the Physics Department, have been thinking about going into experimental nuclear physics for the last five or six years. As you all know, this is a program that involves a huge sum of money and the department itself, in fact the University by itself, could not engage in such a project without government support; and government support was very difficult to obtain. However, in the last two years we have been fortunate enough to get adequate government support and the University is pushing hard to get everything started.

We have made a purchase agreement with High Voltage Engineering and are buying a 9 MV upgraded FN tandem. We are expecting delivery in the next 12 to 15 months. The building is still in the preliminary stage. In fact, I am primarily involved in gathering information which will lead to an acceptable building design and I believe in the next nine months, if we push very hard, we should be ready to accept delivery at the scheduled time.

I don't really know all the uses that are planned for the facility but I do know that initially people intend to use it for purely nuclear physics experiments. Just before I left the University on this training program, I understood that the department is trying to involve other departments. Hitherto, it appears that departments act independently. If a department feels a program is important, it seems that other departments don't want to share in the program and they just say, "Well, that's your own baby, look after it." But I think that everybody realizes that this is a big project, involving a hugh sum of money and that it has to be a collective work. So we are trying to work with all departments, the Chemical Engineering Department, the Electrical Engineering Department, and the solid state physicists on everything. Also, prior to my departure, the University has created two other departments to train would-be researchers and nuclear engineers. I would like to add that the mature part of the program involves the setting up of the accelerator facility. The complete complex will also contain a nuclear reactor facility. The principal

function of the whole center will be the training of engineers, technicians and research personnel. In the future, as we find our feet we shall go into more sophisticated research and most probably into power generation which is the aspect the government is very interested in. Thank you very much.

Discussion:

Chapman: I should like to ask, the FN that you have ordered from HVEC, is that the standard FN or is it the up-graded FN? What electrode material is the tube? Is it aluminum, stainless, titanium, and what charging system are you getting?

Oso: The FN we are obtaining from High Voltage is what I would call the latest machine from High Voltage. It is going to have the stainless steel tube, the laddertron charging chain and some modifications to the standard FN, and I think they are going to guarantee us a terminal voltage of 9 million volts.

Berners: I would also like to know what kind of column resistors this machine will have and what kind of insulating gas you have in the tank?

Oso: The column resistors we are going to use are the high value 600 meg ohm column resistors. I would like to add that initially we asked for proposals on the standard and the up-dated version. As I said, the facility is going to be used by other departments as well and it appears that most other departments are interested in the low energy beams but we thought that if you buy a system and then in the next two or three years you discover you have to upgrade, it becomes more expensive. It is better to go to an upper limit initially, it may mean additional cost but at least you save some money and save some trouble in the future. So we decided to buy the up-dated version and we are going to have the pure SF_6 insulating gas. I wish to just add something; as you all know, we are just beginners and are just entering the field. One of the major problems we are facing is technical personnel. The University has embarked on a crash program to train technicians and engineers, but it will be two or three years before we start getting usable results from these people. So part of my function here is to talk to existing labs to see what kind of assistance we can get in setting up the laboratory as quickly and as efficiently as possible.

THE BUENOS AIRES TANDEM ACCELERATOR FACILITY

A.Filevich, CNEA, Buenos Aires, Argentina and Brookhaven National Lab.

The purpose of this talk is to announce in this meeting that the Argentine Atomic Energy Commission, CNEA, will install in Buenos Aires a large electrostatic accelerator. In order to show the background of this decision, I'll refer briefly to the history of the Nuclear Physics Group at Buenos Aires, which, we can consider was born in 1952* when CNEA purchased, almost simultaneously, from Philips, Holland, a 1.2 MV Cockroft Walton generator and a Synchrocyclotron capable of producing alpha particles of 56 MeV and deuterons of 28 MeV.

During the following years many developments were produced by members of the BA group in the field of beta-ray spectroscopy by using several home-built orange spectrometers. Several new isotopes were first observed using the synchrocyclotron and a Kalutron facility was installed in the Lab.

In 1957 the extraction of the beam from the synchrocyclotron was successfully accomplished (the machine was supplied by Philips with internal irradiation facilities only). In 1965 the IALE Project was born (Isotopes Far Away From the Stability Line) and an on-line isotope separator using the Cockroft Walton as a neutron generator for the fission product-ion source was built and set in operation. In 1966 some ideas about new equipment were first discussed in the group and in 1970 a proposal to convert the synchrocyclotron into an isochronous cyclotron was submitted to the CNEA's authorities. This project failed, but similarly as in the Oak Ridge APACHE Project[1] the ideas continued to grow and develop in the NP Group and in 1975 a feasibility study for a new machine was carried out and submitted in early 1976 with a request for consideration of a large tandem accelerator. The "green light" was obtained in 1976 and the first contacts with the possible suppliers of accelerators were initiated.

*Many years before the "pioneers" had already done a marvelous work and a group of well prepared people was ready to receive and utilize these machines.

In 1976-7 a technical study was performed and the result was that an NEC 20UD machine was selected. The final contract with the Company was signed in December 1977.

The characteristics of the accelerator are listed in Table 1. It should be noted that the machine will be a straight Pelletron, not of the folded type. The reasons for this decision were the higher degree of confidence in favor of such a configuration and the fact that no previous experience with a large folded machine had been previously obtained in other laboratories. It was desired to avoid some of the forseeable difficulties of the folded configuration (Ref. 1), namely

1) magnet in terminal must not fail
2) more complicated optics to isochronize 180° bend
3) increased surface gradients on the terminal cap
4) possibly more difficult diagnosis in case of sparks or small discharges
5) more difficulty in conditioning when only one or few tube sections are desired to be processed at a time

In this matter we feel somewhat conservative but it was decided to pay the price of a poorer charge selection at the terminal, somewhat longer vessel, and a higher tower to avoid the disadvantages listed above. We are concerned with the somewhat long distance from the control console to the ion source, but we believe that a well designed elevator can minimize this problem.

The injector will operate at 300 kV and the preaccelerator will be allocated vertically to save in size of the injector magnet and in horizontal space in the top of the tower.

Gas and foil strippers will be used in the terminal and a second foil stripper will be placed in the high energy tube.

An idea of the relative dimensions of the 20UD in comparison to the beautiful machine we had the opportunity to see under construction in Oak Ridge and with the JAERI 20 MV folded tandem, is given in Table 2.

Figure 1 shows the operating characteristics of the BA machine in comparison with the ORNL folded Tandem and the BNL 3-stage system. In this plot it was assumed that both machines in construction by NEC will attain their specified voltages.

The human resources of the CNEA's Physics Department consists of 37 investigators (including 14 in the Solid State Group), supported by 10 engineers, 40 technicians, and 10 administrators. It should be noted that a program for training young people was initiated at the time of the decision of purchasing the accelerator. By the end of this year 13 juniors (Master degrees) will be already incorporated to the Group; their training includes a good deal of theory but their experimental formation is being strongly emphasized.

The tasks associated with the Project are being carried out by several groups, led by the persons listed in Table 3.

Figure 2 shows briefly the tight schedule to complete the project. By the time of this meeting the companies which will be responsible for the building, the construction of the pressure vessel, and the gas-handling system are being selected.

The artist's view of the tower and experimental rooms shown in Fig. 3 is only tentative and preliminary. Probably a 180^{o} geometry for the experimental area, similar to that of Daresbury, will be chosen, early ideas about a 360^{o} configuration were abandoned because of technical reasons.

Ref. 1 P. H. Stelson, The New Heavy Ion Accelerator at Oak Ridge.

Discussion:

Kutschera: What were the reasons to choose the straight tandem rather than a folded one?

Filevich: Well, in the beginning we wanted to be as conservative as possible in the machine. The folded tandem is an extremely nice design but is not still proved and tested. We felt that the straight geometry would be easier to diagnose in case of failure and would have less problems than a bigger magnet in the terminal can produce.

Kutschera: But you are still thinking of having a charge selection system at the terminal?

Filevich: We have a charge selection system at the terminal. It would be of the offset quadrupole type. One of the figures which was listed showed the resolution of this charge selector.

Table 1. Characteristics of the Accelerator

Terminal Voltage	5–20 MV
Voltage Stability	± 2 kV
Proton Energy	10–40 MeV
Charging Current	400 μA
Beam intensity unstability	20% of mean value
Guaranteed currents	
protons	3–20 μA
alphas	1 μA
^{32}S	150 pnA
I	50 pnA
Au	50 pnA

Features

* Electrostatic Charge Selector in terminal
  efficiency (Au beam): 0.8 pnA @ $Q = \bar{Q} \pm 1$
* Gas and Foil strippers in terminal
* Second stripper in high energy acceleration tube
* 300 kV injector with vertical preaccelerator tube
* Light link for monitoring of terminal parameters

Table 2. Comparison of design parameters of 20U folded and straight versions.

	Vessel		Column	Gas		Tower
	Height (m)	Diam. (m)	Diam. (m)	Press. (psig)	Weight (tons)	Height (m)
JAERI (FOLDED) 20 MV	23.8	8.23	3.35	80	54.0	47.2
CNEA (STRAIGHT) 20 MV	35.05	7.62	2.13	80	66.7	62.0
ORNL (FOLDED) 25 MV	30.6	10.0	5.0	80	101.0	46.5

Table 3. Organization

PROJECT MANAGER

Emma Perez Ferreyra

ACCELERATOR

M. A. J. Mariscotti
A. Ceballos

BUILDING

E. Ventura

RESEARCH LINES

G. G. Bermudez
J. Rossi

CONTROL SYSTEM

D. Camin

GAS SYSTEM

N. Fazzini

DETECTORS

G. Marti
A. Proto

ION OPTICS

A. Ceballos
A. Ferrero

TRAINING

Physicists: E. Maqueda
Engineers, Technicians:
N. Fazzini

COMPUTER & DATA ACQUISITION

E. Achterberg

ION SOURCES

TARGETS, STRIPPERS, CHEMISTRY

RADIATION AND GENERAL SAFETY

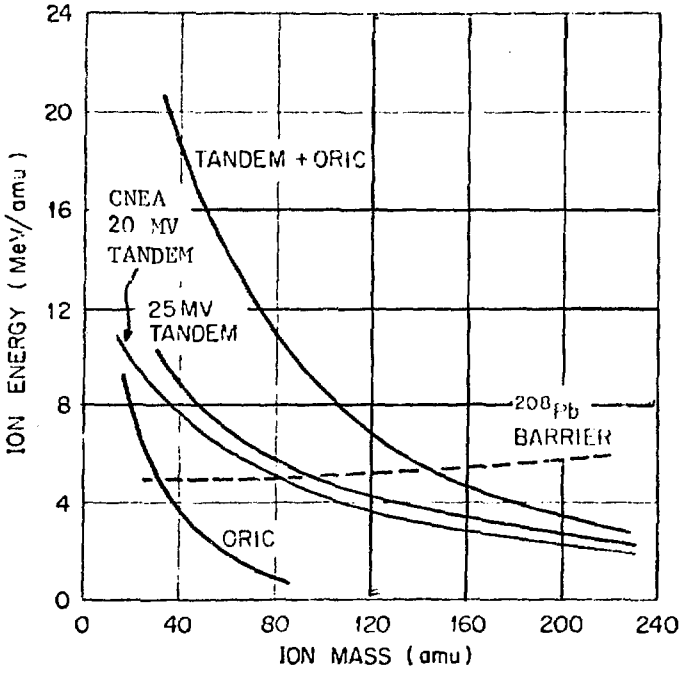

24
20
16
12
8
4
0
ION ENERGY (MeV/amu)
0
40
80
120
160
200
240
ION MASS (amu)
TANDEM + ORIC
CNEA
20 MV
TANDEM
25 MV
TANDEM
208Pb
BARRIER
ORIC

Fig. 1

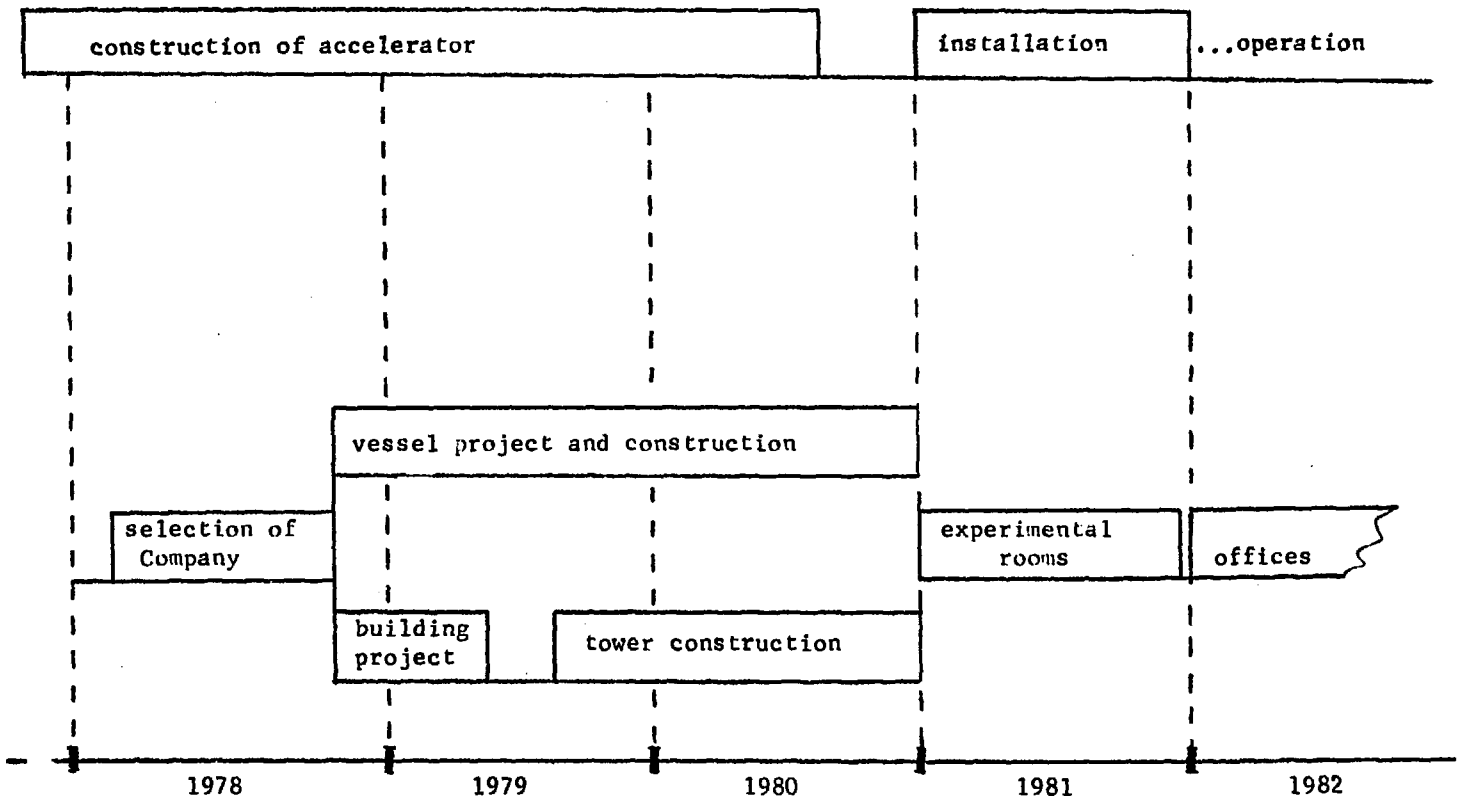

construction of accelerator
installation
...operation
vessel project and construction
selection of Company
experimental rooms
offices
building project
tower construction
1978
1979
1980
1981
1982

Fig. 2

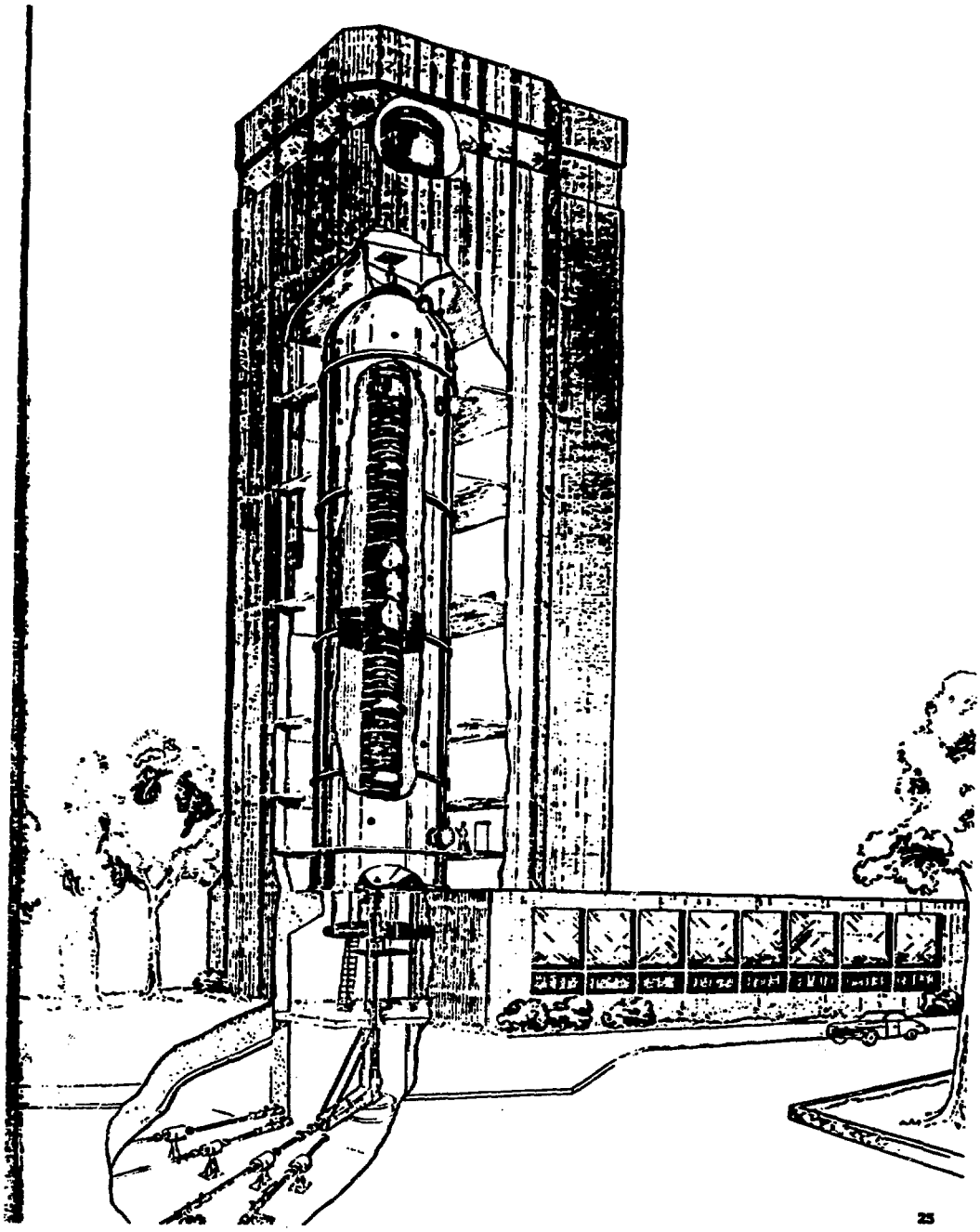


Fig. 3

OPERATION OF THE OXFORD FOLDED TANDEM*

by

P.J.S. Bromley-Barratt, T.R. Brock, G. Doucas, T.J.L. Greenway,

A. Henwood, A.R. Holmes, H.R.McK. Hyder, G.M. Parker, and J. Takacs

Nuclear Physics Laboratory

University of Oxford

Oxford OX1 3RH England

The 10 MV vertical single-ended Van de Graaff in the Nuclear Physics Laboratory at Oxford University has been converted to a folded tandem and is now in operation. Preparations for the conversion began four years ago and the accelerator was shut down for the changeover in July 1977. The first beam of $^{4}He^{2+}$ ions was transmitted through the machine on September 7th 1978. Since then the test programme has been devoted to measuring the properties of the transmitted beam and extending the operating voltage and beam current. The general arrangement is illustrated in figure 1.

The commissioning tests have been carried out with a beam of $^{4}He^{-}$ ions from the charge exchange source developed in the laboratory (figure 2). This source uses a General Ionex model 820 duoplasmatron which produces an intense, divergent beam of positive helium ions. This beam is refocussed through a special charge exchange canal which incorporates a wick to improve the recirculation of lithium. Close temperature control of the oven and canal reduces lithium losses while maintaining the optimum vapour pressures. With lithium vapour, beams of 12 μA of $^{4}He^{-}$ have been measured having an emittance of <8 (π) mm mrad $\sqrt{MeV}$. Rather larger beams (up to 19μA) can be obtained if sodium is used. Source life has been satisfactory as is evidenced by an operation period of over 250 hours without reloading lithium or changing the filament.

*Presented by S. J. Skorka.

The negative ion beam from the source is pre-accelerated by 20-100 kV, analysed in a 12 inch radius 90° inflector magnet and then focussed onto the tube object by a gridded einzel lens. The accelerator tube has a gridded entrance, followed by a short magnetically suppressed section. In this region electrons are trapped on the titanium electrodes by the action of a small dipole magnetic field of about 20 gauss whose direction rotates about the beam axis. The remainder of the accelerator tube consists of inclined titanium electrodes in spiral configuration.

On entering the terminal, the beam passes through a beam sensor which detects the intensity and position of the beam, then through a foil stripper tube which is surrounded by a 500 ℓ/s ion pump and then into the 180°, 19 inch radius bending magnet. Slits near the mid point of this magnet select the required momentum and charge state and the emerging beam is then focussed by a magnetic quadrupole doublet and measured by a second beam sensor and by a Faraday cup. A second ion pump is connected to the positive beam tube and to the magnet box. There is a differential pumping canal between this pump and the gas stripper.

The positive accelerator tube is similar to the negative one, having a magnetic section at the top to permit the injection of low energy positive ions (particularly helium and neon) from an R.F. ion source in the terminal. At the intermediate point in the column, which is 8 feet below the terminal and 12 feet above the ground plate, there is a second foil stripper to permit double stripping for high energy heavy ions. This is the point at which the intershield is attached.

The equipment in the terminal is controlled and monitored by a fast serial data link using infra red transmitters and receivers. The bit rate is 1 MHz and 64 10-bit words of data are available in each direction.

Analogue signals occupy 32 words in the down direction and digital

commands use 6 words in the up direction. The 180° magnet field demand is transmitted upwards as a 16 bit digital signal requiring two words. The overall cyclic time is 2 msec.

A similar but slower system employing fibre optic links is employed to control and monitor the negative ion sources. This system has now operated without faults for over one year.

In addition to the two terminal ion pumps, the accelerator tubes are pumped by two 500 ℓ/s ion pumps inside the tank at the base of the column. A single 450 ℓ/s Leybold turbo-molecular pump is used to pump down the entire system and to maintain high vacuum in the low energy and high energy beam tubes. The operating pressure is between 1.5 and 5 x 10^{-7} Torr at the ends of the accelerator tubes.

After acceleration the beam is focussed by a magnetic quadrupole doublet located inside the tank onto the object point of the double focussing 6 foot radius analysing magnet. This magnet can be rotated about a vertical axis and is used to direct beam into one of the five target lines located in the basement target area. Initially the experimental facilities will include a cryogenically pumped gas target, a large spherical scattering chamber and a general purpose angular distribution table. Later on the beam will be transported past the EN tandem into a large magnetic spectrometer. Eventually it is hoped to replace the EN tandem by a superconducting booster.

Beam tests to date have shown that the injection system transmits the negative beam clearly and with minimal loss into the terminal. The 180° magnet has a measured momentum resolution of 0.24% (FWHM) with its analysing slits set rather wide. The high energy beam transport is straightforward and the stabilization system partly inherited from the

old accelerator produces a very constant and controllable beam at the image slits of the analyser magnet.

Acknowledgements

The project, which has been accomplished at a cost of £100,000, is supported by the Science Research Council.

It is a pleasure to acknowledge the active involvement of Professor K.W. Allen, Dr. P.S. Fisher and Dr. M.A. Grace. We have benefited from discussions with many colleagues, especially Professor W.D. Allen, Dr. T.W. Aitken and Dr. J.M. Lowe. The success of the project has depended on the enthusiasm and efforts of the technical staff of the Laboratory, especially the workshop staff who have built the new equipment.

February, 1979

Discussion:

Wegner: You mentioned problems with voltage, what kind of problems are they having with voltage holding?

Skorka: They started the tests and wanted to have no sparks so that nothing goes bad because they have a beam through the system, that's why they had relatively low voltage and Dick Hyder didn't even mention how low, they just started to see whether everything in principle is okay, and there seems to be a little optical problem in the 180° magnet, that's why the transmission was relatively low, they had to open up the slit in the center of the magnet relatively wide to get maximum transmission. There's one trick, the magnetic quadrapole at the high energy side is energized by the same current as the magnet. So they cannot adjust it independently except for a little adjustment of ± 2% and it seems that for some reason this range is not enough. They have to change it, but its not a principal problem.

Rathmell: It appears that the 180° magnet does not have any shielding on the coils, do you know if it was installed that way?

Skorka: One saw the coils, that's right.

Rathmell: You can see the coils, there appears to be no shielding around them.

Skorka: No, I'm sorry, I don't know. They used double shielding for all the electronics, so they are aware of these problems, but I agree, I didn't notice, I should have asked him.

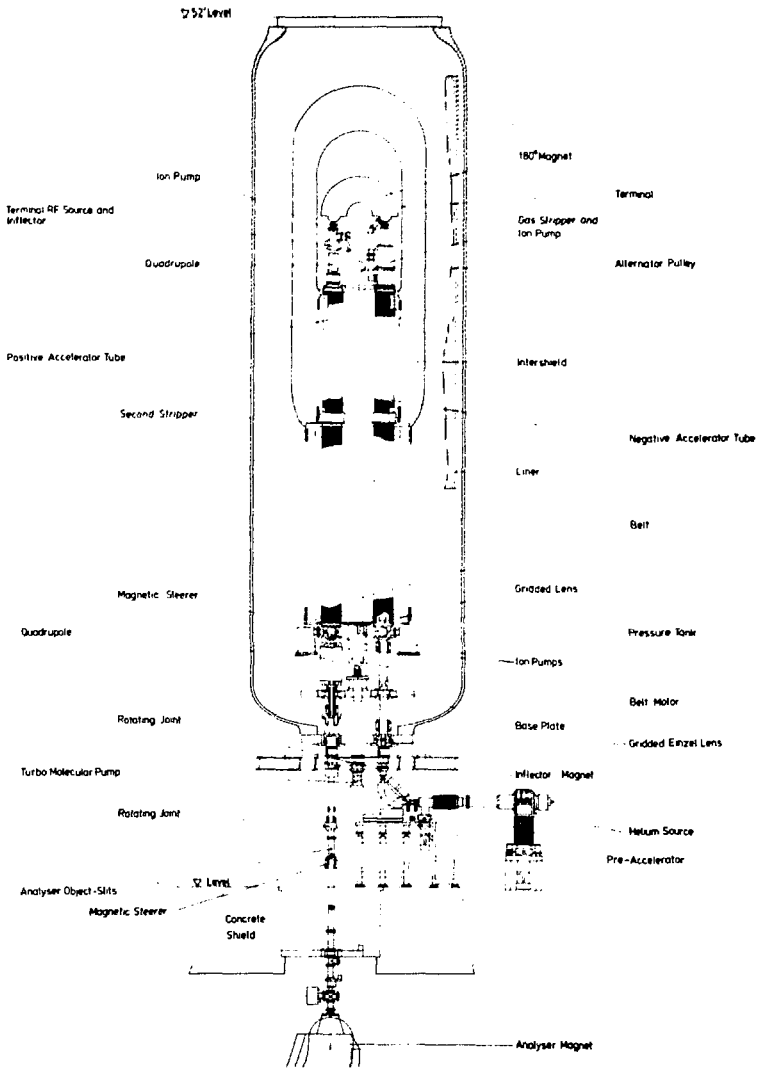


Figure 1. N-S section of the Oxford Folded Tandem.

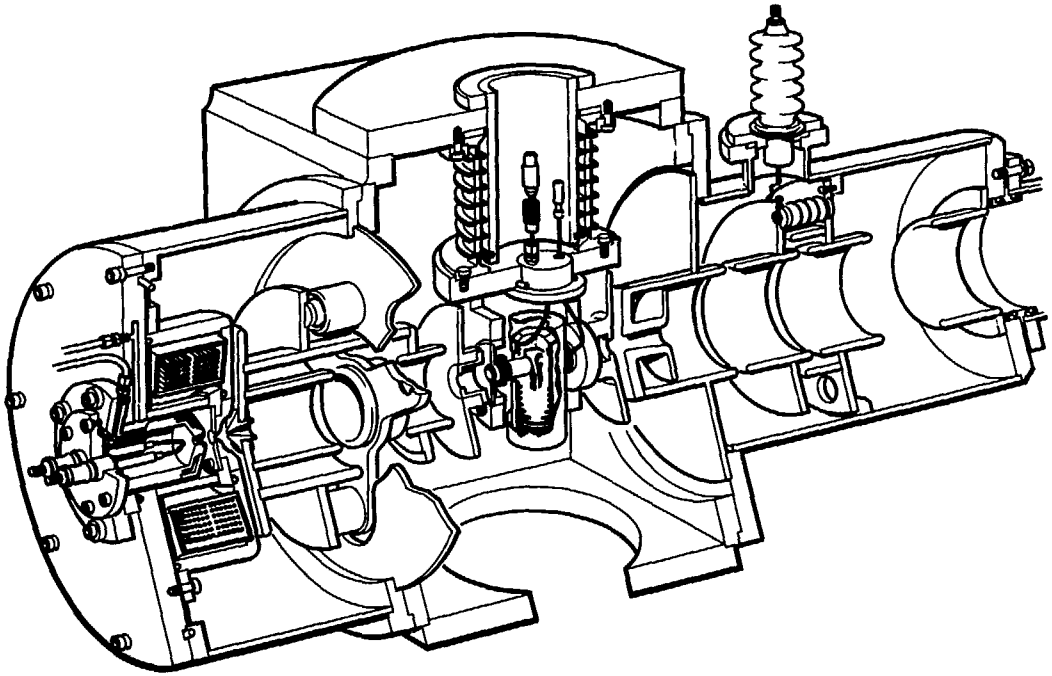


Figure 2. Helium charge - exchange source.

Session VIII, Chairman, J. D. Larson
Editors, W. T. Milner and R. O. Sayer

Operation of a Large Gridded Foil Stripper in the Terminal of a Tandem Accelerator

K. Chapman
Department of Physics, Florida State University
Tallahassee, Florida 32306

Abstract: A prototype mechanism has been built and installed in the terminal of the F.S.U. S.F.N. tandem accelerator to use a large gridded foil stripper and scan this across the beam. Results are presented from the first two foils used and some observations made about foil preparation and a possible alternative foil mounting grid.

I. INTRODUCTION

The use of a large gridded foil to avoid frequent tank entries to replace stripper foils was proposed earlier.[1,2] The necessary prototype mechanism and stripper box has now been built and installed in the terminal of the F.S.U. tandem. To simplify the initial test equipment, this was made to use a single large foil and scan this across the beam in a raster of approximately 60 steps. The final design will have facilities for changing foils and will have a capacity of some 50 foils. If found desirable this will also have the ability to move the foil slowly but continuously across the beam for use with such heavy ion beams and intensities as cause a very short foil life. The prototype box has been arranged to take a gas stripper before the foil for use with molecular beams where "coulomb-explosion" effects may be serious but this was not installed initially so as not to confuse the transmission results for the new stripper. This mechanism is shown in Fig. 1 and the box is shown installed in the tandem terminal in Fig. 2 and 3.

II. FOIL PREPARATION

Large gridded foils 7.5 cm square and 5 $\mu gm/cm^2$ can now be produced reliably. The carbon is initially deposited on a chromium plated stainless steel "ferrotype" plate using a suitable release agent. This foil is then floated off on distilled water.

It is necessary to immerse the backing plate very slowly to allow the water to creep up between the foil and the backing ahead of the water surface. This is most easily achieved by maintaining the backing stationary and causing the water level to rise by controlled flow into container. This, if care is taken to avoid turbulance, avoids problems of vibration or erratic mechanical motion. When the foil is floating on the water it may be lightly sprayed with formvar type material to considerably increase its strength. The foil is then picked up on a mounted grid very lightly sprayed with vacuum grease or epoxy resin to ensure a bond between the grid and the foil. It is most important that the face of the grid and mount, on which the foil is to be picked up, is completely flat. Any recessing of the grid in the mount causes folds and tears in the foil which once started will frequently spread completely across the foil.

Baking the foil and mount in vacuum or a neutral atmosphere to approximately 900^{o} C before use[3] would appear to offer several advantages. The assembly is thereby cleaned and the trace of vacuum grease used is reduced to carbon which is more acceptable in the accelerator vacuum but the main advantage would be that the deposited amorphous carbon will start to crystalize to graphite and the resultant shrinkage will occur before the foil is placed in the beam. We have baked small test foils but await installation of a suitable oven before baking large foils is possible.

III. RESULTS

The equipment was only installed for a few weeks but some provisional results of interest were obtained. Transmission through the accelerator was found to be similar to that of the small foils used previously. In fact the first foil, which was known to be a little thick (≈ 8 $\mu g/cm^2$), showed the transmission effects we have come to expect from a small foil of this thickness. That is good transmission through the accelerator but below average transmission analyzed. The second foil which was of correct thickness (≈ 5 $\mu g/cm^2$) gave good analyzed transmissions for heavy ions. The first foil was a very good testimony to the strength of these foils. It was made several months ago during our early foil mounting tests and had been around the lab ever since, sometimes under vacuum, often out on the bench in a plastic box. It was used as it was readily available when installation of the mechanism was completed and was still as sound as the day it was made.

After two weeks of running with heavy ion beams, principally oxygen and boron, it was removed so that it could be examined and a thinner foil substituted. This first foil is shown in Fig. 4. The separate beam positions can be plainly seen. It shows that with modification of the mechanism more foil positions could be used by reducing the spacing as good foil remains between the used positions. This could also be clearly seen in

the accelerator as when a rescan of the foil was made, the beam rose as these parts passed through the beam between the stationary positions. Some indication of lifetimes with this first foil is given in Table I. Here lifetime is defined to be the time for an initial target current of 0.5 μA to fall to 0.3 μA. At this point the foil was changed to maintain the experimental counting rates but useful beam could still be obtained from these used positions. It is also generally found that the beam from a new foil position fell rather fast at first and then flattened out so that a small reduction in acceptable target current gives a disproportionately large increase in foil "lifetime".

This foil was rescanned several times and was used for two weeks in the accelerator. A thinner foil may be expected to give a longer life before thickening renders the transmitted beam unacceptably low. Nevertheless it may be pointed out that 50 foils having a lifetime of only 2 weeks will last 2 years and with a lifetime of one month, which one may reasonably expect, would last 4 years. Not too many accelerators will run for even 2 years without a tank opening being required for some repair or modification.

The scanning mechanism has now been removed from the terminal and the N.E.C. stripper replaced. This was done to allow modifications to the mechanical scan apparatus as the initial version has been made as simply and quickly as possible and had, for example, aluminum bearing surfaces. Now following these initial tests it is being correctly engineered to give long reliable service.

This opportunity will also be taken to reconsider the foil mounting itself. It is now generally accepted that breakage is due to the shrinkage that occurs in a foil when a beam is incident on it. This shrinkage appears to be due to the change of the amorphous carbon, as laid down by evaporation, into crystalline graphite which being a more orderly structure is more compact. The resultant shrinkage continues until the tensile stresses introduced in the foil surrounding the beam spot exceeds the tensile strength of the carbon and rupture occurs. Supporting the foil on a small mesh grid means that there is no carbon to yield around the beam spot and the lifetime of any one spot on the foil is hereby reduced. This is consistent with the experimental result that although by using many foil positions, on a large foil a very long lifetime can be achieved, the lifetime of any one position is less than that of an unsupported foil.

Successful results are reported by "Daresbury" using a foil mounted slack.[4] This is achieved by mounting the foil on a ring that is then reduced in diameter. Results are also reported from Germany for a very few foils that had greatly extended lifetimes; these were produced by cracking carbon onto the slide and perhaps allows the deposition in a more orderly array though crystallographic inspection does not appear to reveal this.

Our new mounting techniques will attempt to take advantage of these developments. We are currently experimenting with a grid structure as shown in Fig. 5 on which a foil may be mounted

as shown in Fig. 6. This should allow shrinkage to occur in the foil with the resultant forces being partially relieved by distortion of the non-ridged frame. If this technique is successful it is an obvious development to produce a large foil with many such hexagons and arrange the mechanism to step from aperture to aperture.

IV. CONCLUSIONS

The tests though preliminary show that such large gridded foils may be used in the terminal of a tandem accelerator successfully and offer promise of a technique by which tank opening may, once again, be reduced to those necessary to make other repairs or modifications.

ACKNOWLEDGMENTS

Thanks are due to G. Velisek and the N.R.B. machine shop for their patience and understanding with the many changes that have had to be made and to R. Leonard and his assistants for the considerable effort necessary to develop and perfect the foil production and mounting techniques.

References

[1]A new method of mounting stripper foils, K. R. Chapman, Revue de Physique Appliquée, 12, Oct 1977 p. 1547-1550.

[2]Mounting of large stripper foils for use in the terminal of an electrostatic accelerator, K. R. Chapman, Nucl. Inst. & Meth. 148 (1978) 209-212.

[3]Private communication, H. V. Buttlar, Institut für Experimentalphysik (III) Ruhr - Universität D(RFA) Bochum.

[4]The enhancement of the lifetime of carbon stripper foils under heavy-ion bombardment, B. H. Armitage et al., Nucl. Instr. & Meth. 155 (1978) 565-567.

Discussion:

Wegner: Presumably you destroy these flimsy little frames -- it doesn't look as if they are reusable.

Chapman: No, the initial tests were done on the gridded normal foil frames in the tandem terminal, and the beam does not destroy the grid under normal beam intensities and the entire grid is reusable.

Wegner: I'm just thinking of handling say in the NEC type frames. What is the cost of a frame?

Chapman: The initial cost is fairly high because the negatives have to be made; although I'm not absolutely certain, my impression is that they were over $40 apiece, somewhere in that range.

Skorka: You mentioned pre-baking of foils. There was a paper presented in Munich in a target conference by Sanders and von Boheholm (?) and they found that this crystallization takes place at 1300°C, and they recommend a pre-baking at 2000-3000°C. It's very difficult to handle these foils because they are very fragile.

Chapman: One of my colleagues went to a conference recently in Germany, maybe the one of which you spoke, and there was a report there of a gentleman who managed to make some stripper foils by cracking carbon. Only two of four had survived, but these two showed at least an order of magnitude extension in life over the other foils. Presumably this implies that perhaps this carbon was put down at least partially in the crystalline form. I think if you get them in that form they are very fragile to handle. But, in this instance, if they will stand baking they will stand the shrinkage. Then we may not feel so bad about putting them into the machine as amorphous carbon and letting them shrink in the frame in the machine.

Adams: Two questions. One, what is the thickness of the target frame and the grid and, two, do you glue the carbon foil to the hexagon grid?

Chapman: I'll answer in reverse order. An attempt is made to bond the foil to the grid. We have tried a number of ways. One of the early ways was to do it with a trace of vacuum grease which is perhaps not entirely desirable particularly in an ultra high vacuum machine. It's true of course that if you pre-bake the foils, much of this grease will change to carbon and ceases to be so undesirable. But we have, based on a suggestion you made in Tallahassee some while ago, bonded some of the foils using a sprayed epoxy resin, and this seems to work

quite nicely too. But the foil you saw in this picture was just a normal foil mounted on formvar without any other bonding to the grid wires. The material of the grid structure you make of anything you want. My memory is that one is made out of about 0.002" tungsten sheet, and the wire is about the same thickness as the sheet.

Broadhurst: This is just an observation. There is a preprint out from Daresbury on foils prepared by ethylene cracking which appear to have an order of magnitude longer lifetime.

Larson: That work will be discussed later in the session.

Broadhurst: What would be the feasibility of electron bombardment heating of the foil before you move it into position? The same way as ion gauges degas by having a hot filament to bombard it and heat it.

Chapman: What would you aim to do by doing that?

Broadhurst: Crystallize it.

Chapman: That may be possible, but if its going to crystallize in the beam anyway, this may not gain you too much. The initial object of our baking test was purely to see how well the foils survive under the crystallization process. In the oven the whole foil will crystallize rather than the beam spot, and that may or may not be desirable. If it is desirable, then some technique such as you suggest may be beneficial.

TABLE I.

GRIDDED FOIL LIFETIMES

L.E. FARADAY CUP CURRENT ^{16}O -- 4 μ A

H.E. FARADAY CUP CURRENT +5 CHARGE STATE -- 8 μ A

8 MV ON TERMINAL

FOIL POSITION NO.	INITIAL TARGET CURRENT (nA)	FINAL TARGET CURRENT (nA)	TIME IN USE
12	450	300	3 HR 10 MINS
13	550	300	2 HR 50 MINS
14	500	300	4 HR
15	500	250	2 HR
16	'500'	300	5 HR
17	500	300	3 HR 20 MINS
19	440	300	5 HR 50 MINS
20	500	300	4 HR 20 MINS
21	580	200	3 HR 45 MINS
22	500	'300'	3 HR 45 MINS

AVERAGE LIFE - DEFINED AS TIME FOR INITIAL TARGET CURRENT OF $\simeq$ 500 nA TO FALL TO $\simeq$ 300 nA.

AVERAGE LIFE = 3 HR 48 MINS PER FOIL POSITION.

TABLE II.

GRIDDED FOIL #2 LIFETIMES

L. E. Faraday Cup Current ^{12}C -- 5μA

H. E. Faraday Cup Current +4 charge state -- 5μA

4 to 5 MV on Terminal

Foil Position No.	Initial Target Current (mA)	Final Target Current (mA)	Time in use	
1	150	150	2 hr	foil still good changed with no increase in beam
2	150	150	2 hr 30 min	" "
3	150	150	1	" "
4	150	140	7 hr 20 min	
5	240	120	7 hr 10 min	
6	240	140	8 hr	
7	240	150	8 hr	
8	210	150	8 hr	
15	300	180	8 hr 22 min	
16	300	100	11 min	foil position broke?
17	300	150	3 hr 27 min	
18	270	90	1 hr 15 min	
19	270	150	7 hr 50 min	
20	270	---	4 hr 35 min	used till end of run

Average Life - defined as time for initial target current to fall to = 0.5 of initial value.

Foils 4 through 8, 15 and 17 through 20 = 6 hr 24 min per foil position.

NOTE: 1 through 3 not included as no measurable decrease in target current during short period used in beam - foil effectively unused.

Fig. 1 - The mechanism for translation of the foil across the beam.

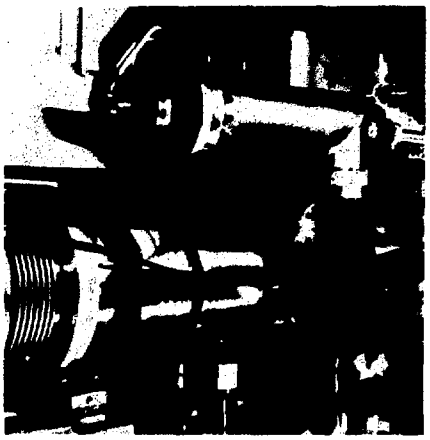


Fig. 2 - Prototype stripper box installed in the tandem terminal.

Fig. 3 - Installation in tandem terminal showing motor drives for horizontal and vertical motion.

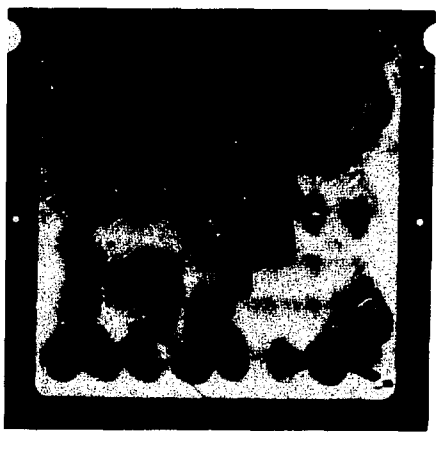


Fig. 4 - Foil #1 after removal from the terminal. Photographed by transmitted light.

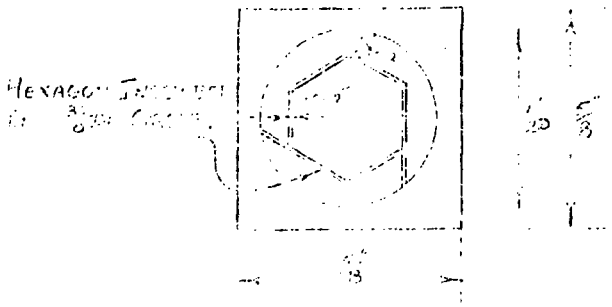


Fig. 5 - Non-rigid foil support grid.

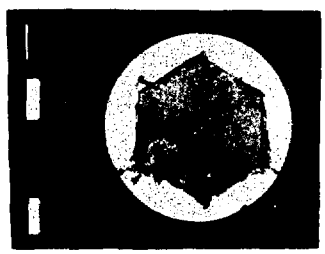


Fig. 6 - Grid with carbon foil mounted.

SUMMARY OF CARBON FOIL LIFETIME RESULTS FROM A HARWELL-DARESBURY COLLABORATIVE INVESTIGATION

J. D. Larson
Independent Consultant

The speaker presented a brief review of work on lifetimes of carbon foils based on preprints of the following articles:

1. "The Enhancement of the Lifetime of Carbon Stripper Foils Under Heavy Ion Bombardment", B. H. Armitage, J. D. H. Hughes, D. S. Whitmell, N. R. S. Tait, and D. L. Tolfree, Nucl. Inst. Meth. 155, 565 (1978).

2. "A New Long-Lived Carbon Stripper Foil", D. S. Whitmell, B. H. Armitage, D. W. L. Tolfree, and N. R. S. Tait, submitted to Nucl. Inst. Meth.

Discussion:

Broadhurst: This is sort of a herring thrown out. Daresbury foils showed no graphitic diffraction patterns, but they would have been made with an absolute saturation of gas atoms combined in the lattice of anything you were making. This may inhibit shrinkage, but it should be duplicatable by vacuum deposition in the poorest possible vacuum.

Larson: Of course this is entirely speculative as to whether the material is prevented from shrinking or whether it is preshrunk.

Berners: Does anyone know how a carbon arc deposited foil of the same thickness as the cracking deposited foil would have performed under those same test conditions? I'm worried about the factor of 10 difference in thickness.

Jones: There is a recent study from Britain which indicates that lifetime is independent of thickness over reasonable values of thickness. I have the reference in my office.

Wegner: The bottom line is making foils that you can do research with. At Brookhaven we use 2 $\mu g/cm^2$ foils in all our first stripper positions, and they are roughly twice as thick as you'd like. But the 1 $\mu g/cm^2$ foils seemed to be very difficult to handle. The 2 $\mu g/cm^2$ foils are appreciably better than 5 $\mu g/cm^2$ foils. Now if these other techniques simply don't

make 2 $\mu g/cm^2$ foils that you can handle, long lifetimes with foils that don't perform very well aren't very useful.

Larson: I agree, but I think we may be premature in trying to judge this new technology. I emphasized the thickness of the foils to make sure it was understood that these foils were not necessarily the same foils one would be using in the terminal, but I have no way of judging whether that is detrimental to the procedure.

Chapman: I wanted to comment that it was my impression that the cracked carbon foils for which data were presented at the German meeting were of the same thickness as the standard foils and they did get the marked increase in life although they were dealing in both cases with about 5 $\mu g/cm^2$ foils.

Larson: I think there are only two foils yet created as of the time of this writing, and they are definitely described as being 15 $\mu g/cm^2$ foils.

Chapman: They are the Daresbury foils. I'm talking in fact about the foils that were produced in Germany and described at the German conference.

Larson: It struck me that the argon ion bombardment might be something that could be employed after the foil was laid down as a conditioning while the foil is still on the substrate.

Alton: I wondered if anybody has attempted to exploit the fact that antimony has a negative coefficient of expansion for mounting foils so that, in the presence of heat, the frame shrinks.

Larson: For their slack foils they use 10% reduction in diameter. I suspect that your frame wouldn't move that much.

Broadhurst: Probably one of the simpler ways would be to mount it on a piece of heat shrink material and warm it up afterwards if one wants to slacken the foil.

Larson: There is no description in the Daresbury material of how they go about slackening the foils, although I think everyone can think of ways to do it. Also, Ken Chapman's flimsy frame may be still an ideal way to mount any foil.

Sayer: What was the bombarding energy for the Daresbury-Harwell foils?

Larson: I think it was about 4 MeV.

ROCKET PLOTS OF CHARGE STATE DISTRIBUTIONS FOR TANDEM ELECTROSTATIC ACCELERATORS

R. O. Sayer
Computer Sciences Division *
Oak Ridge National Laboratory

Predictions of the ion intensity performance of a tandem electrostatic accelerator require estimates of the charge state distributions following stripping in the terminal. These distributions are usually generated from semi-empirical formulas since the experimental stripping data are rather sparse.

Two types of questions about accelerator performance are of interest here. The physicist wants to know how much beam of a specified final energy E can be obtained on target. On the other hand, the operator wants to know the combination of charge state q, terminal voltage V_T, and stripper material that maximizes the intensity for a given energy.

One approach is to compute a table of intensities I_q for various charge states and for a set of discrete V_T values. Interpolation on V_T is difficult, however, and comparison between gas and foil strippers is tedious. Another approach is to write a program to compute q, V_T, and I_q for a specified E. This is convenient for one energy but may be somewhat cumbersome for a range of energies.

Rocket plots, a new type of display for charge state intensities for tandem accelerators, offer several advantages over previous formats. A typical rocket plot is illustrated in Fig. 1. A plot of E as a function of V_T is made for a

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

series of charge states of I^{127} produced by a carbon foil terminal stripper. Of course the result is a series of lines obeying the relation $E = (q+1)\ V_T$. Predicted intensities, indicated by the width of the lines and by numbers for $I_q \geq 10\%$, are computed as a percentage of the injected beam intensity. Solid lines are used for $1.0 \leq I_q < 10.0\%$, and dashed lines are used for $0.1 \leq I_q < 1.0\%$. The q values are drawn in the right margin. For example, for $V_T = 20$ MV the I_q values for q = 16, 18, and 20 are about 12%, 3%, and 0.2%, respectively. Thus, the respective currents for a 1 μa injected beam would be about 120 pna, 30 pna, and 2 pna. Values of $I_q < 0.1\%$ are not plotted.

A vertical slice, therefore, gives the usual charge state distribution in q for one V_T value. However, a horizontal cut for one E value yields a distribution in q, V_T space that is extremely valuable to the operator. For example, the maximum intensity for 400 MeV I^{127} is obtained with $V_T = 25$ MV, q = 15.

It should be emphasized that the semi-empirical formulas[1] used herein are based on fits to measurements of equilibrium charge state distributions. Furthermore, perfect transmission through the accelerator has been assumed.

A companion rocket plot for I^{127} in a dilute gas terminal stripper is shown in Fig. 2. Clearly the operating regime of energies is much lower than in the foil case. Note, however, that the significant positive skewness of the distribution in q permits operation at higher energies with reduced intensity.

Figure 3 is a composite rocket plot for I^{127} that is essentially a superposition of Figs. 1 and 2. Narrow lines and light numerals were used for gas whereas wide lines and heavy numerals were used for the carbon foil predictions. The distinction between gas and foil may be greatly enhanced by the use of color; e.g. red for gas and black for foil. The light, solid curve corresponds to an analyzing magnet mass energy product of 320, and the dashed curve to a product 95 magnet in a folded tandem accelerator. The optimum combination of

stripper material, q, and V_T can be deduced quickly. In the 100-200 MeV range gas stripping and charge 6 or 7 is preferred whereas foil stripping and charge 14 or 15 is indicated for the 300-400 MeV range. In the 200-300 MeV region, the maximum terminal voltage and experimental intensity requirements may dictate the configuration selected. Of course foil lifetime will be another very important consideration.

Light ions may be treated with a slightly modified format that eliminates the overlap between gas and foil predicted I_q values. Figure 4 is an example for S^{32} ions in the 10-30 MV range. The "q lines" for gases are displaced down and those for foils up. Thus for V_T = 20 MV and E = 180 MeV, I_8 is 35% for gases and 12% for foils. Another typical example for a different range of terminal voltage, 4-14 MV, is shown in Fig. 5, a rocket plot for S^{32} with $35 \leq E \leq 160$ MeV. Gas stripping yields reasonably intense beams for $E \lesssim 100$ MeV.

Higher energies can be attained by utilization of a second stripper located at some intermediate potential between active HE tube sections. This second stripper is usually a carbon foil whose mean life may be an order of magnitude longer than that of a terminal foil as demonstrated by the measurements of Thieberger and Wegner.[2]

Figure 6 is a rocket plot for dual stripping with the second stripper being a carbon foil at a potential of 2/3 V_T. The projectile is U^{238}, V_T = 10-30 MV, and E = 400-900 MeV. Gas-foil predictions are drawn with light lines and foil-foil with dark lines. Results are given only for a single charge state q_1 selected after the terminal stripper, where q_1 is the most probable charge at approximately 25 MV. The regimes for gas-foil and foil-foil operation are delineated clearly. Comparison with the corresponding single stripper rocket plot leads quickly to the optimum configuration of strippers, charge states, and terminal voltage for a specified final energy. For U^{238} and other heavy projectiles, dual stripping extends

the operating range of large tandem accelerators to significantly higher energies.

In summary, rocket plots for tandem accelerators have several advantages: they are compact, comprehensive, and efficient; they facilitate both experiment planning and beam preparation; and they are fun to use!

One obvious limitation imposed by the scarcity of stripping measurements is that semi-empirical formulas must be employed to calculate intensities. An apparent limitation, the omission of beam losses due to imperfect transmission of beam through the accelerator, is actually an advantage in that the single stripper rocket plots may be applicable to any tandem electrostatic accelerator.

References

1. R. O. Sayer, Revue de Physique Appliquee 12, 1543 (1977).

2. P. Thieberger and H. E. Wegner, Nucl. Inst. Meth. 126, 231 (1975).

Discussion:

Jones: One can also put on these plots a curve which characterizes the limitations of your analyzing magnet so there is a usable and an unusable region.

Clegg: How do you get rocket plots?

Sayer: I will be glad to generate, on request, a standard set of single-stripper rocket plots for any interested tandem laboratory.

ORNL-DWG 79-7278

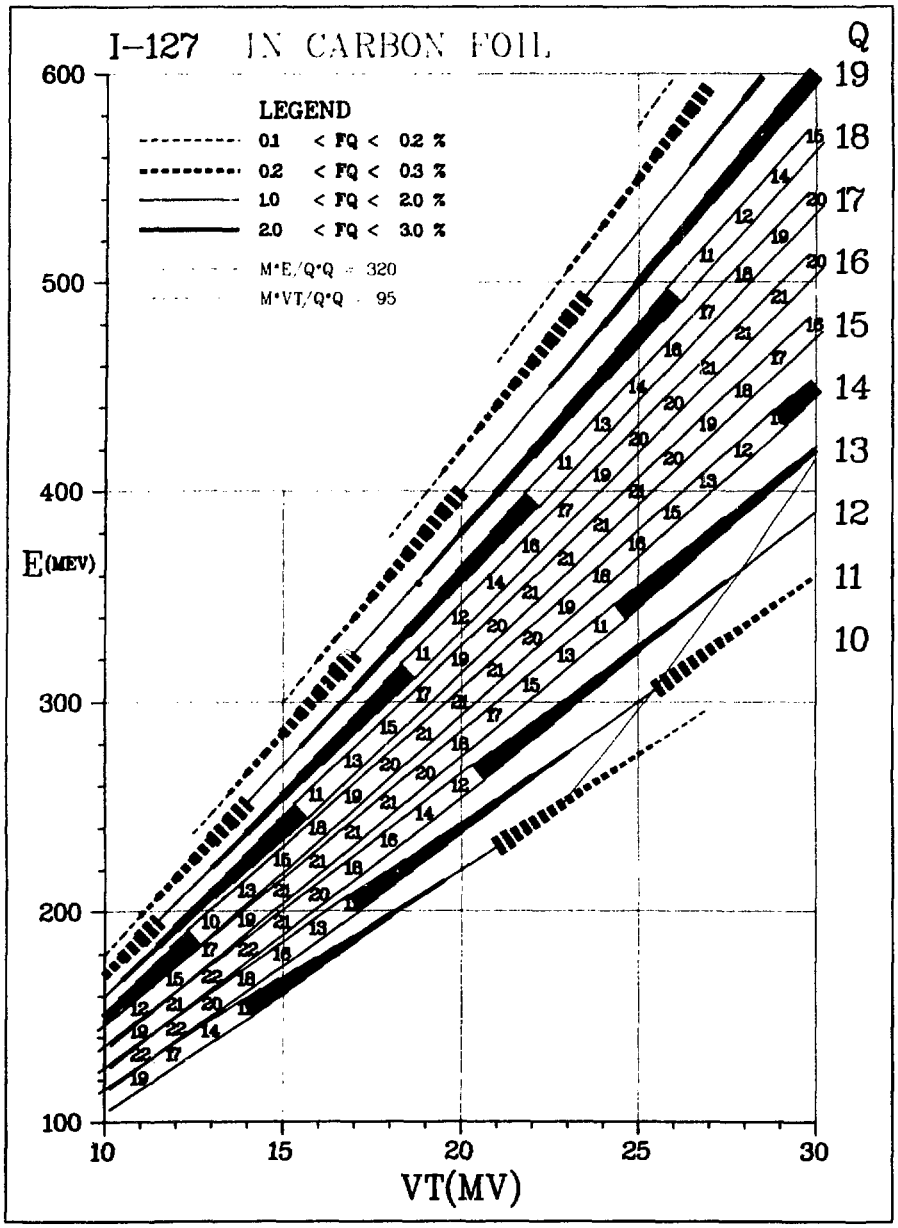


Fig. 1. Rocket plot of analyzed beam energy vs. terminal voltage for I^{127} ions in a carbon foil terminal stripper.

ORNL-DWG 79-7275

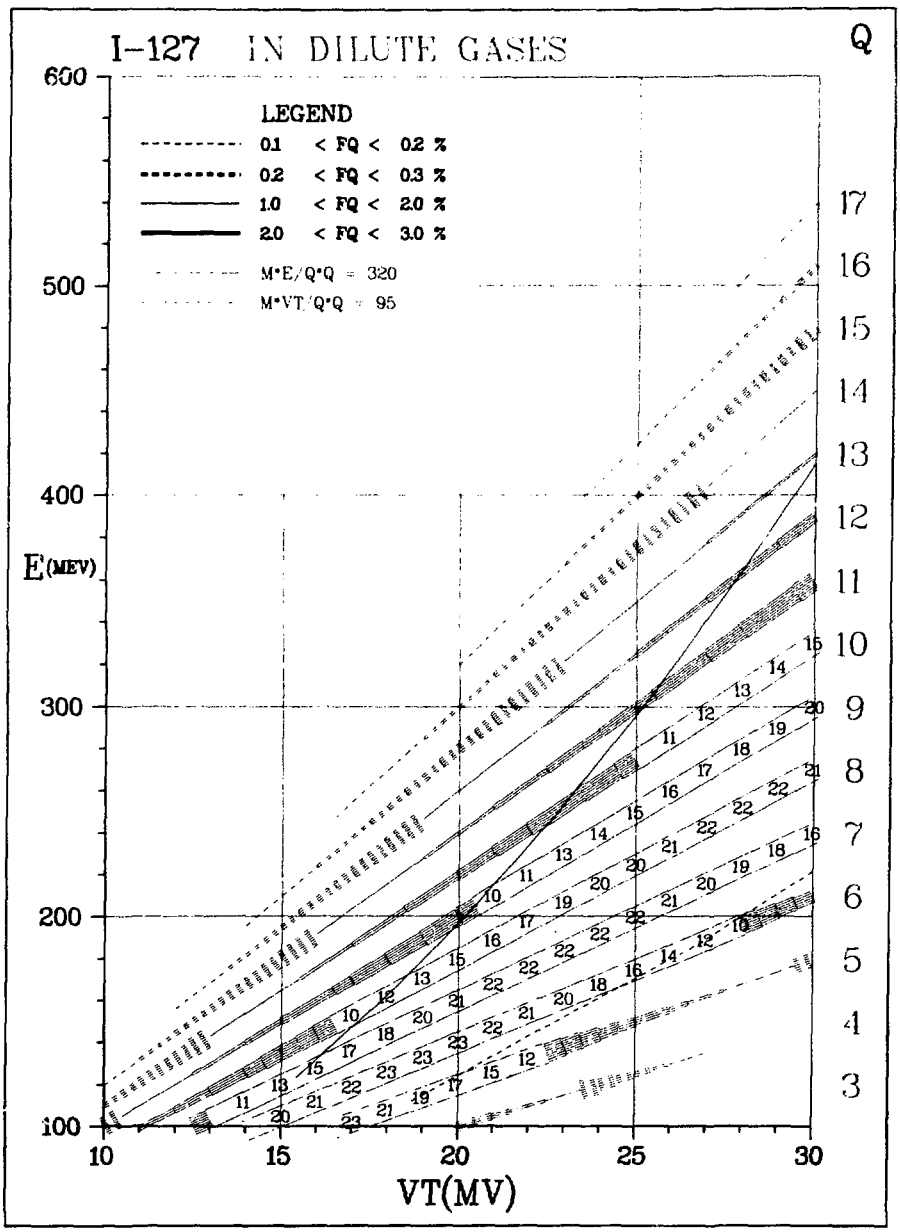


Fig. 2. Rocket plot for I^{127} ions in a dilute gas terminal stripper.

ORNL-DWG 79-7276

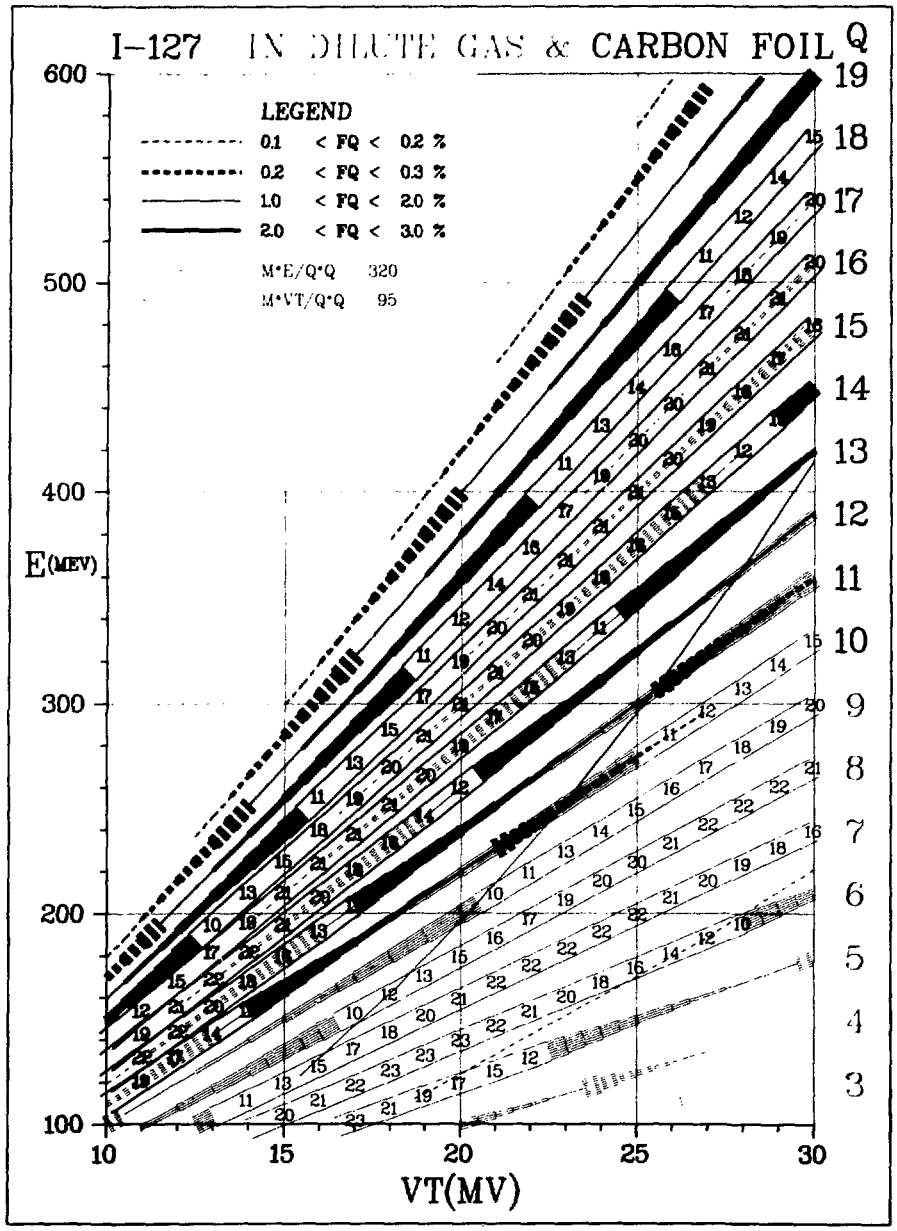


Fig. 3. Composite rocket plot for I^{127} ions in both gas and foil terminal strippers.

ORNL-DWG 79-7279

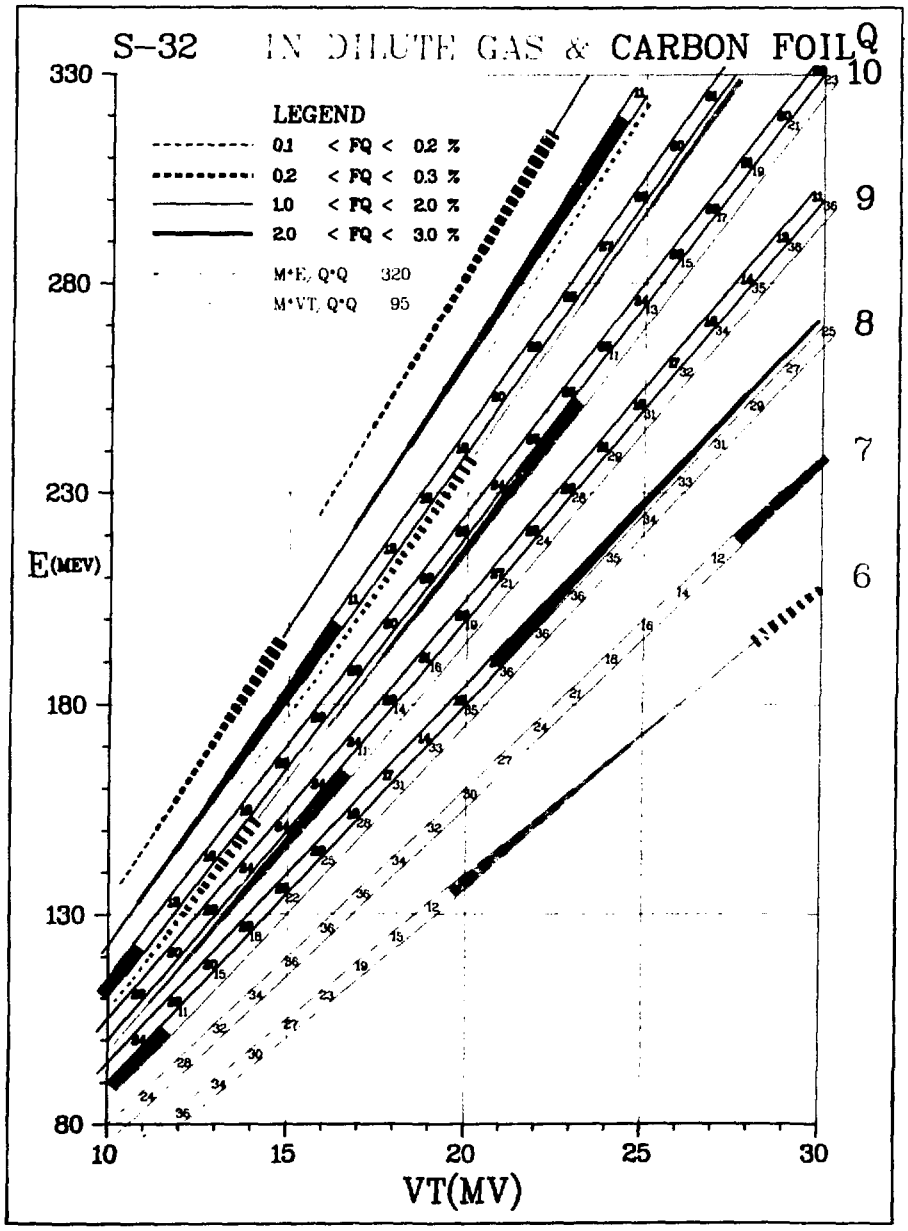


Fig. 4. Rocket plot for S^{32} ions in gas and foil terminal strippers for V_T = 10-30 MV.

ORNL-DWG 79-7277

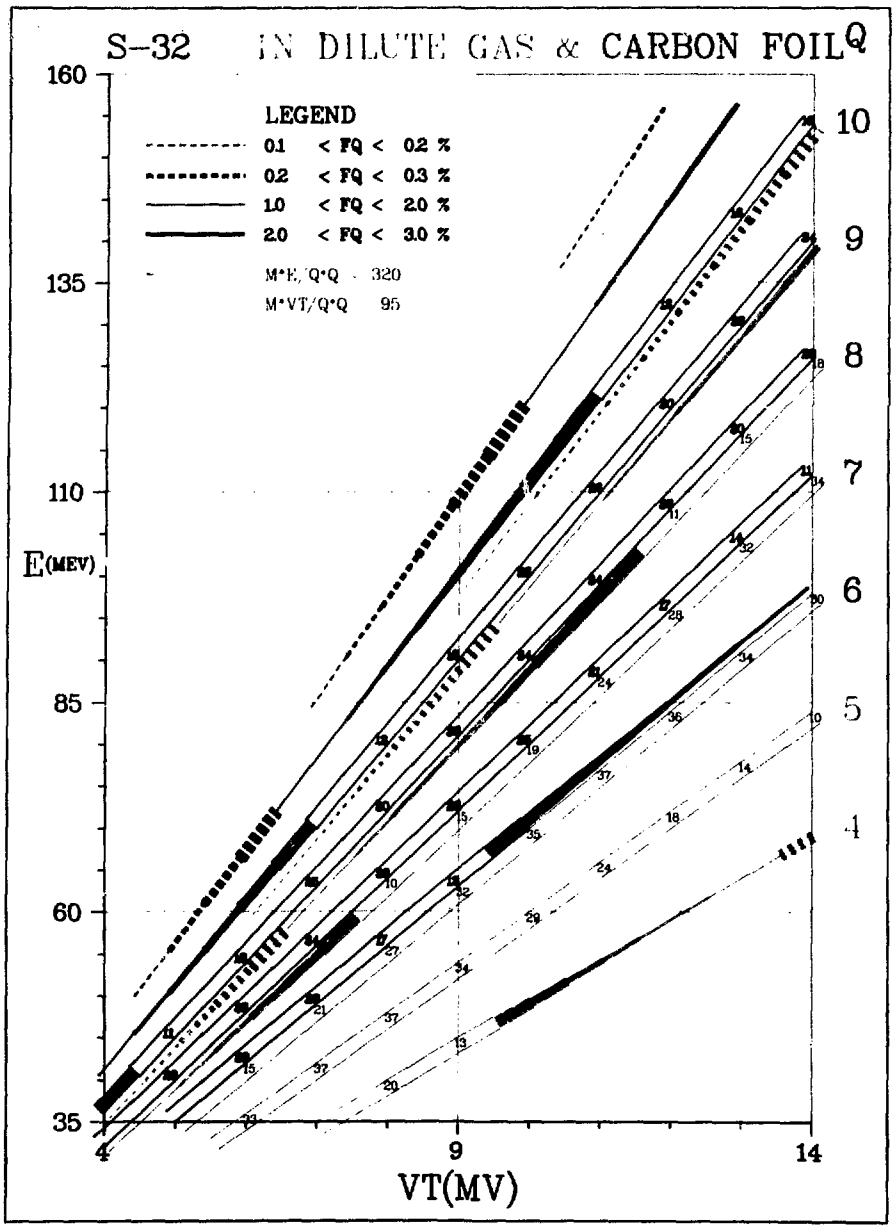


Fig. 5. Rocket plot for S^{32} ions in gas and foil terminal strippers for V_T = 4-14 MV.

ORNL-DWG 79-7280

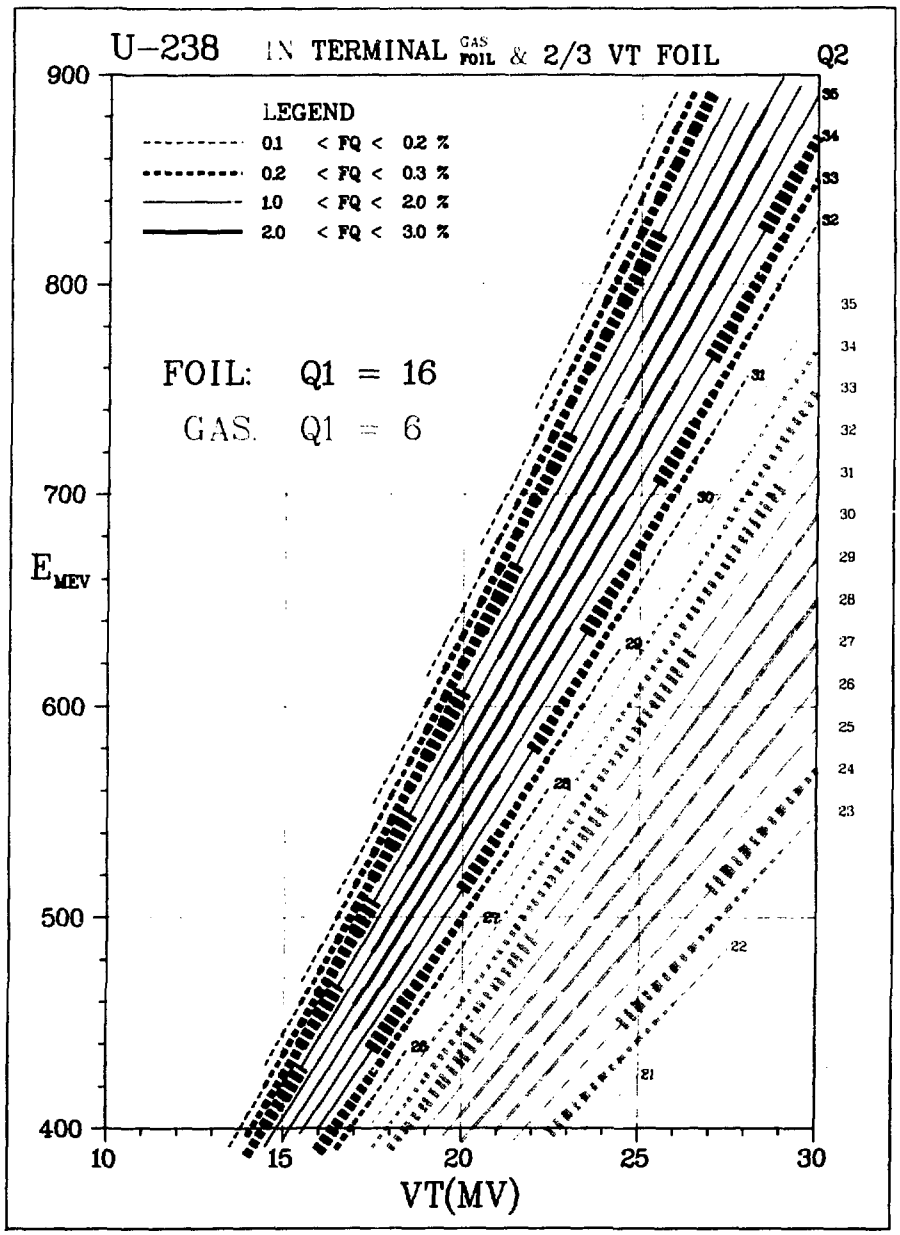


Fig. 6. Dual stripper rocket plot for U^{238} ions in a gas/foil terminal stripper and a $\frac{2}{3}$ V_T foil stripper.

INSTRUMENTATION AND OPERATING PROCEDURES FOR THE VICKSI BEAM LINE

H. Homeyer
Hahn-Meitner-Institut für Kernforschung Berlin GmbH
Bereich Kern- und Strahlenphysik
Berlin, Germany

The VICKSI beam line has a total length of about 100 m, the 30 m low energy injection line and the 70 m high energy extraction and distribution of the beam to the experimental caves. There was nothing to be kept from our old system so we could start from scratch just meeting the overall specifications and order and set up dipoles and quadrupoles according to the data we got from the designers of the injection and extraction system[1)2)].

Overall specifications:

1. Vacuum 10^{-7} - 10^{-8} torr
   All moveable feedthroughs bellow sealed
2. Maximum beam power 100 - 200 watts
   Water cooling for all parts exposed to the beam for a longer time
   Electrical insulation by BeO plates between water cooled blocks and beam probes like slits and Faraday cups
3. Alignment accuracy $\pm$ .1 mm with respect to the central beam trajectory by using markers outside vacuum
4. Compatibility with the computer control system
   Standard outputs for amplifiers, status signals etc.
5. Reliable and sufficient information for the operator

First I want to give a survey on the different types of the diagnostic elements we use. I start with their mechanical performance, then I proceed to the readouts and displays with the associated electronics. Finally I like to discuss the first year of experience with the system.

The different types of diagnostic components are

1. Beam profile monitors (BPM) for nondestructive beam observation
2. Slit pairs for accurate centering, long term stability check and beam limitation
3. Faraday cups
4. Emittance measurement device for accurate beam matching control between the accelerators and the beam handling system.

The beam profile monitor is a modified version of the design proposed by Meier and Richter[3]. An ellipse mounted on an axis tilted 45° with respect to the x-y plane of the beam hits the beam in x and y direction during rotation. The pickup signal displayed on a normal oscilloscpe triggered with rotation frequency shows beam width and position (see fig.1 a). Off center beam position can easily be detected: deviation of the beam center from the ellipse center shows up in double peaking of the pickup signal in the respective direction of deviation.

Rotation is performed by a synchroneous motor with stator coils outside vacuum and the rotor, small permanent magnets on the rotating shaft, inside vacuum, both separated by a thin stainless steel tube. The rotation frequency is 25 Hz. Fig.1 a shows the BPM with the ellipse inside the housing. The aperture in front of the BPM can be biased to 100 V for secondary electron suppression. When the BPM is switched off, a dc current is fed into two stator coils to keep the ellipse out of the beam.

For slits only pairs of slits mounted on one flange are used. Only the slit width can be changed leaving the center of the slit always fixed. Fig. 2 shows the principle. Two

bellow sealed feedthroughs with screw drives are moved by a worm gear drive. Driving is performed by a stepping motor, the slit width is read out by an angular encoder fixed to the worm gear drive. Fig. 3 shows the slit. The slit width accuracy and reproducibility is better than $\pm$.1 mm. The slit center, measured from the surface of the mounting flange can be adjusted manually within 4 mm to an accuracy of $\pm$ .1 mm.

The Faraday cups are mounted on water cooled actuators. These actuators are pneumatically driven water cooled bellow sealed linear feedthroughs with a stroke of 50 mm. The actuators are generally supplied with five electrical feedthroughs for the current signal, the secondary electron supression electrode and 3 spares (fig. 4).

The most accurate beam diagnostic element is the emittance measurement device. In principle we use a moving slit to scan the position and a detector head with 15 conducting stripes silk screened on a BeO plate to measure the angular divergence for each position. The way of scanning is more or less a copy of the BPM movement. The advantage is that the x and y plane are scanned with the same mechanical and electronical setup. Fig. 5 shows the principle. Rotation of the axis leads to a linear displacement (linear within a certain range) of the slit and the detector head. Both, slit (fig. 6) and detector (fig. 7) are mounted on high precision notary feedthroughs driven by a stepping motor via a 1:10 transmission gear. Both elements can be mounted separately into standard 4" i. d. tees. Slit width separation of stripes on the detector and the distance between slit and detector are chosen to match the desired resolution.

With the description of the emittance measurement I start the chapter about associated electronics and display modes. The slit and the 15 detector stripes are each connected to a current integrator where the integration times and loading capacitors can be adjustea according to the beam intensity. The measurement is performed completely by a computer program. The position is scanned in 30 steps the angle in 2 x 15 resulting in a 30x30 x, x′ or y, y′ matrix. The total measuring time is depending on beam intensity. For typical beam currents of some μA the measuring time is approximately 30 sec. The resolution is $\pm$ .1 mm and $\pm$1 mrad. Fig. 8 shows an example of a phase ellipse just at the entrance of the cyclotron.

Current signals from the Faraday cups are fed into a 6 decade linear current to voltage converters (fig. 9). The range of the amplifiers is set automatically or manually by a monitor on the control desk. The connection between the monitor and the different amplifiers is made by a computer controlled multiplexer. On the monitor the range and the Faraday cup number are displayed digitally, a normal meter shows the analog value (fig. 10).

For the BPM's we use the same amplifiers (fig. 11). There are 4 independent oscilloscope tracks where any combination of four BPM can be assigned to. The four BPM drive generators fixed to the respective oscilloscope track are displaced by a phase difference of 30° to prevent any combination of BPMs hitting the beam at the same rotating phase.
Fig. 10 shows the beam diagnostic part of the control desk. On top are three independent meters for Faraday cups or similar beam probes. Below two scopes with 2 tracks each for the BPM's.

The slit currents are fed into 6 decade logarithmic amplifiers and from there directly into a analog bus of the computer. These signals can only be displayed digitally on the color TV of the control desk.

Before I come to the operating procedures I like to say something the overall alignment. Fig. 12 shows the injection line with all diagnostic elements. The slits following each bending were very carefully aligned to the axis of the bending magnet. To reach a high degree of accuracy magnets and diagnostic stations are equipped with alignment markers outside vacuum. The tops of these markers have a very accurately measured distance to center of the beam level, defined by the center of the magnet gap or the center of the vertical slit. The projection of the middle of the markers define exactly the horizontal trajectory of the beam. The vertical adjustment has then been performed with a level with reference to some marker on the wall of the accelerator hall. The horizontal alignment has been done with a theodolite according to a frame of reference on the floor of the hall. The mean deviation of the actual alignment horizontally and vertically is ± 1 mm.

The magnetic fields of dipoles and quadrupoles have been mapped to determine exact calibration effective lengths and effective input and exit angles respectively. The actual data (effective lengths, angles drift spaces etc.) have been put into the transport program to calculate the exact settings. Fig. 13 and fig 14 show the extraction line with the alignment markers on magnets (fig. 13) and the standard diagnostic box (fig. 14) can be seen.

With all these preliminaries the operating procedures turned out to be very simple. The phase ellipse measured after the analysing slit is adjusted by a quadrupole and some steerers following the Van de Graaff accelerator. The computer sets all dipoles and quadrupoles to the nominal value. Slits following the magnets are used for fine tuning of the dipoles. The focussing is checked by the BPM's.

When the beam has been brought to the cyclotron the computer makes a comparison between nominal values and those coming from fine tuning. Generally the deviation is not larger than 1 - 2 % in quadrupole settings and much less at the dipoles. The same procedures works in the high energy area.
Whenever this procedures failed - which was rather often the case in the beginning - the reason was in some hardware errors like errors in the alignment, wrong cabling, shorts in quadrupole coils etc.
The overall transmission of the useable beam*) from the ion source through the Van de Graaff, injection cyclotron to the target area is better than 80 % after one year of experience with the system.

I like to summarize our experience with following remarks:
The preliminaries for the setup of the whole system - installation of the frame of reference for the alignment, measurement of all magnetic elements, the alignment itself were very tideous and time consuming. But it pays off very rapidly.

A comparably large amount of beam diagnostic elements has been installed. After one year experience roughly 25 % of them are not used so frequently any more. On the other hand they were necessary in the beginning to get a fast knowledge of the whole system and to detect hardware and software failures.

*) useable beam: current of the ion source in particle nA x stripper efficiency x buncher efficiency or phase width acceptance of the cyclotron.

The combination of direct (fast feedback) signals like Faraday cup currents and BPM pickups with slower more accurate computer processed signals (emittance, slit currents) has turned out to give the operator adequate and sufficient information for the tuning and running of the system.

References

1) G. Hinderer and K. H. Maier, IEEE on Nuclear Science Vol. NS-22, No. 3, June 1975

2) F. Hinterberger, B. Efken, G. Hinder, and K. H. Maier, Nuclear Instr. and Meth. 21 (1974) 525-532

3) J. H. Meier, F. W. Richter, Nucl. Instr. and Meth. 134 (1976) 213-215

Discussion:

Tykesson: In your emittance measuring device the resolution is determined by the slit. Do you know the smallest slit size that can be used before the slit scattering would make it lose transmittance?

Homeyer: Two things to say about that. The accuracy with which we want to know the beam position and size, that's roughly 0.1 mm, and our slit is 0.4 mm now. That is also the distance of the stripes on the detector head. As far as the slit is concerned we use a slit configuration found in Heidelberg in work with microbeams where they have looked at different slit shapes to find the best shape. They looked at round shapes and others and ended up with a profile having a 10° cut on one side and a 4° cut on the other. That work has been done four years ago by Schwalm and others. They show that with this configuration the least scattering occurs. The surfaces are polished.

Wegner: What is the smallest beam that your profile scanners can see, or what is the noise level on them? As you change the beam intensity and work it up, do you manually change gain on the display?

Homeyer: The lowest beam current we had in the system was roughly 2 na full scale, and the monitors worked quite well. A lot of work has gone into that. The sensitivity depends mainly on the polishing of the pickup and the correct spring that presses these things together. Right now I think that 1 na can be detected. There are several features of the display I did not talk about. You can choose either the automatic display on both the Faraday cup and BPM so that if the height exceeds a certain level it switches to the other range. On the other hand there is a feature which means automatic pulse height leveling. After each revolution there is a final amplifier that runs the signal up and down so as you focus you have the same height and the width is increasing or decreasing.

Billen: Do you ever have any trouble with cross-talk between the wires on the last detector in your emittance device after prolonged running?

Homeyer: Surprisingly not. That is one thing we thought we might have trouble with. We started, first of all, with just plates made from printed circuit boards and then put one grounded stripe in between and used different configurations. We found that the cross-talk is essentially zero. The way we checked this was to put a slit in front of the beam and squeeze it down and see if we see it on one single side. I think the reason is that the silk screening makes a profile; this is the

conductor and this is the insulator, and there is little chance that electrons from here reach the other electrode. We don't see it but I really don't have an explanation.

Larson: The loop that you use in your beam profile monitor cuts the beam simultaneously in two directions, thus averaging out and essentially symmetrizing the beam in a certain respect. Does this hinder your interpretation of the beam or cause any problems?

Homeyer: It causes problems if you don't know how to handle it. You need some time to learn it. When you really want to see the width of the beam, you move it a little bit off center. Then you center it again and then you see the two peaks coming up, especially if you have a broad beam. After some experience you can move the beam in a known way, look at the picture, and interpret what is going on.

Larson: Am I correct in interpreting that when the beam is centered you get the most symmetric pattern?

Homeyer: Yes.

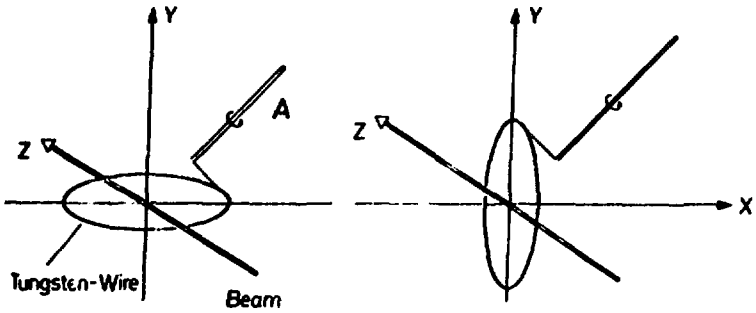


Fig. 1 a): Principle of the scanning mode of the BPM: Left y, right x scanning. Both positions differ by a 180° rotation around axis A

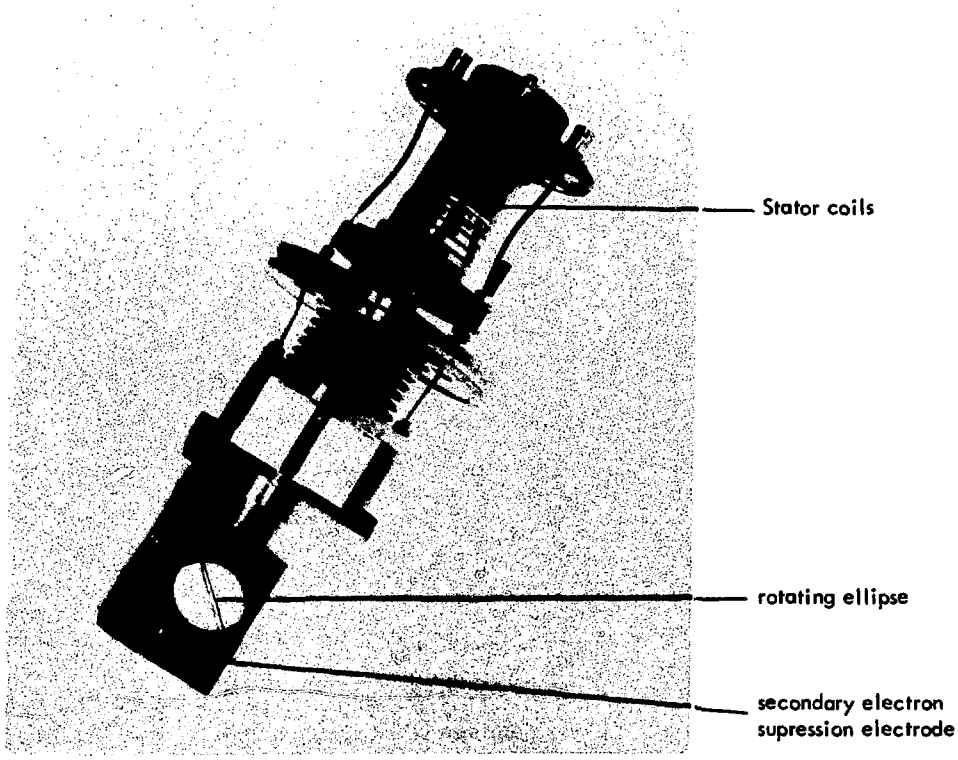


Fig. 1 b): Model of the BPM

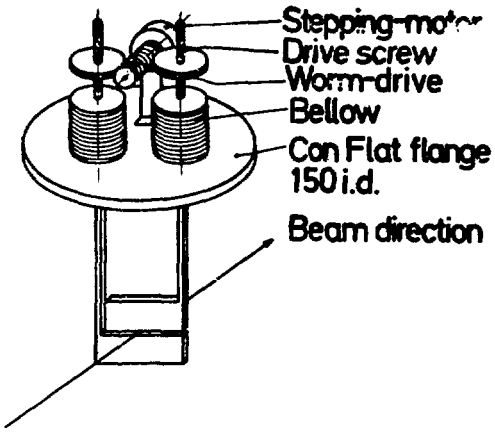


Fig. 2: Principle of the slit pair system

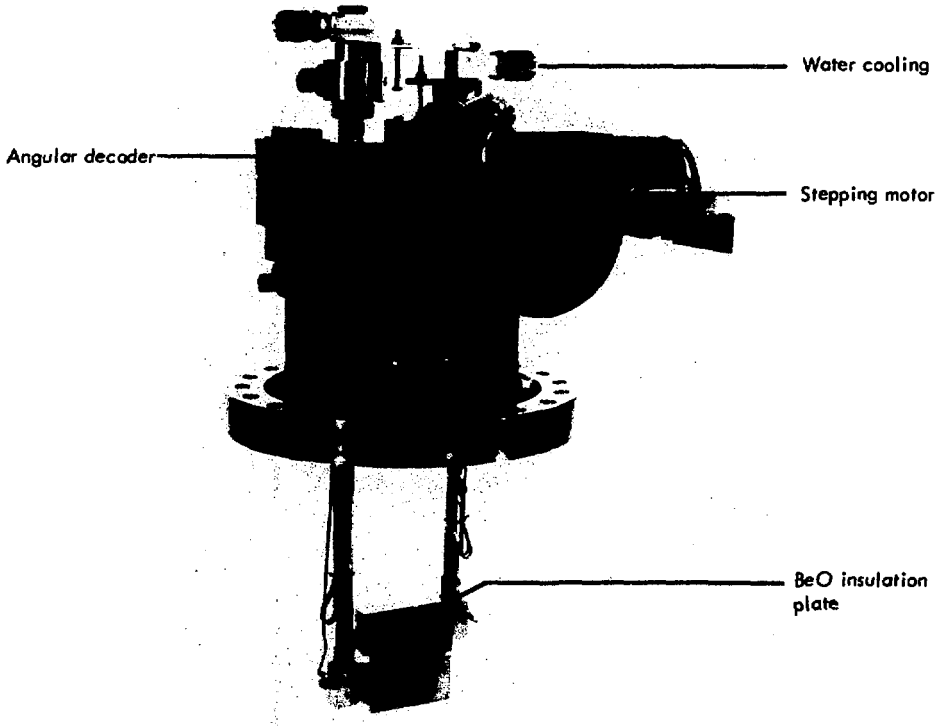


Fig. 3: Slit

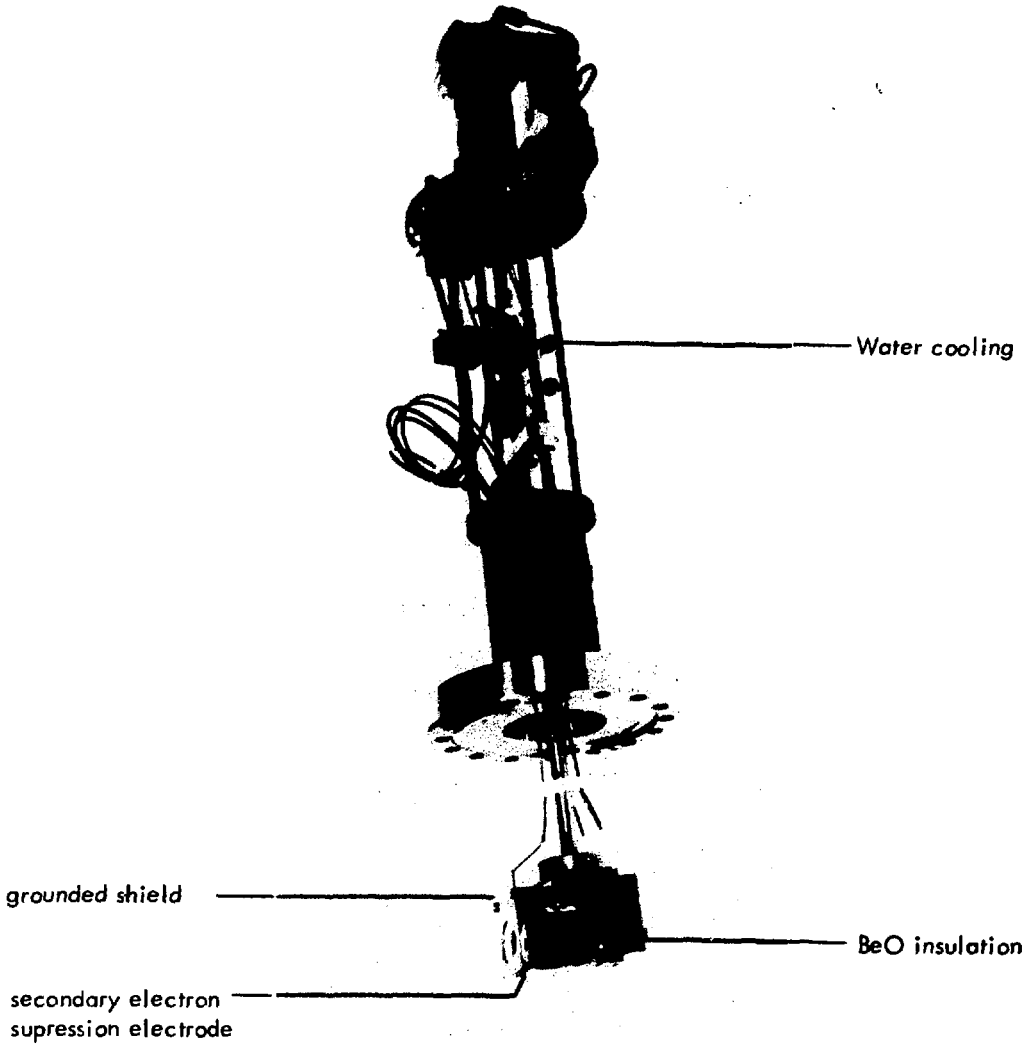


Fig. 4: Water cooled actuator with Faraday cup

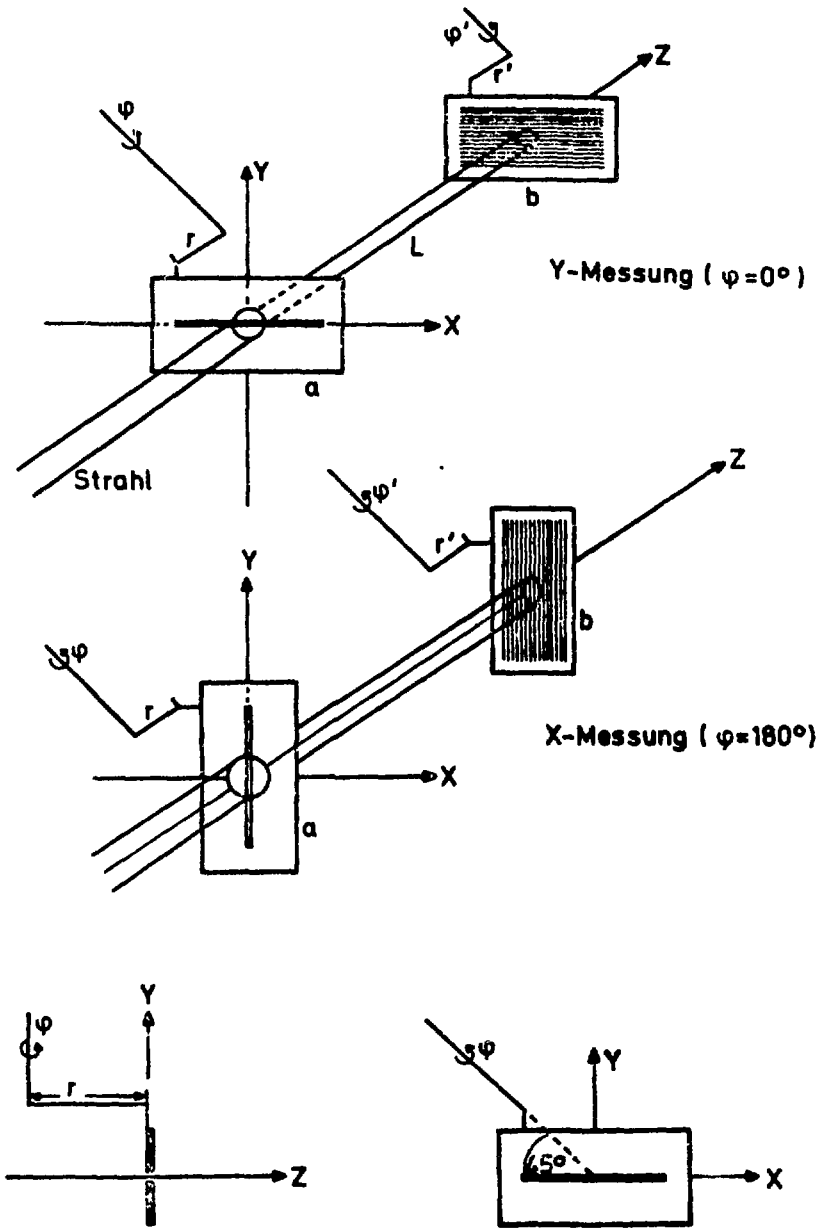


Fig. 5: Principle of emittance measurement

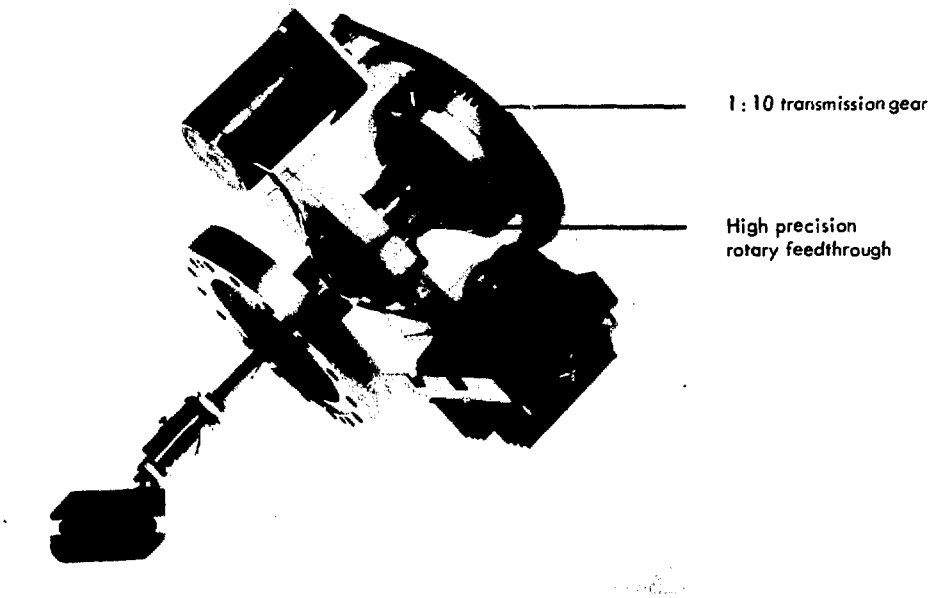


Fig. 6: Slit for emittance measurement

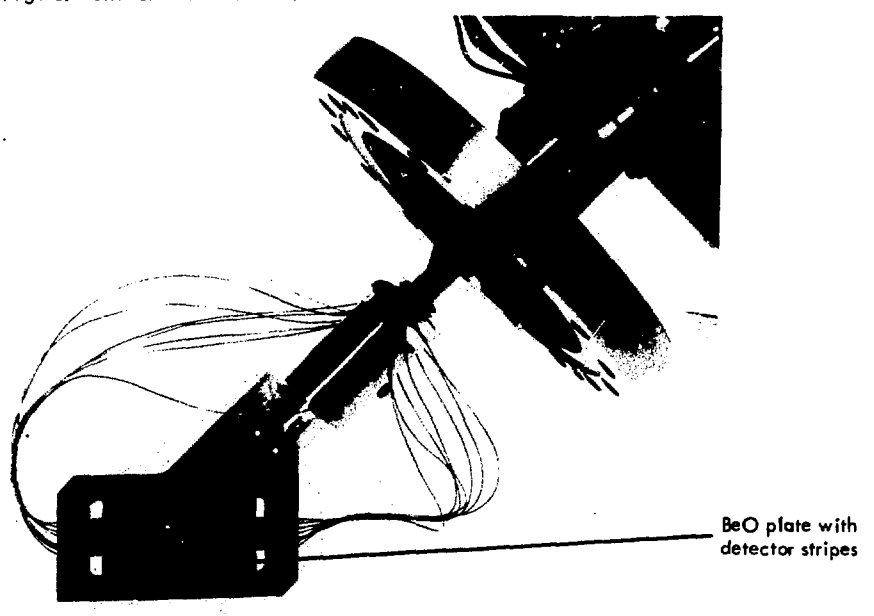


Fig. 7: Detector for the measurement of the angular spreading

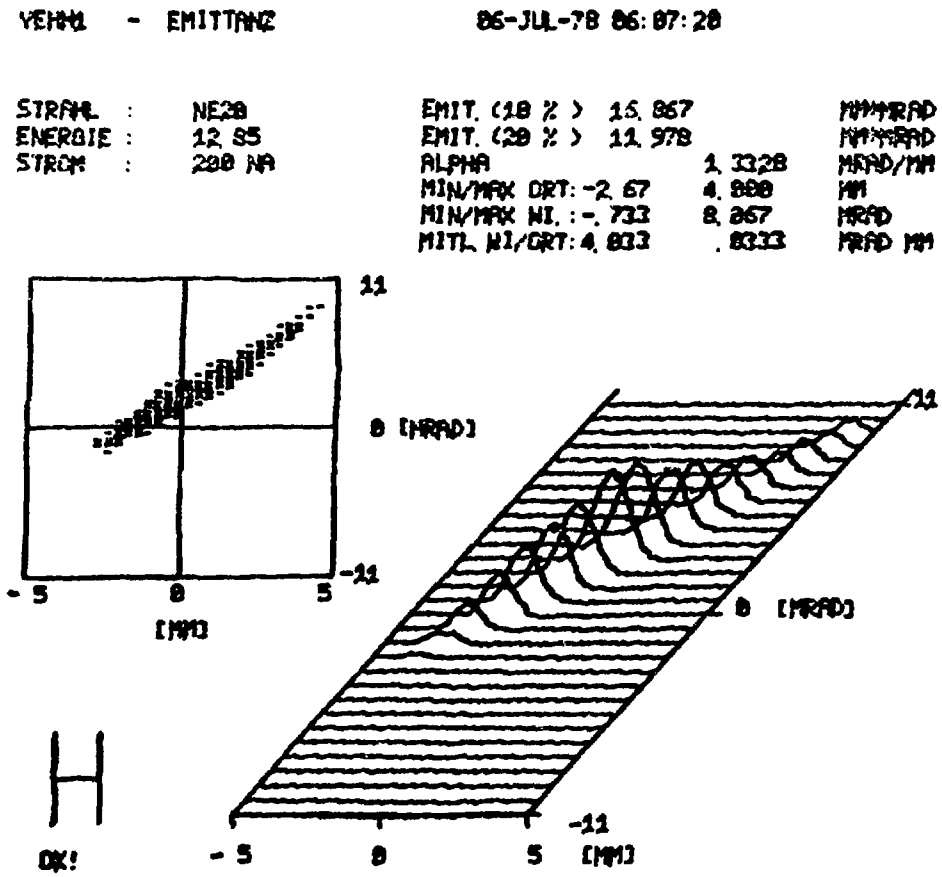


Fig. 8: Measured phase ellipse at the entrance of the cyclotron. Contour and map display.

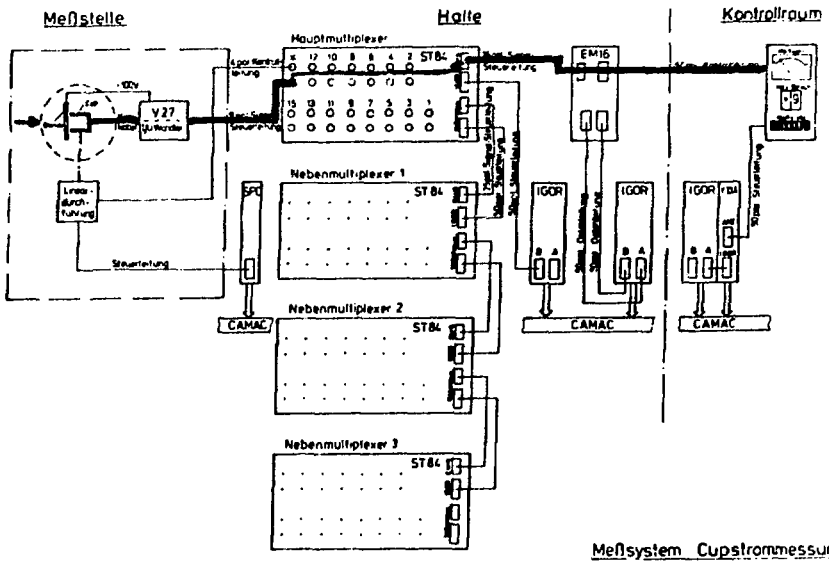


Fig. 9: Principle of current measurement of Faraday Cups

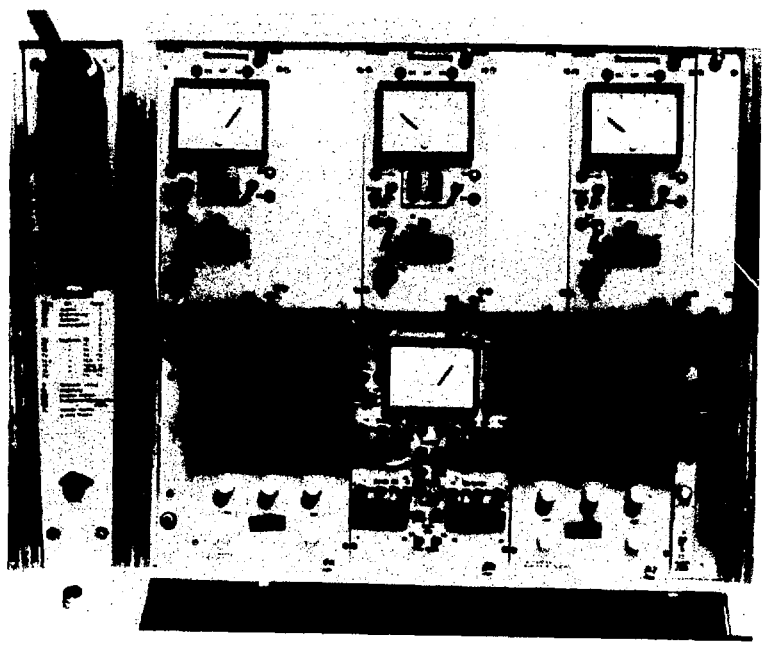


Fig. 10: Beam diagnostic part of the control desk. Top: 3 independent lines for current measurement, Bottom: BPM traces

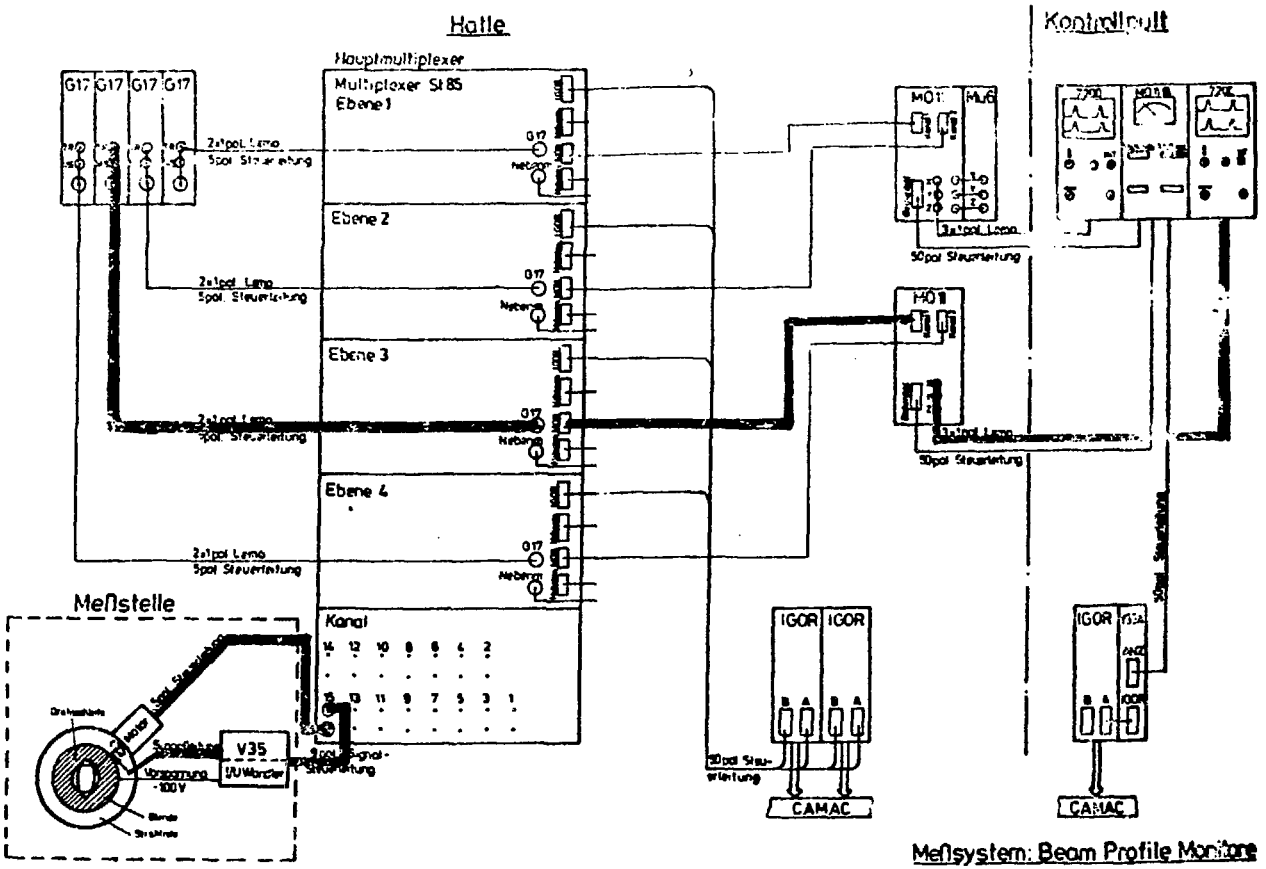


Fig. 11: Principle of the BPM electronics

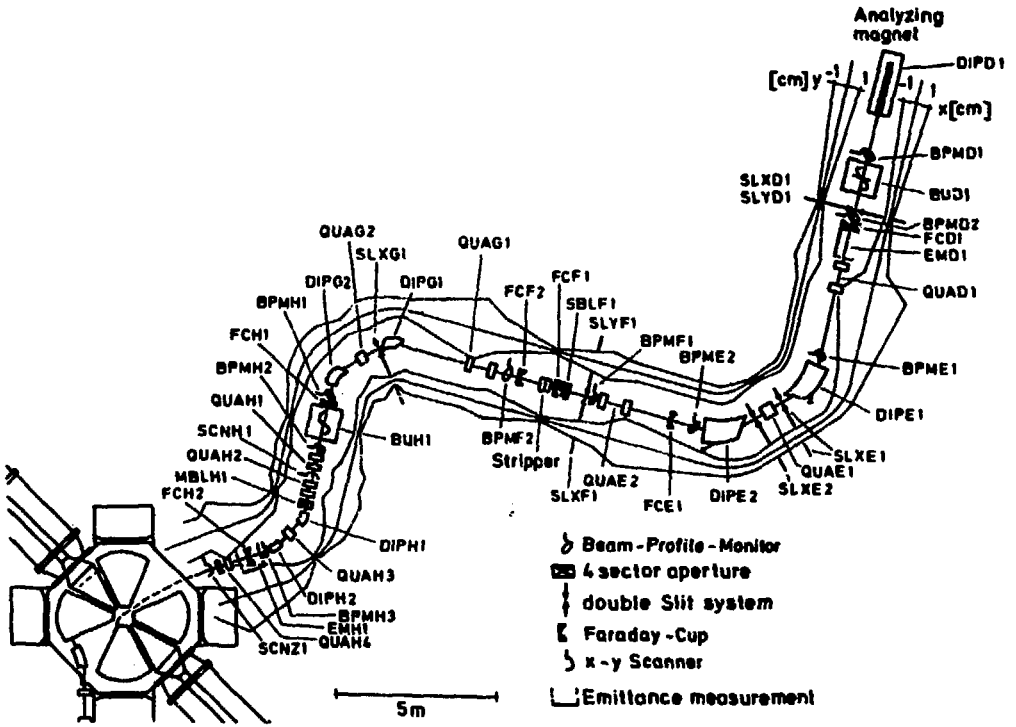


Fig. 12: Injection line of the VICKSI accelerator.
Beam path from the 90° analysing magnet of the Van-de-Graaff to the cyclotron. Included are all focussing and bending elements as well as beam diagnostic instruments. The upper and lower curves represent the vertical and horizontal beam envelopes, respectively.

Fig. 13: Extraction beam line

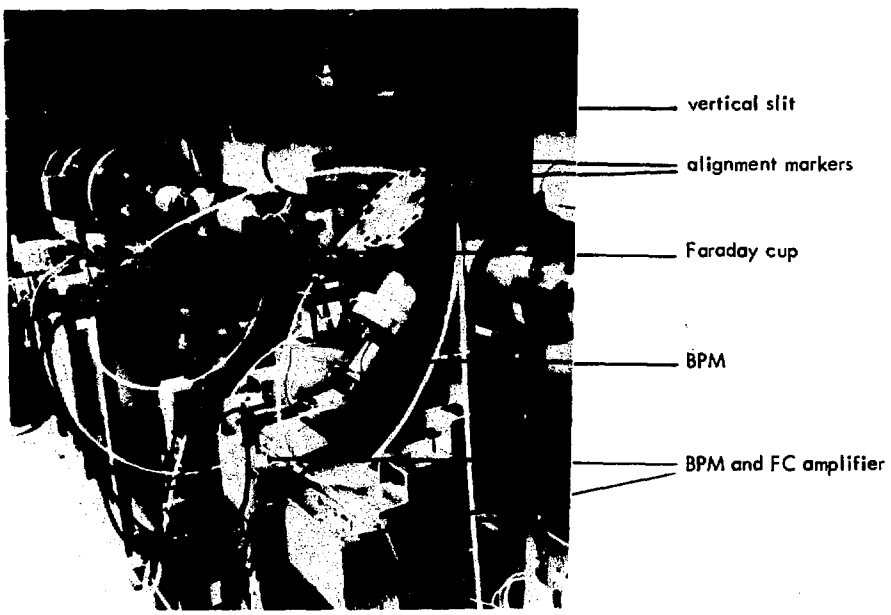


Fig. 14: Standard diagnostic box

PHASE MEASUREMENT AND CONTROL OF BUNCHED BEAMS*

ROBERT N. LEWIS

Electronics Division, Argonne National Laboratory,

Argonne, Illinois 60439 USA

1. Application

An ion beam buncher has been developed at ANL for bunching all ion species through a tandem accelerator. Bunching to approximately 1 ns is achieved by the system[1] and a superconducting helix further bunches to about 50 ps.[2] These short pulses are to be injected into a superconducting linear accelerator designed to accelerate heavy ions (up to 58) from the tandem to over 6 MeV/nucleon.

Transit time variations through the tandem, caused by ripple and fluctuations in the injection and lens power supplies and terminal voltage, and to varying voltage distributions in the accelerating tube, cause a beam-phase variation at the output of the tandem. A beam-phase measurement and control system was designed and installed in conjunction with the ion beam buncher to control beam phase at the tandem output.[3]

Editor's Note: This paper was not verbally presented due to illness.

*Work performed under the auspices of the U. S. Department of Energy.

2. Beam-phase detector

The beam-phase detector should have several desirable characteristics. These are:

a) The detector must have high sensitivity with low noise and be capable of responding to the high frequency pulse rate.

b) The detector should have a large dynamic range. It is desirable that beams with average intensities as low as several picoamperes be measured and controlled; however, it is also desirable that beams of hundreds of nanoamperes be measured and controlled without readjustment.

c) It should be noninteracting/nondestructive. It is highly desirable that the process of phase detection in no way modify the beam quality in any of the density distributions in six-dimensional phase space.

d) The detector should be simple. It is desirable that the device be easy to construct and operate.

A high-Q resonant structure, tuned to the beam pulse rate or to a higher harmonic thereof, possesses these characteristics. It will also signal-average over a number of pulses thus enhancing the S/N ratio; the number of pulses being approximately Q/H, where H is the order of the harmonic. This follows because the resonator response decays as:

$$i(n) = i_0 \, e^{-n\pi H/Q} \quad , \tag{1}$$

where n is the number of beam pulse intervals. Thus for a second harmonic resonator operating at 100 MHz with a Q of 1000, the output is the average for about 500 pulses (bunches) and the averaging time is about 10 μs.

The phase detector is a room temperature helical resonator, one-half wavelength long at the bunching frequency (48.5 MHz). The unloaded Q is about 2000 and the impedance about 200 Ω. The helix is a slow-wave structure

in that current, stimulated by the axial electric field of a moving bunch of ions, flows along the spiral at approximately the velocity of light but the forward progress along the beam line is reduced in proportion to the pitch of the helix and approximately matches the velocity of the ion beam. Thus, for a time period of about 10 ns, each ion bunch is coupled to the helix for energy transfer. (The ion velocity from the tandem accelerator does not vary appreciably for ions from ^{12}C to ^{58}Ni because as the mass increases so does the energy due to the higher charge state.)

The circulating tank current, i_h, in the helical resonator due to an average beam value, i_b, is

$$i_h = i_b Q_L \beta \quad , \tag{2}$$

where Q_L is the loaded Q of the resonator and β is the bunching efficiency; $\approx$ 0.75-0.80 in our case. Eq. (2) presupposes that the degree of bunching and the beam velocity is such that the transit time of the ion bunch through the helix is less than about 10 ns.

The helix current is an average value due to about 1000 bunches. The resonator response to an individual bunch decays as

$$i(t) = i_o \, e^{-\pi ft/Q} \quad . \tag{3}$$

Thus when $ft = Q$ (about 1000 cycles or bunches), the current due to i_o has decreased to 4.3% of its initial value. As a consequence, the response of the resonator builds for the first 1000 bunches or so and its output is the average over about 20 μs.

The voltage of the resonator is $e_h - iZ$ and the useful input, e_{in}, to a preamplifier is

$$e_{in} = ke_h = kQ_L\beta Zi_b \quad , \tag{4}$$

where k is the coupling factor from the helix to the preamp. We routinely use a broad-band preamp with 50 Ω input impedance coupled to the helix by about 1 pF of stray capacity. The value for k is thus about 0.03. For $\beta = 0.75$, Q_L - 1000, Z - 200, k - 0.03 and i_b - 1 nA, eq. (5) yields:

$$e_{in} = 0.03(10^3)(0.75)(200)10^{-9} - 4.5\ \mu V \quad . \tag{5}$$

We have also used low-noise FET narrow-band preamplifiers, optimally coupled with an improvement of an order or so of magnitude in output signal level and a larger improvement in S/N.

The beam-phase detection method described here is based upon vector addition in a detector of a beam-induced signal and a phase-flipped (-90° to +90°) reference signal. The resonator is excited by the periodic ion bursts passing through it, as well as by an rf reference signal which is reversed in phase at an audio rate, f_a. The low-level output signal, comprised of the vector sum of the beam-induced signal and the phase-flipped reference, is amplified by a tuned preamplifier and a radio receiver where it is a.m. detected. The signal is combined in a phase detector with the phase-flip square wave, f_a, to provide a phase error voltage that is used to control the phase of the buncher via an electronic delay.

The vector voltage relationships between the reference and the beam-induced signals are shown in fig. 1. In (a) the desired condition is shown. Vector 101 is the reference voltage for one-half period of the sampling cycle and vector 102 is the reference voltage for the other half of the sampling cycle. Vector 100 is the beam-induced signal.

In fig. 1b the desired beam phase is shown with vectors 103 and 104 equal in amplitude. This results in a constant-amplitude signal out of the detector. In fig. 1c the condition of early arrival of beam is shown. Vector 103 is larger than vector 104 and the detector output signal is the amplitude-modulated wave shown. In fig. 1d the condition of late beam arrival is shown. Vector 104 is now larger than vector 103 and the phase of the amplitude modulation is 180° different than that for fig. 1c.

Since the signals resulting from the two phases of the reference are processed in the same manner by the electronic system preceding the synchronous detector, the operation is independent of phase shift in the detector resonant circuit and later electronics. The automatic gain control of the radio receiver does not interfere with the phase information and permits operation over a large dynamic range, 10^6 to 1.

If the amplitude-detected signals of figs. 1c and 1d are applied to a synchronous detector along with the sampling square wave, a dc error signal is produced. The form of the error curve vs phase error is sinusoidal, being zero for zero phase error. The amplitude of the error signal off-zero depends upon not only the phase error but also upon the amplitudes of the reference and beam-induced signals.

As a consequence of the resonator memory shown by eq. (1), there is a period of false signal during and immediately following phase flip of the reference signal. The number of beam pulses during the interval is about $N = 2Q/H$ and the time interval is about

$$T_L = \frac{2Q}{fH} \tag{6}$$

In addition, the phase flip represents a phase modulation and gives rise to an rf spectrum that can easily exceed the bandwidth of the following rf amplifiers. This can cause spurious responses that result in additional false signal time.

A gating signal is thus required to gate off the false signal which occurs twice per sampling cycle. It seems reasonable to require at least 50% efficiency in signal detection and thus the upper limit on sampling rate is taken as the reciprocal of four times the false signal duration. This can be stated as

$$F_5(\max) = \frac{1}{4T_L} \tag{7}$$

where T_L is described by eq. (2) or is a larger value due to bandwidth limitations.

3. Detection and control system

As shown in fig. 2, the reference master oscillator signal, a $\cos(\omega_0 t)$, is shifted β degrees in phase by a manual phase shifter and applied to a phase-flipper (for example, a double-balanced modulator). The audio-frequency square wave, also applied to the phase-flipper, causes its output to periodically change in phase by 180°. This signal is coupled to the beam-phase detector.

A beam-induced signal, $b \sin(\omega_0 t + \phi)$, where b is related to the beam intensity and ϕ to the phase of beam transit, is added vectorially to the reference signal. The preamp output is a square-wave amplitude-modulated signal having maximum and minimum envelope values:

$$e_{max} = K\sqrt{[a^2 + b^2 + 2ab\ \sin(\phi+\beta)]},$$

$$e_{min} = K\sqrt{[a^2 + b^2 - 2ab\ \sin(\phi+\beta)]}.$$

For zero percentage amplitude modulation and for stable loop operation $\sin(\phi+\beta)$ must equal zero which means that the arrival phase is 90° with respect to the reference.

The preamp output signal is fed to a radio receiver having a broad-band (~100 kHz) intermediate frequency response. (The wide bandwidth causes the system to be relatively insensitive to receiver tuning.) A delayed and amplified AGC system used in the receiver allows a wide range of signal amplitudes. A gate between the receiver AM detector output and the phase detector provides for gating off the false signal caused by phase-flip of the reference.

The phase detector output can be controlled in bandwidth and gain and applied to a voltage-controlled phase-shifter (VCϕ) thus controlling the master oscillator phase drive to the pulser, buncher, or other device that controls beam modulation.

A simplified drawing of the overall system is shown in fig. 3. A dc ion beam from source (1) is bent by a 40° bending magnet (2), passes through bunching grids (3), is accelerated by the FN Tandem Van de Graaff accelerator and achieves a time focus while passing through a phase detector (6). Our bunching frequency is 45.795 MHz. Thus each 21.836 ns of dc beam is bunched to a time-focus less than 1 ns wide. The superconducting buncher is a helix and further bunches the 1 ns bunch to about 50 ps for injection into a superconducting helical linac. It is the function of phase detector (6), signal processor (7), and voltage-controlled phase-shifter (8) to cause the bunches

to arrive at the superconducting buncher at a precise phase angle with respect to the master oscillator signal as selected by manual phase shifter (9). Changes in transit time between the bunching grids (3) and the phase detector (6) will arise due to source and terminal voltage drift, as well as imperfect beam optics, and a high-frequency jitter in transit time will appear due to source and terminal ripple, column section arching, etc. Thus the phase control system must have a bandwidth from dc to several hundred Hz.

4. Initial tests

^{12}C ions from a sputter source were injected at 90 keV and accelerated to 45 MeV. Observation of the phase-error signal revealed 60 and 120 Hz components as well as a low-frequency "bounce." The 60 and 120 Hz energy modulation was due to ripple on the source elements, and the low-frequency energy modulation was determined to be due to terminal voltage variation.

A 15 min run was made without phase lock, resulting in fig. 4a. The full width half maximum (fwhm) is 1.2 ns. With phase lock, a run of 1.5 hours gave a fwhm of 0.9 ns with 76% of the dc beam in its peak. This is shown in fig. 4b.

Shown in fig. 5 are oscilloscope waveforms for phase error, upper trace; and phase lock, lower trace. The oscilloscope was connected to the "receiver output" jack. Fig. 5a shows a small phase error, indicated by a small separation of the traces in the upper waveform. The lower trace shows the condition for phase lock. The amplitide modulation is due to injection power supply ripple. Note that phase lock does not remove amplitide variations of the beam due to source modulation but only servoes the beam phase to that of the master oscillator.

Fig. 5b is a small section of fig. 5a using a faster time base. Fig. 5c shows phase lock of a beam of heavier ions, requiring different beam optics and, therefore, having different injection power supply ripple than for fig. 5a.

Operational results of the phase control system are more dramatic when used with bunched beams of heavier ions. For example, Fig. 6 is a time-spectrum of a bunched ^{58}Ni beam, injected at 132 kV and accelerated to 82.5 MeV. Without phase lock the spread is 2.5 ns fwhm. Fig. 7 is a time-spectrum of the same beam with phase lock. The width is now about 1 ns fwhm with 72% of the d.c. beam in its peak.

5. Additional applications

A nondestructive system for determining the energy of an ion beam by measurement of transit time between two detectors is under development. The system uses two helix resonators, spaced 1 m apart. For a frequency of 50 MHz and v/c ranging from 0.05 to 0.15, the phase difference between detectors will range from approximately 500° to 1200°. The angular resolution of the readout will be 0.1° and the overall accuracy is expected to be 0.2°. This will provide a transit time readout between 22 and 67 ns with a maximum error of 10 ps. This corresponds to an energy resolution of 0.1% or better.

It is not necessary to have a bunched beam for energy determination. A fraction of a percent modulation by a simple sine-wave buncher should be adequate for measuring beam energy by transit time between spaced detectors. It is also possible that the noise structure of a normal d.c. beam will yield a measurement of beam energy through cross-correlation techniques between the signal outputs of spaced detectors.

Simple resonators along beam lines can serve as nondestructive beam monitors for slightly modulated beams. The total equipment required is a simple buncher, a resonator, an inexpensive radio receiver and a meter. Since signal strength is directly proportional to beam intensity, the method provides a ready indication of beam loss through apertures, etc. This measurement in no way affects beam quality.

6. A note on the ANL buncher

The ANL pre-tandem buncher is a single-gridded accelerating gap with aligned grids. It is excited by a sawtooth-like waveform obtained by combining the first four harmonics of the beam bunching frequency, 48.5 MHz. Use of four harmonics enables us to bunch a large fraction (> 70%) of the d.c. beam from the ion source. (Only 31% of the d.c. beam can be bunched when bunching with a single sine wave.)

The interested reader may wish to consult reference 1 which discusses the theory of bunching and provides a complete description of the ANL four-harmonic buncher.

APPENDIX

Some Details of the Electronic Circuits

Fig. 8 is a schematic of the electronics unit. A 100 kHz frequency is counted down to 1000 Hz and provides a square wave to operate the phase flipper and the synchronous detector. A mono-stable with variable pulse width controls the input gate to the synchronous detector. A noise clipper at the input clips large noise spikes and reduces electrical interference

problems. The beam-phase error can be observed on the meter and on an oscilloscope connected to the monitor output. With closed-loop operation, the "beam reference phase" control is adjusted so the phase error meter has an average reading of zero. Then the bandwidth and gain controls are adjusted for minimum phase error as observed on an oscilloscope connected to the "receiver output" jack. Phase error shows up as 1000 Hz chopping of the receiver output signal.

For the initial tests a commercial radio receiver was used; a Communications Electronics, Inc. (CEI) Model 960 with an associated signal monitor type SM-9310. (CEI is now a part of Watkins Johnson Co., 700 Quince Road, Gaithersburg, Md., 20760.) This equipment is general-purpose, general coverage and is quite expensive (something like $8000.00!). Fixed-tuned special-purpose receivers can be constructed at a substantially lower cost.

Since our bunching frequency is 48.5 MHz, our preamplifier and receiver designs were based upon the use of integrated circuits designed for color television intermediate frequency circuits that are optimized to operate near this frequency. The ones we use are the Motorola MC 1350 I.F. Amplifier and the MC 1330 Low-Level Detector. Fig. 9 shows how a special-purpose receiver can be constructed using these devices.

The preamplifier uses an MC 1350. Total parts cost is about $15.00. The circuit is shown in Fig. 10. Best S/N operation results when the gain control is adjusted for maximum gain.

The "receiver" amplifies the signal with an MC 1350 and detects it with an MC 1330. The output is fed to a trip circuit that provides logic-level signals indicating the presence or absence of the beam. The signal is also displayed on a "beam-current" meter with full-scale readings ranging from

1 na to 1000 na. This provides a non-destructive non-interacting measurement of beam current.

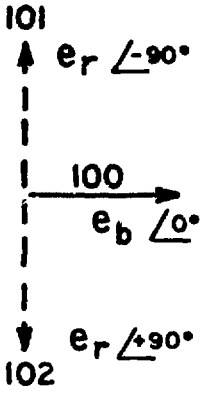
The r.f. reference signal will cause a false indication of beam current. This can be nulled out by a front-panel screwdriver adjustment marked "ref null." The "receiver output" signal is applied to a synchronous detector along with the 1000 Hz square wave. The resulting output signal controls the phase of the buncher.

A circuit diagram of the receiver is shown in Fig. 11. Total parts cost is estimated at less than $200.

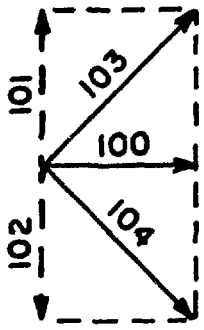
Design information on the helix structure can be obtained from L. Bollinger, T. Wangler or W. F. Henning, all of the Physics Division, ANL[4].

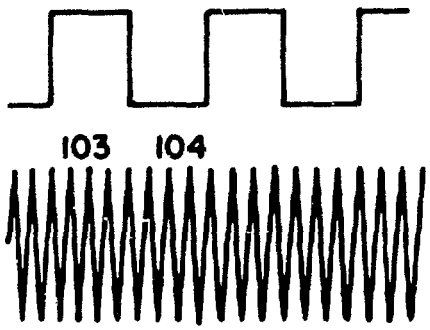
References

1. F. J. Lynch, R. N. Lewis, L. M. Bollinger, W. F. Henning and O. D. Despe, "Beam Buncher for Heavy Ions," accepted for publication in Nuclear Instruments and Methods.
2. L. M. Bollinger, T. R. Khoe, F. J. Lynch, B. Zeidman, R. Benaroya, J. J. Bicek, Jr., B. E. Clifft, A. H. Jaffey, K. W. Johnson, J. M. Nixon and W. A. Wesolowski, IEEE Trans. Nucl. Sci., NS-22 (1975) 1148.
3. R. N. Lewis, "Phase Measurement and Control of Pulsed Charged Beams," Nucl. Inst. Methods, 151 (1978) 371.
4. Details of the electrical design of helical lines and resonators may be found in the literature. See, for example, "Reference Data for Radio Engineers," pp 24-27 to 24-30, Howard W. Sams, Inc., 1975.

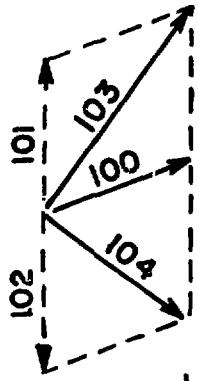


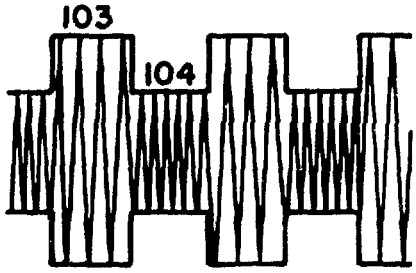
1(a)



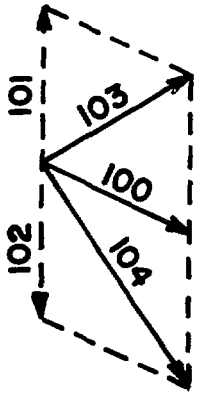


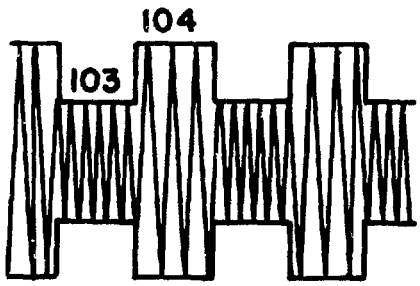
1 (b) BEAM PHASE OK





1 (c) BEAM PHASE EARLY





1 (d) BEAM PHASE LATE

FIG. 1

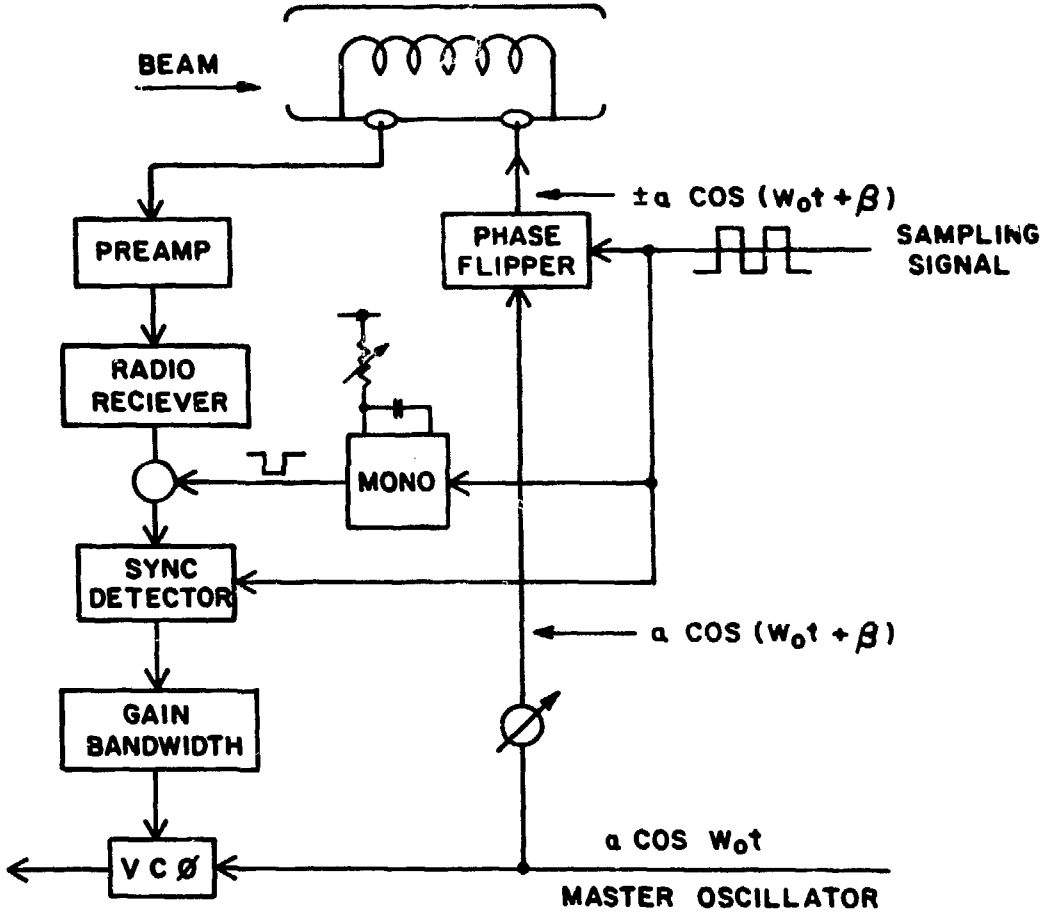

BEAM
±a COS (W0t + β)
PREAMP
PHASE FLIPPER
SAMPLING SIGNAL
RADIO RECIEVER
MONO
SYNC DETECTOR
a COS (W0t + β)
GAIN BANDWIDTH
V C Ø
a COS W0t
MASTER OSCILLATOR

FIG. 2

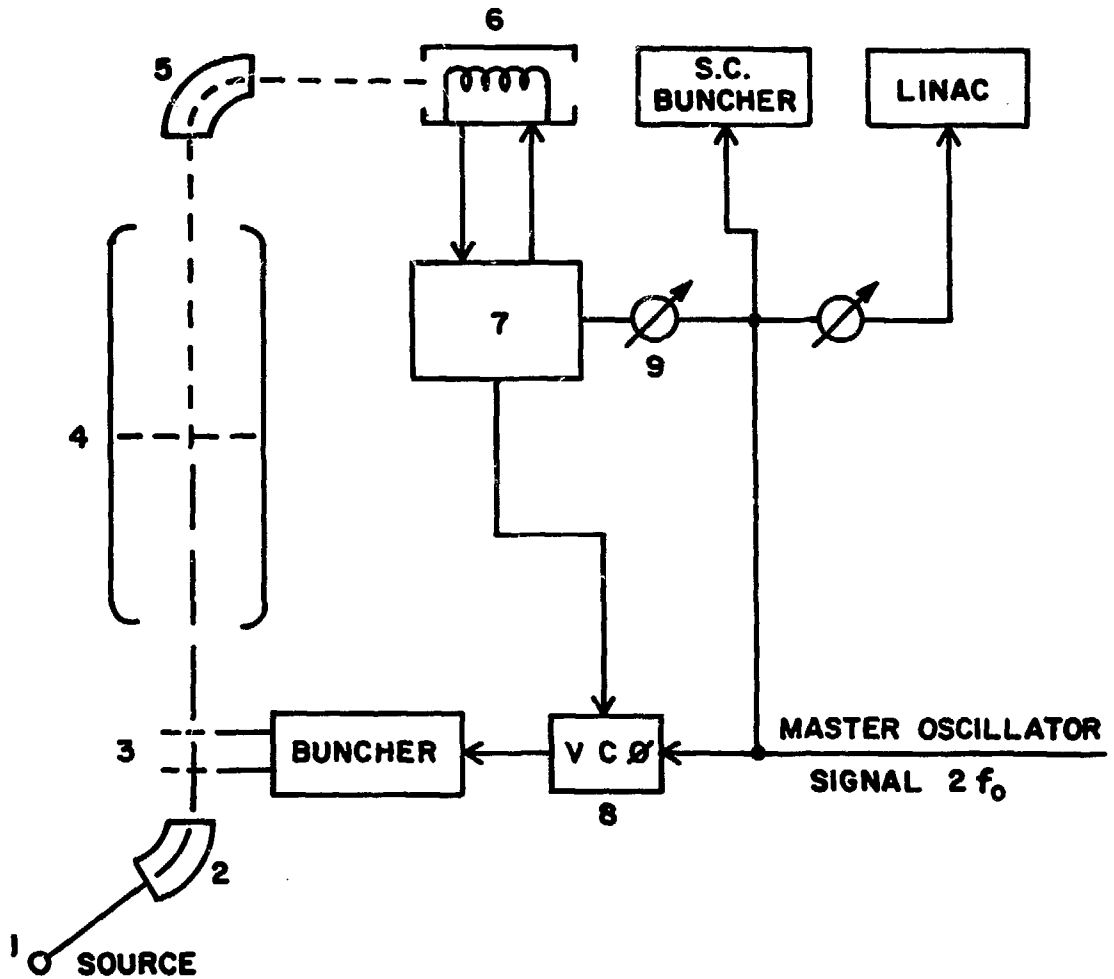

6
5
S.C.
BUNCHER
LINAC
7
9
4
3
BUNCHER
V C Ø
8
MASTER OSCILLATOR
SIGNAL 2 f_o
2
1
SOURCE

FIG. 3

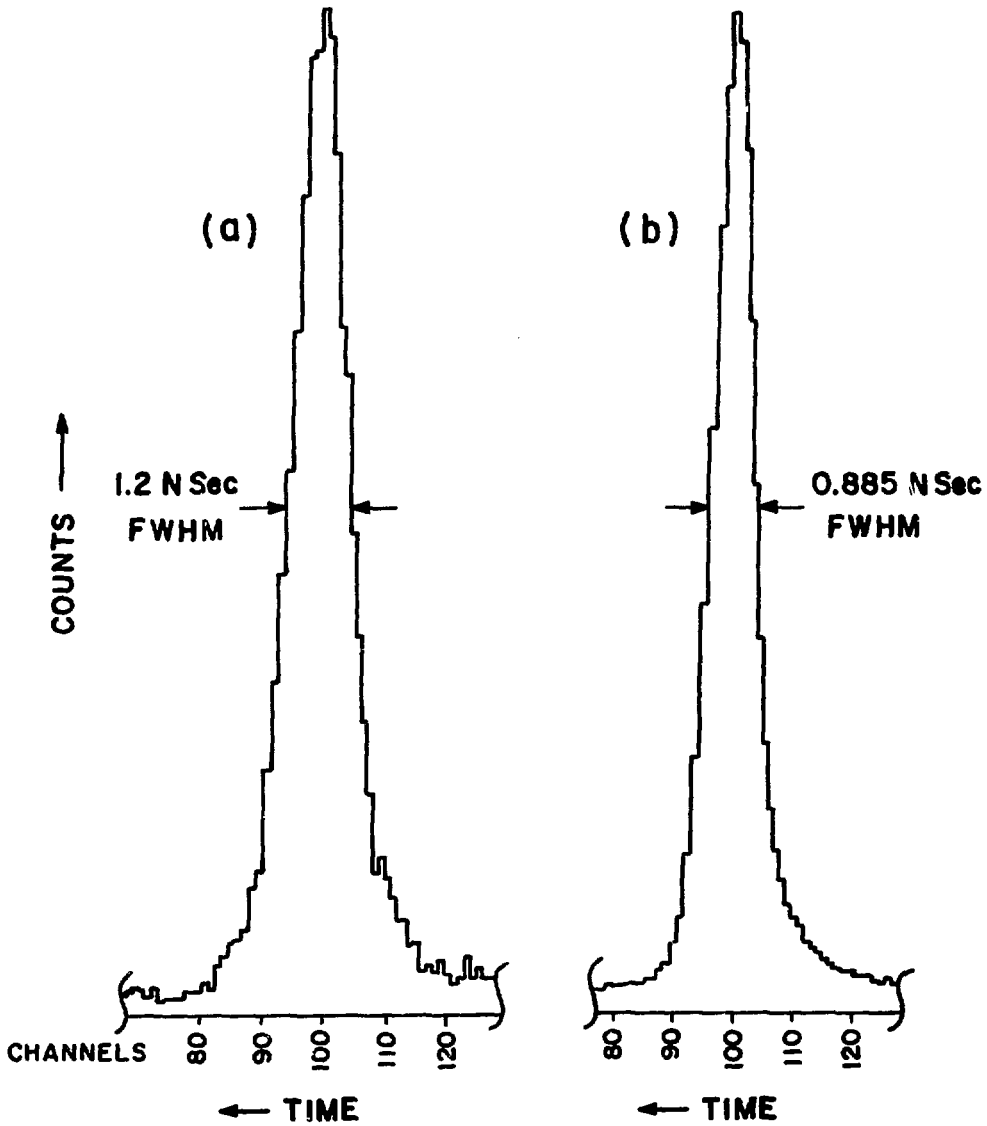
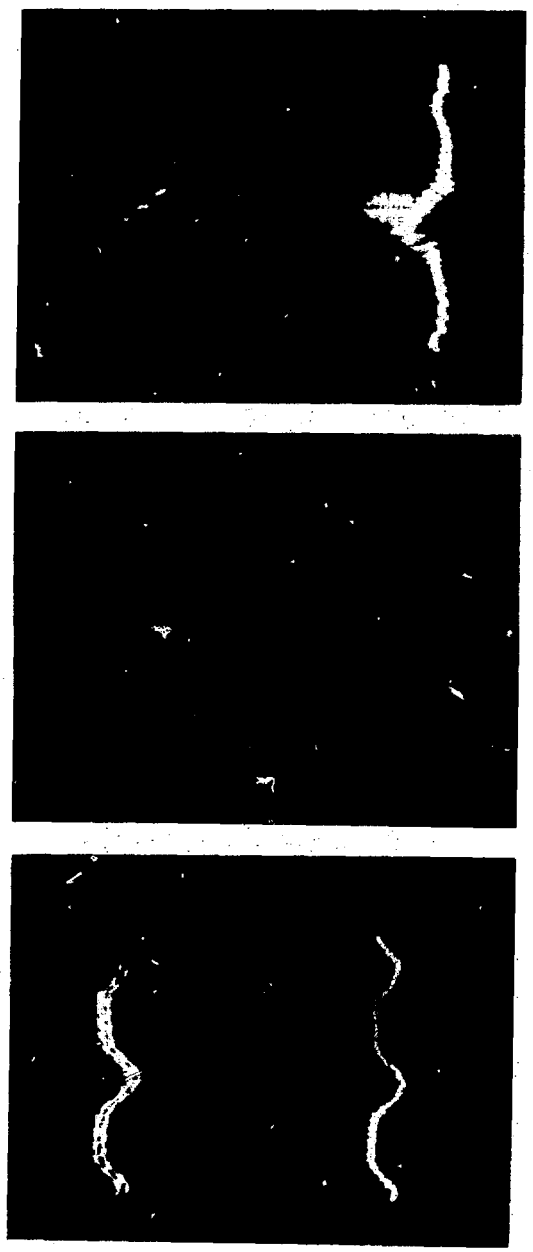

(a)
(b)
1.2 N Sec
FWHM
0.885 N Sec
FWHM
COUNTS
CHANNELS
80
90
100
110
120
TIME
TIME

FIG. 4



a b c

FIG. 5

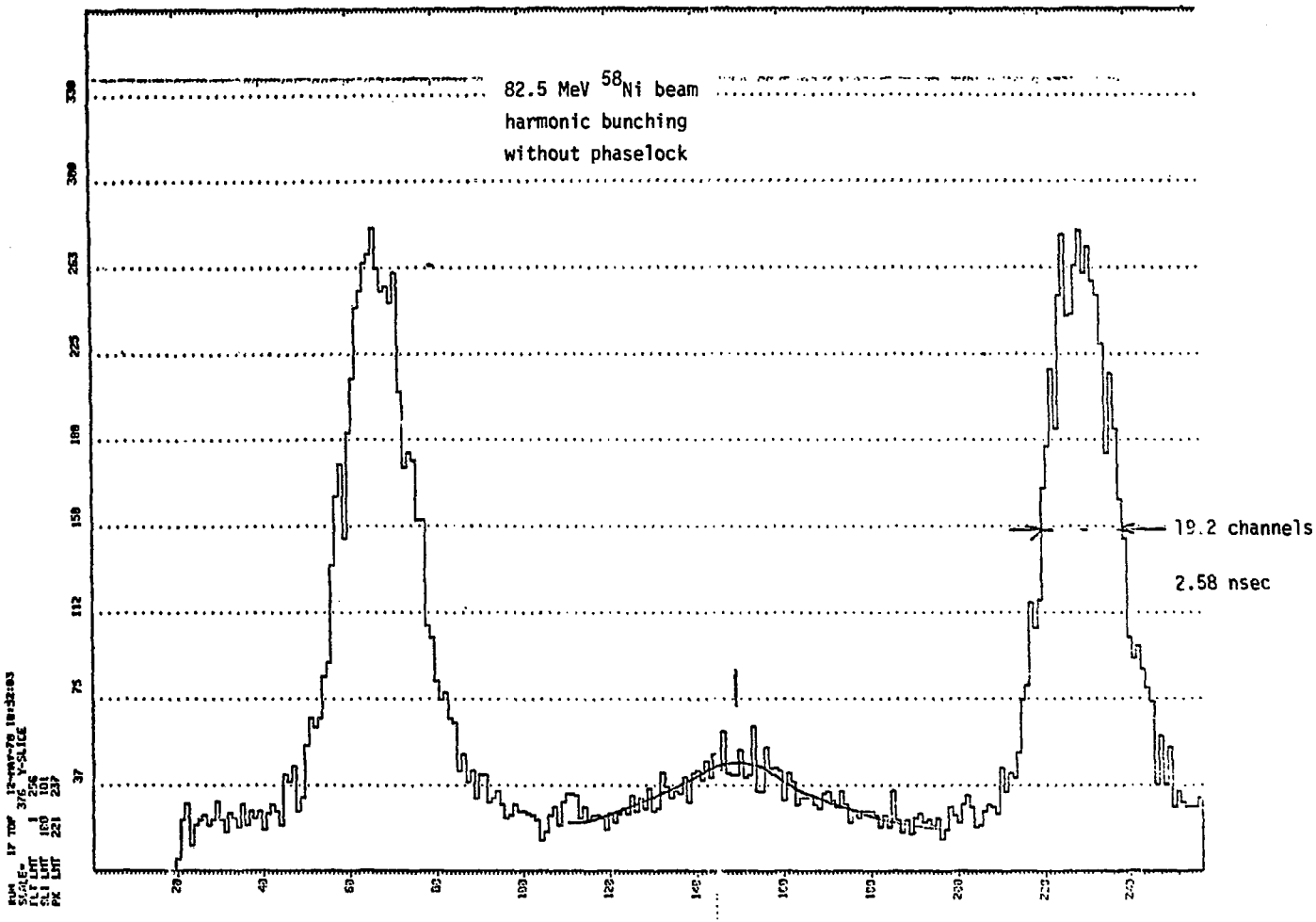

82.5 MeV ^{58}Ni beam
harmonic bunching
without phaselock
19.2 channels
2.58 nsec

FIG. 6

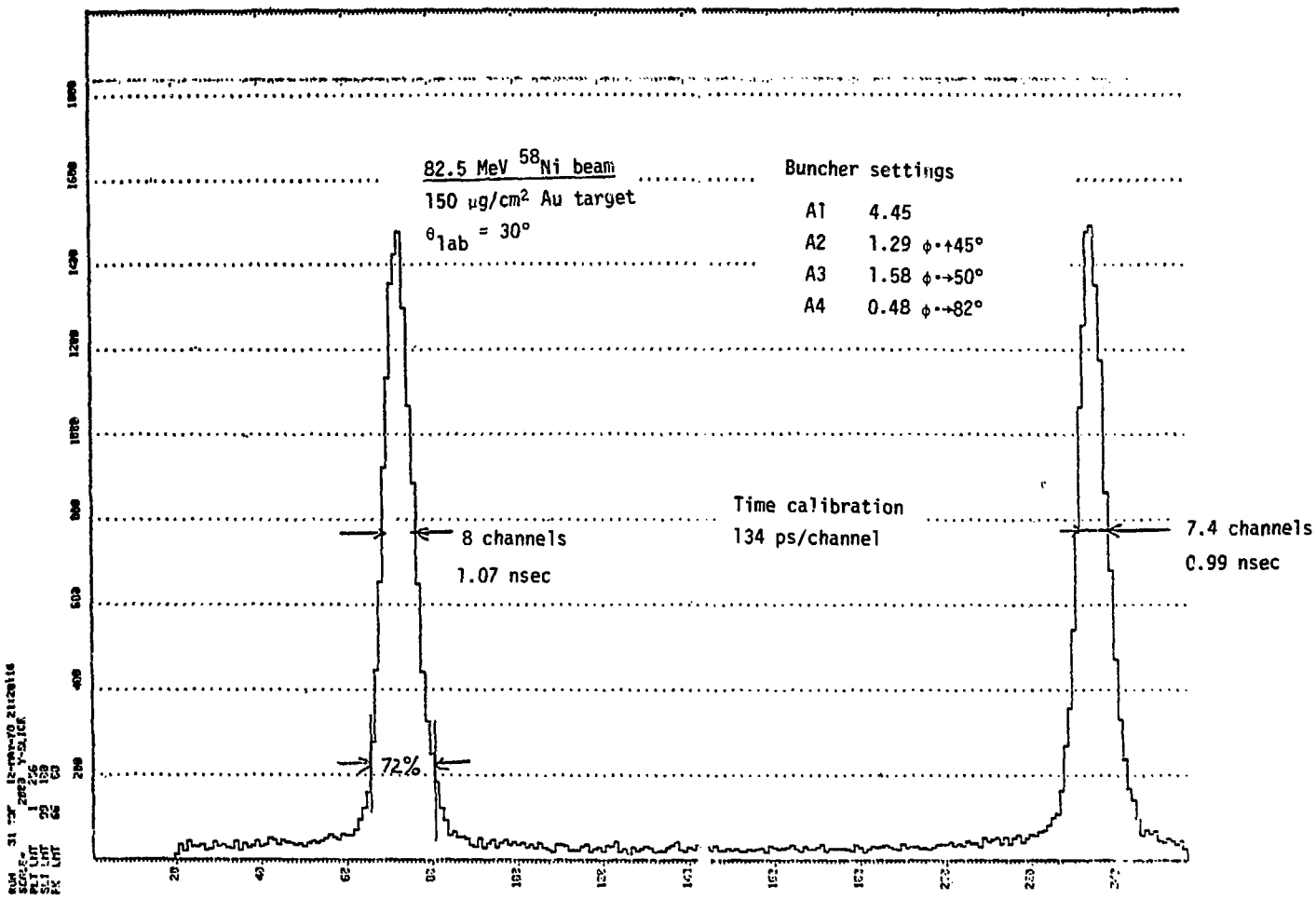

82.5 MeV 58Ni beam
150 μg/cm2 Au target
θlab = 30°
Buncher settings
A1 4.45
A2 1.29 φ·↑45°
A3 1.58 φ·→50°
A4 0.48 φ·→82°
8 channels
1.07 nsec
72%
Time calibration
134 ps/channel
7.4 channels
0.99 nsec

FIG. 7

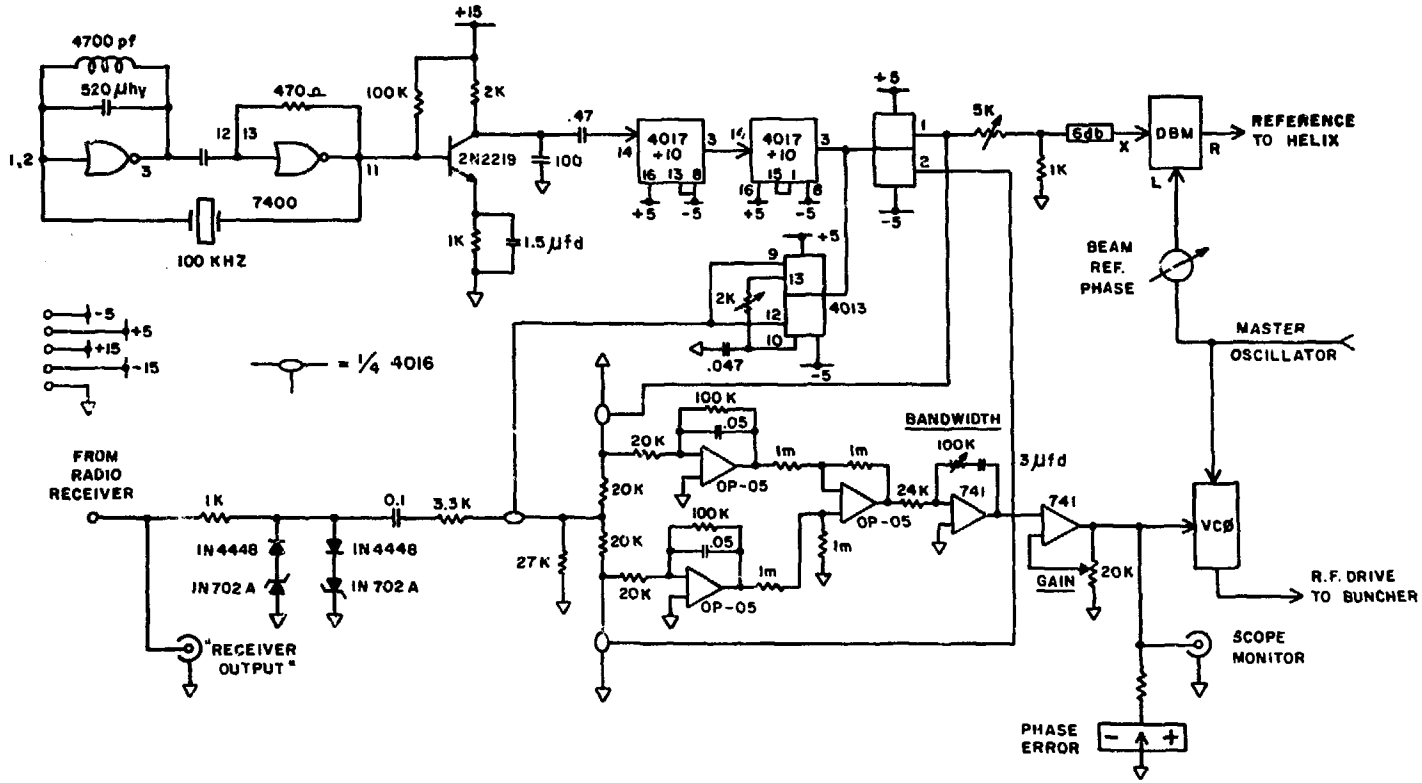

4700 pf
520 μhy
470 Ω
100 K
2K
+15
.47
100
2N2219
1K
1.5 μfd
100 KHZ
7400
4017 ÷10
4017 ÷10
+5
-5
5K
5db
DBM
REFERENCE TO HELIX
BEAM REF. PHASE
MASTER OSCILLATOR
4013
.047
= ¼ 4016
FROM RADIO RECEIVER
1K
0.1
3.3K
IN4448
IN702A
"RECEIVER OUTPUT"
27K
20K
.05
1m
OP-05
BANDWIDTH
100K
3 μfd
24K
741
GAIN
VCØ
R.F. DRIVE TO BUNCHER
SCOPE MONITOR
PHASE ERROR

FIG. 8

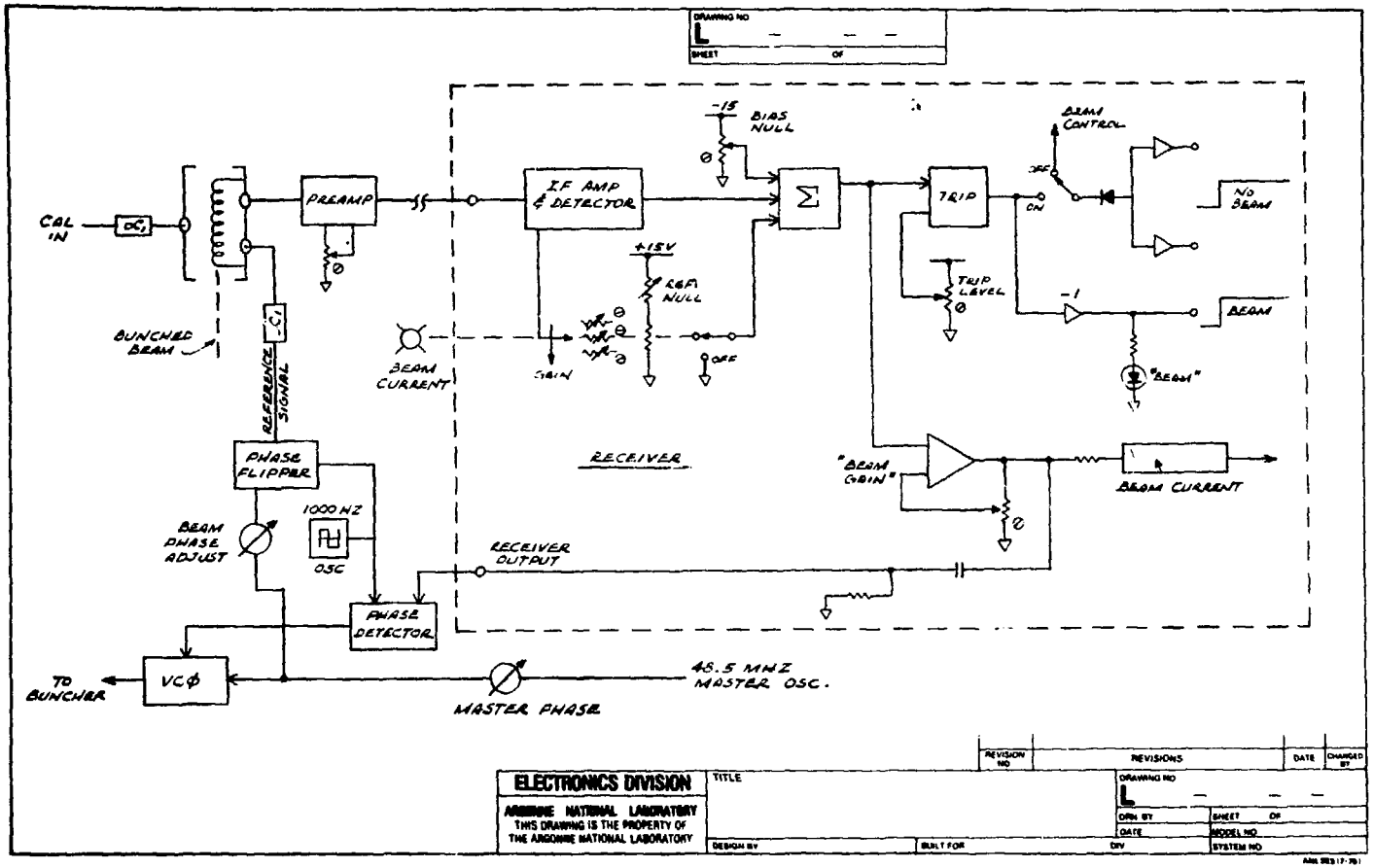
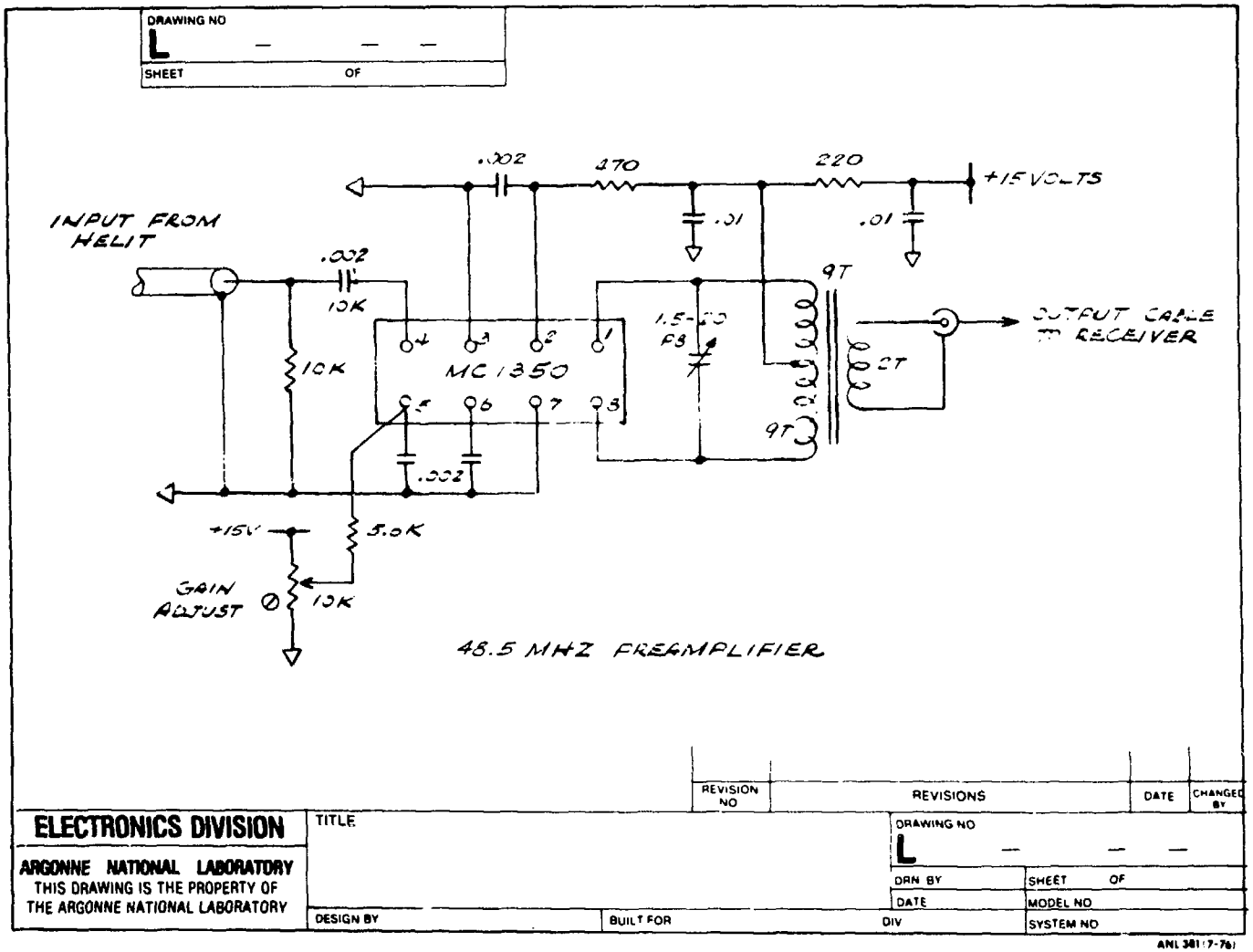

DRAWING NO
L
SHEET
OF
CAL IN
DC
PREAMP
I.F. AMP & DETECTOR
-15
BIAS NULL
Σ
TRIP
BEAM CONTROL
OFF
ON
NO BEAM
BEAM
+15V
REF. NULL
TRIP LEVEL
-1
"BEAM"
BUNCHED BEAM
C.T.
REFERENCE SIGNAL
BEAM CURRENT
SIGN
OFF
RECEIVER
"BEAM GAIN"
BEAM CURRENT
PHASE FLIPPER
1000 HZ
OSC
BEAM PHASE ADJUST
RECEIVER OUTPUT
PHASE DETECTOR
TO BUNCHER
VCΦ
45.5 MHZ MASTER OSC.
MASTER PHASE
REVISION NO
REVISIONS
DATE
CHANGED BY
ELECTRONICS DIVISION
ARGONNE NATIONAL LABORATORY
THIS DRAWING IS THE PROPERTY OF THE ARGONNE NATIONAL LABORATORY
TITLE
DRAWING NO
L
DRN. BY
SHEET
OF
DATE
MODEL NO
DESIGN BY
BUILT FOR
DIV
SYSTEM NO

FIG. 9


DRAWING NO
L
SHEET
OF
.002
470
220
+15 VOLTS
.01
.01
INPUT FROM HELIT
.002
10K
9T
1.5-20 PF
OUTPUT CABLE TO RECEIVER
2T
10K
MC1350
9T
.002
+15V
5.0K
GAIN ADJUST
10K
48.5 MHZ PREAMPLIFIER
REVISION NO
REVISIONS
DATE
CHANGED BY
ELECTRONICS DIVISION
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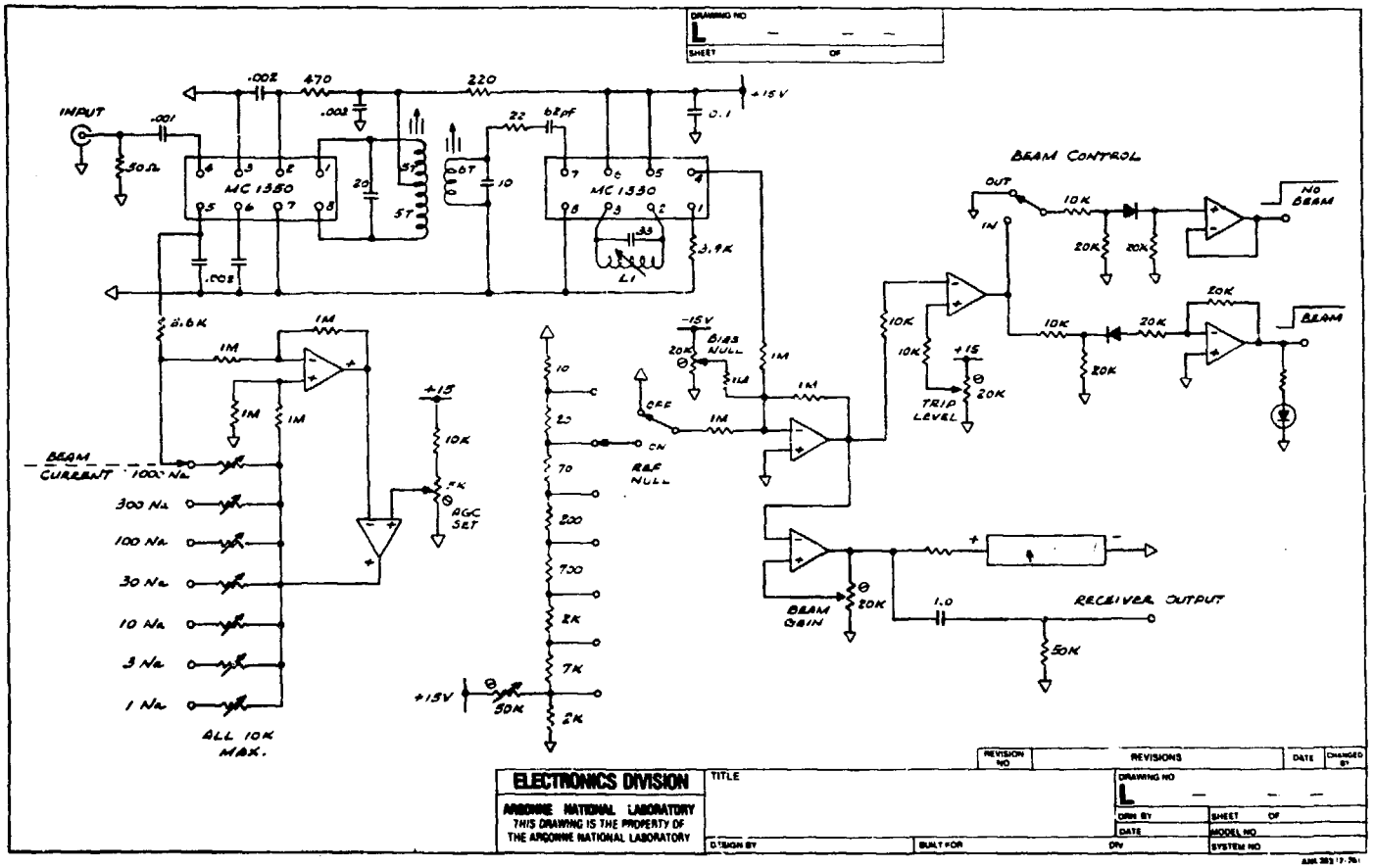
SYSTEM NO
ANL 381 (7-76)

FIG. 10


INPUT
.001
50Ω
.002
470
.003
220
+15V
0.1
22
62pF
MC 1550
20
5T
6T
10
MC 1550
33
L1
3.9K
.005
2.6K
1M
1M
1M
1M
BEAM CURRENT
100 Na
300 Na
100 Na
30 Na
10 Na
3 Na
1 Na
ALL 10K MAX.
+15
10K
5K
AGC SET
10
20
70
200
700
2K
7K
2K
+15V
50K
-15V
20K
BIAS NULL
1M
OFF
ON
R&R NULL
1M
1M
BEAM CONTROL
OUT
IN
10K
20K
20K
NO BEAM
10K
10K
+15
TRIP LEVEL
20K
10K
20K
20K
20K
BEAM
20K
BEAM GAIN
20K
1.0
50K
RECEIVER OUTPUT
ELECTRONICS DIVISION
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TITLE
REVISIONS
DRAWING NO
L
SHEET
OF
MODEL NO
SYSTEM NO

FIG. 11

Session IX, Chairman, J. A. Biggerstaff
Session Editor: R. C. Juras

EXPERIENCE WITH THE VICKSI-CONTROL-SYSTEM

VICKSI-Group, presented by B. Spellmeyer
Hahn-Meitner-Institut für Kernforschung Berlin GmbH
Bereich Kern- und Strahlenphysik
Berlin, Germany

Introduction

The control system of the VICKSI-accelerator facility is based on the following features

- standarized hardware which is commercially available
- peripheral interfaceing standards considering hardware and software aspects
- comprehensive operator interface which is easy to use
- man-machine communication routed through the computer
- system information held in a central data base
- accelerator parameters identified by names
- interpreter for interactive and flexible system use.

The present state of the system allows full control and monitoring of the whole accelerator and incorporates about 2000 system variables. In the following a short description of the hardware and software of the control system and of the telemetry system for the Van de Graaff injector is given.

Hardware

The basic aim of the control system is to enable the operator to perform the following simple operations in a convenient and reliable way with the help of the computer:

- Data Acquisition (READ parameter values)
  (READ parameter status)
- Data Command (WRITE/SET parameters)
  (EXECUTE a status change)

i. e. the power of the accelerator is to a large extend used for multiplexing digital information between the control room and accelerator components. Fig. 1 shows a schematic view of the VICKSI-control system hardware.

A PDP-11/40 with 128 K of core memory is used as a control computer. A second PDP-11/40 used mainly for software development is also implemented as a back up computer. The peripheral instrumentation installed for both computers is shown in fig. 2.

The operator main console is connected to the computer by a Parallel CAMAC Highway. The accelerator and beam line equipment is interfaced to the computer via two Byte-Serial CAMAC loops which cover a distance of about 350 m and drive a total of 50 CAMAC crates. Bypass units are installed with each crate to provide isolation between crates, signal fresh-up and signal bypass in case of a faulty serial crate. The layout of the serial system is laid out to allow an operating speed of 2.5 M Bytes/sec at a maximum distance of about 90 m between two bypass units.

The monitoring and control units which interface a great variety of different accelerator components were specified in such a way as to minimize the number of different module types. The system has only 7 different modules. Therefore a stringent standardization during the design of the different accelerator components was necessary, which in turn eleviated the software design.

A further advantage of the module to accelerator component correlation is in the area of trouble shooting and maintenance. Before it is connected to the control system every component is tested by a module simulator and the module itself can

be tested on line by a test module connected to the CAMAC module.

Software

The data bank feature known from other control systems has been adopted. All relevant specifications of the different parameters are aggregated into a Data Base (e. g. hardware, CAMAC-interface, acquisition or control values, engineering units, name of conversion routines, etc.). Once this information has entered the Data Base the user need only concern himself with the name of that parameter, which is called a System Variable of the control system. The naming convention of the System Variables is shown in fig. 3. Control and acquisition of parameters will then result in WRITE and READ to and from System Variables, i. e. application programs call the respective interface routine for all control and acquisition operations of the given parameter. This routine handles the necessary Data Base operations and will induce the appropriate action.

In the following a summary of the basic software packages is given.:

Resident CAMAC Library:

- CAMAC Interface subroutine which operates all CAMAC transfers and which is transparent to the user (calling program) to whether a crate is installed on the parallel branch or in any of the serial loops. Crate addresses may range from 1 to 62; they must be unique.
- CAMAC interrupt service routines
- Crate status tables including initialization status and installation position

Resident SYSVAR Library:

- Data Base and Process to System Variable interface routine. It searches the Data Base testing whether a given name is a System Variable, if yes, it determines whether type and direction (WRITE/READ) are permitted combinations and will on

READ acquire the appropriate CAMAC register, pass control to the conversion routine given in the Data Base and finally output the "acquired physical value" and on

WRITE pass control to the appropriate equipment driving routine which converts the physical input to CAMAC register output and initiates the CAMAC transfer

- Equipment driving routines, which reflect the type of equipment and the type of interface module, if this applies.

The application programs presently available allow full control and monitoring of the accelerator facility through the existing man machine interface. Beside these a number of service packages for control system internal use allow the management of all the necessary back up files.

Man Machine Communication

The man machine communication for the VICKSI control system is done in two ways. The first way uses the operators console relying on the available program packages, the second uses the MUMTI interpreter.

Main Control Desk

The control desk represents by itself a multi user "parallel access" man machine interface.

Each bay has only a few standard devices such as displays (BW and Colour) touch panels, knobs, while others contain special equipment dedicated to special accelerator components like beam monitoring, analogue meters etc.

A view of the control desk is given in fig. 4 and 5. In the bay at the left of the control desk some analogue instruments are located which display parameters of the injector stabilization system. These signals bypass the computer for a quick operator access. Also belt charge and drive motor switch offs are installed. During the running of the injector these instruments were found to be very helpfull for a proper setting of the injectors stabilization parameters. The next bay is identical to the fifth bay. It incorporates a colour TV screen for the digital or quasi analogue presentation of parameters, a touch panel and two knobs for the setting of parameters. The next bay is mainly used for the read out and setting of the beam line system (e.g. switching and current measurement of Faraday cups, beam profil monitors etc.). The fifth bay provides a graphic display which is used for the setting of the cyclotron parameters which is mainly done from the last bay but one.

The touch panels consist of a BW-monitor covered by a screen with 16 touch sensitive areas for parameter selection, program request, program-option request, device-to-knob assignment and device-to-display assignment. Above the BW-screen seven hard wired push buttons ollow to go up and down, forward and backward at the same level in the tree-like ordered pages displayed on the touch panel. The functional use of the touch panel can be seen in fig. 6 where the different pages to go through in order to start the drive motor of the injector are shown (the whole procedure takes about 5 - 9 sec.).

The knobs are provided for the variation of control variables once the assignment has been made. Their alphanumeric display areas display the variables name, the present setting and the value of a corresponding acquisition variable including their engineering units.

The usage of the general purpose colour-TV-monitors is explained by fig. 7. The upper third of the screen is used for fixed purpose, such as display of experiment and date. The string of symbols are set by a surveillance program indicating whether

all beam stops along the beam line sections A, B, C,... allow the beam to pass (green colour) or whether they are set in the beam pass by the operator (yellow colour) or an interlock (red colour). Parameters may be displayed in a digital (cf. left half) or a quasi analogical way (cf. right half).

After a minimum of training and without previous computer knowledge an operator can select parameters and adjust these in a similar manner as it is done in the classical "one-parameter-one-knob" control system.

MUMTI Interpreter

A Multi User Multi Task Interpreter was implemented into the control system to eleviate running in, hardware testing and study of the accelerator system.

Some properties of the interpreter are given

- 3 integer denotations (binary, octal, decimal)
- logical and shift operators
- powerful IF-statement
- linkage to external FORTRAN or MACRO subroutines
- indirect and immediate mode command lines
- periodical execution of parts of the program (HOOK statement)
- compile command

The interpretive mode of working through MUMTI makes work much quicker and more satisfying. Small programs can be written within a short time and existing programs can be adapted to new requirements. For example the setting of the whole beam line system and of the cyclotron parameters are done by MUMTI-programs.
Higher level application programs such as beam isochronization or centralizing in the cyclotron have been written in MUMTI. Presently the lack of more sophisticated surveillance programs is felt to be a bottle neck in operation.

Telemetry System of the Injector

Part of the whole VICKSI control system is the telemetry system which controls and sets the parameters at the high voltage terminal of the injector. Fig. 8 shows a layout of this system. All terminal data are collected and stored in memories at ground and terminal potentials.Data transfer is provided by two infrared data links. A third infrared line is used for a clock signal to check the data transfer. The light links consist of a light emitting diode (center wave length 900 nm) and a photo diode as a receiver. The transfer is done with a bit rate of 1 MHz, the total time for a memory transfer is about 0.76 ms upwards and 1.5 ms downwards.
Within the terminal the respective memory sections are transfered to subsections at higher potential decks by fiber glass optics.

The main data station at the terminal can be seen in fig. 9 at the lower left hand side. Above that a data substation connected to the main data station by fiber glass optics is installed. The diode in the center of fig. 9 is used for the transfer of the buncher signal.

Fig. 10 shows three cabinets where the control units of the injector are installed. The CAMAC crates in the left hand cabinet control the power supplies and control units which are installed in the other two cabinets. Above the first cabinet the manual control unit for the terminal can be seen. By this unit the terminal parameters can be set independently from the control computer. This was done sometimes during the running in phase of the injector and the control system and it presently eleviates the service at the injector.
Presently sparking of the injector sometimes causes terminal components to turn off. Damages to the control system components and power supplies due to sparking have been eleminated by the installation of filters at the necessary points.

Experience

The down time of the control system is extremely low (~ 2 % of the machine down time). The high flexibility of the computer control system has proven to be a major advantage during the running in period of the machine. For normal operation the setting of standard values and automatic optimization using on line routines reduces the set up of the machine considerably.

Discussion:

Rathmell: Approximately how many signals are handled by this system?

Spellmeyer: Well we have installed so far about a little over a thousand variables in the system.

Rathmell: And what's the update rate--how often do you cycle through that number?

Spellmeyer: I can't say. Maybe a few milliseconds or something like that. You have the possibility to install some priority levels into the system, so the updating at the screen is not the same as in the data bank.

Biggerstaff: I'd like to ask or confirm something. You said 2 1/2 megabytes per second. That means you must have a byte parallel transmission. That is, does the serial highway go bit serial or byte serial?

Spellmeyer: Byte.

Biggerstaff: Byte serial and that's fine.

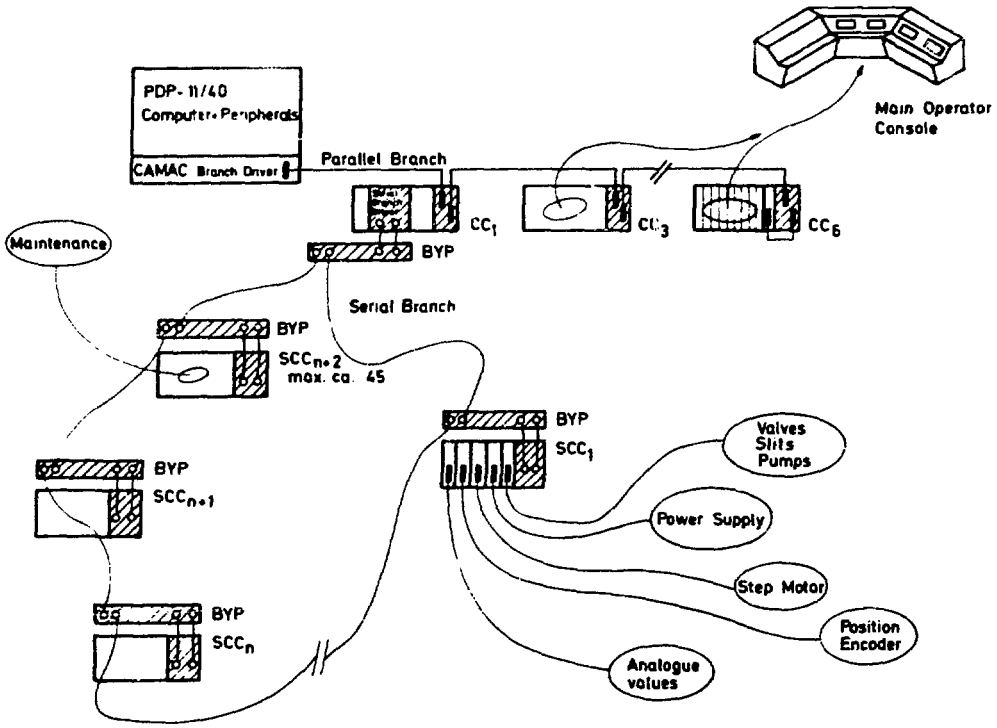


Fig. 1: Schematic View of the VICKSI Control System

HMI/-BERLIN 83 78
VICKSI-E-RA-S-001

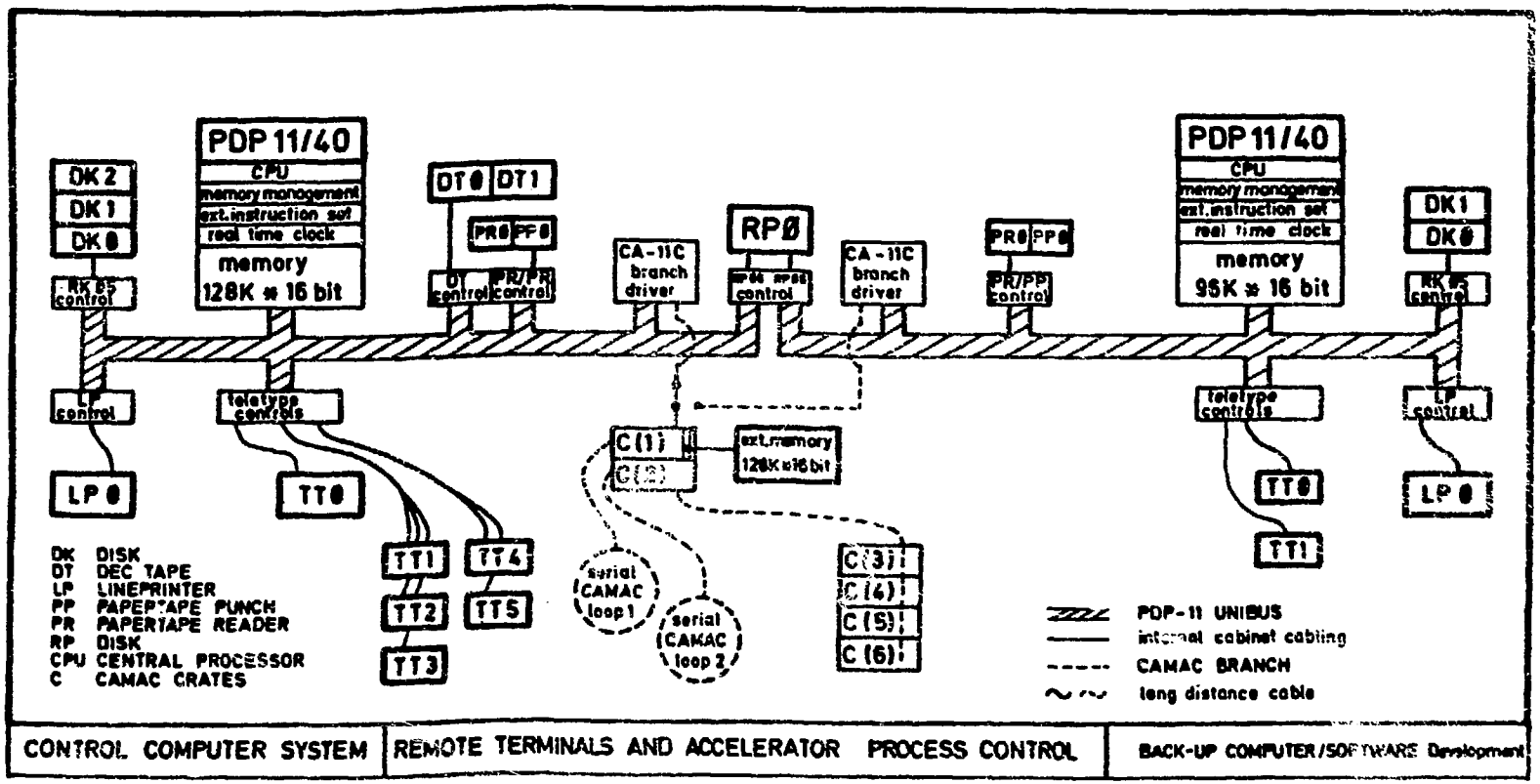


Fig. 2: Layout of the Control Computer.

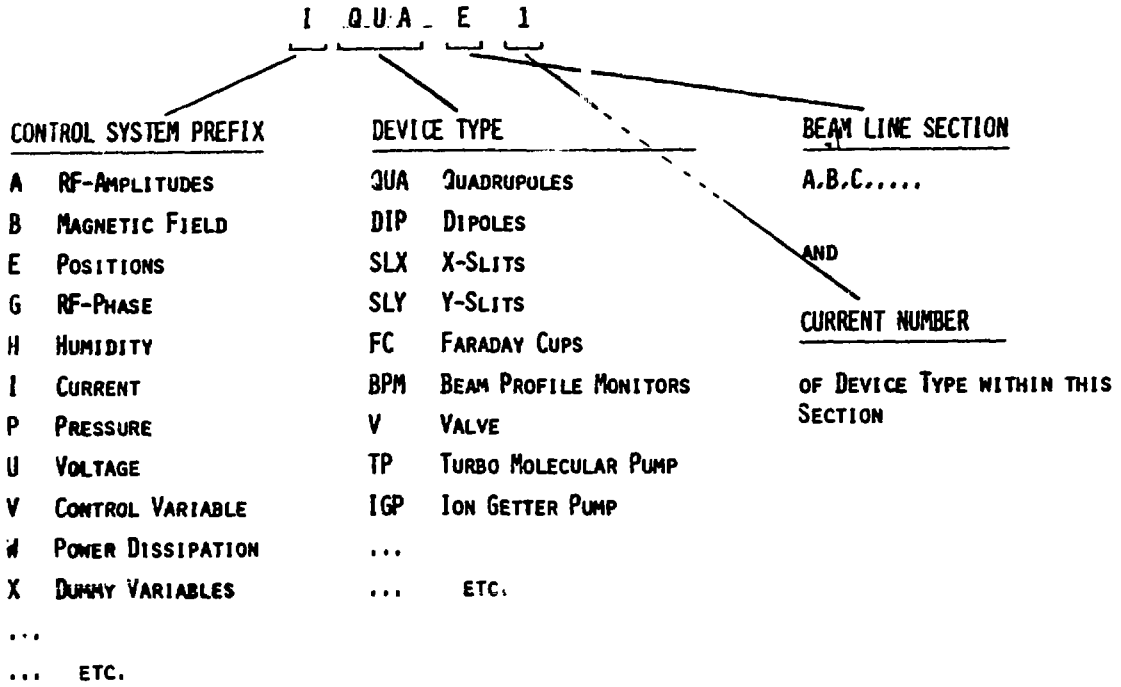


Fig. 3. System Variables naming conventions.

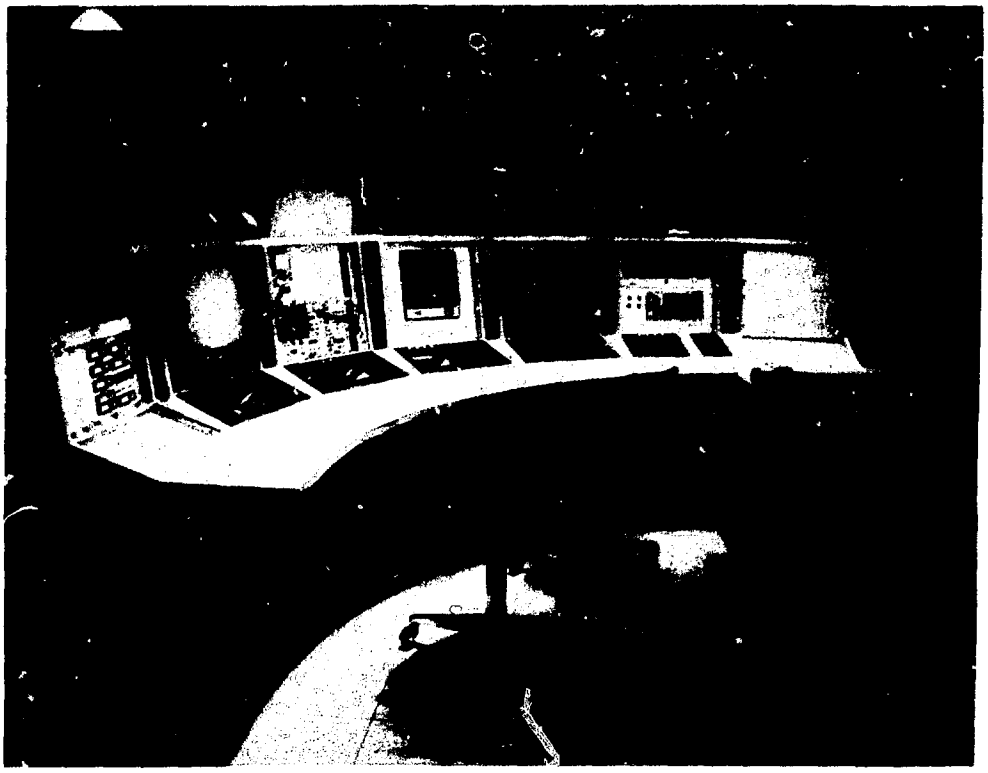


Fig. 4: The main control desk.

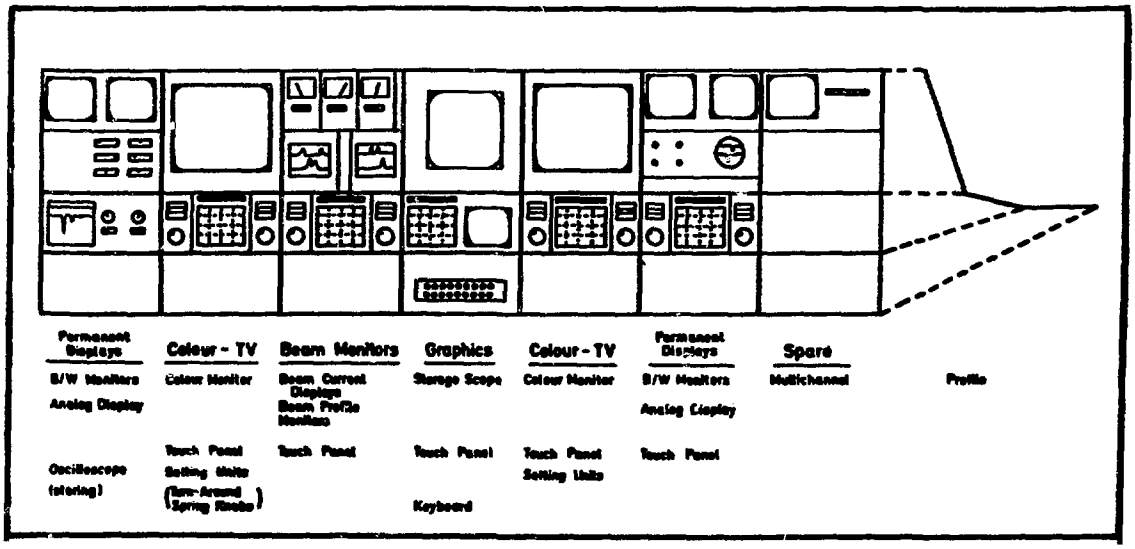


Fig. 5: Schematic view of the control desk.

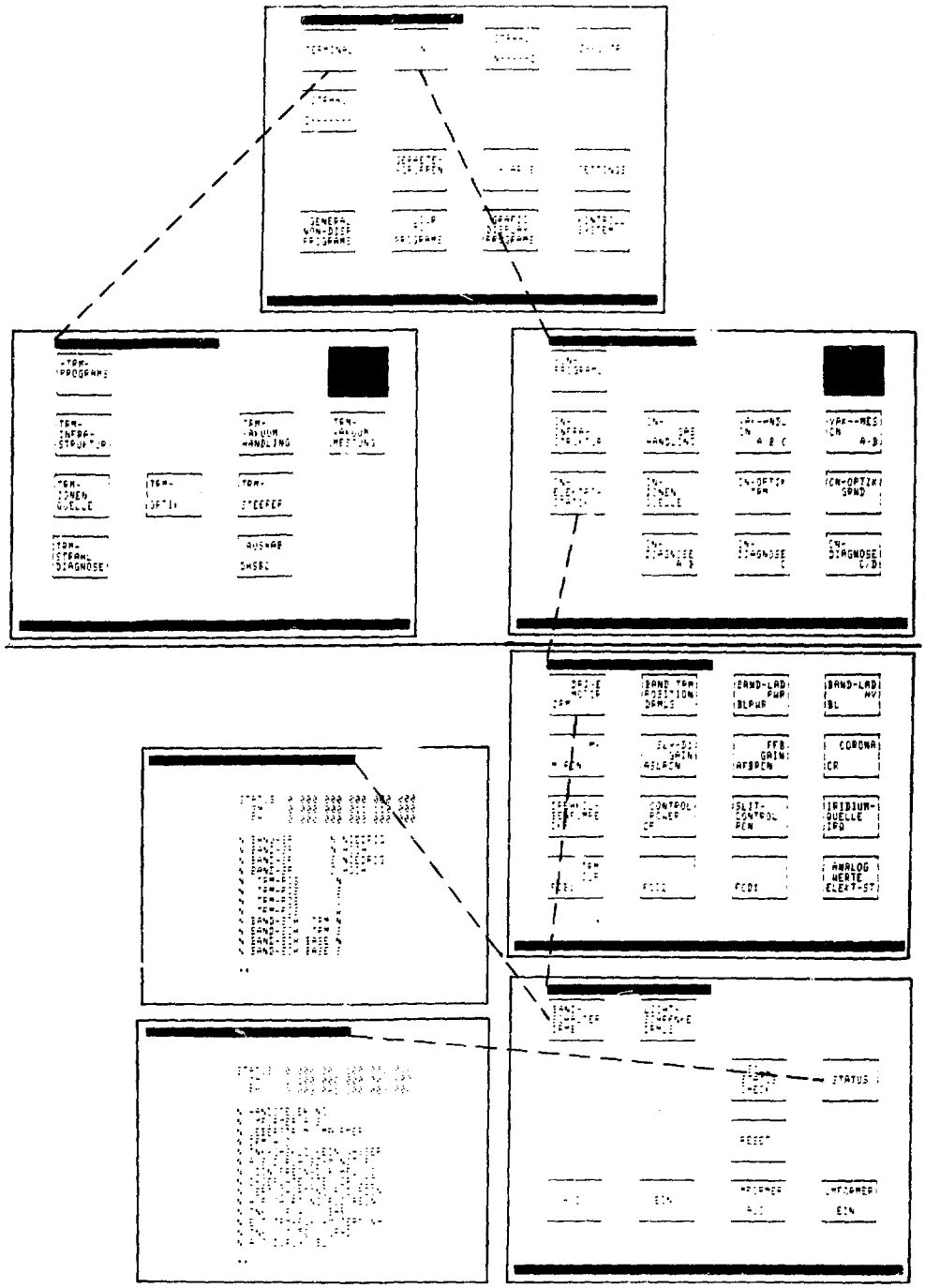


Fig. 6: Touch panel pages used to start the drive motor of the injector.

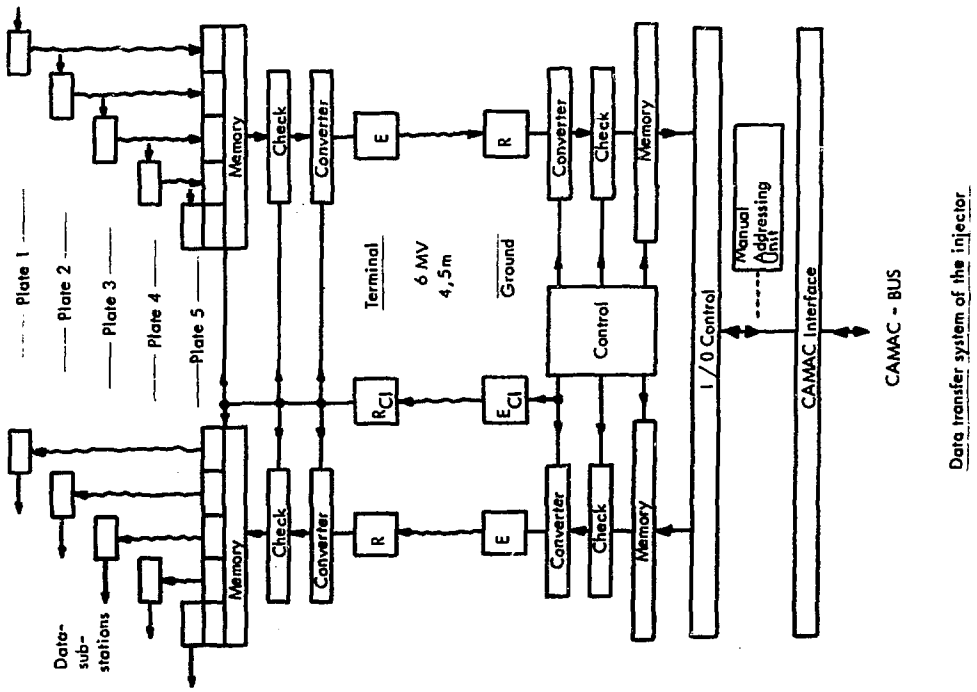
12 C 1+/ 3+ 60 MeV auf TB 23-NOV-78 11:13:38

DAMe BMe3432+1 CMe33+33+ DMe*25+23) EMe-23 FMe*33 GMe2+ HMe3*23)
ZeINMSE3 JMe+33+ K35

ISLXB4M	0.001	µA
ISLXB4P	0.000	µA
ISLYB4M	0.000	µA
ISLYB4P	0.493	µA
ISLXC1M	0.000	µA
ISLXC1P	0.009	µA
ISLYC1M	0.000	µA
ISLYC1P	0.121	µA
ISLXC2M	3.024	µA
ISLXC2P	4.454	µA
ISLYC2M	1.310	µA
ISLYC2P	1.845	µA
UACA1	1.05	kV
IACA1	1.27	mA
IFCB1	0.007	µA
UGVM	3.558	MV

PPENA1
+03
-07
mbr

Fig. 7: Display of a Colour-TV-Monitor.



Data transfer system of the injector

Fig. 8: Schematic view of the Data transfer system of the injector.

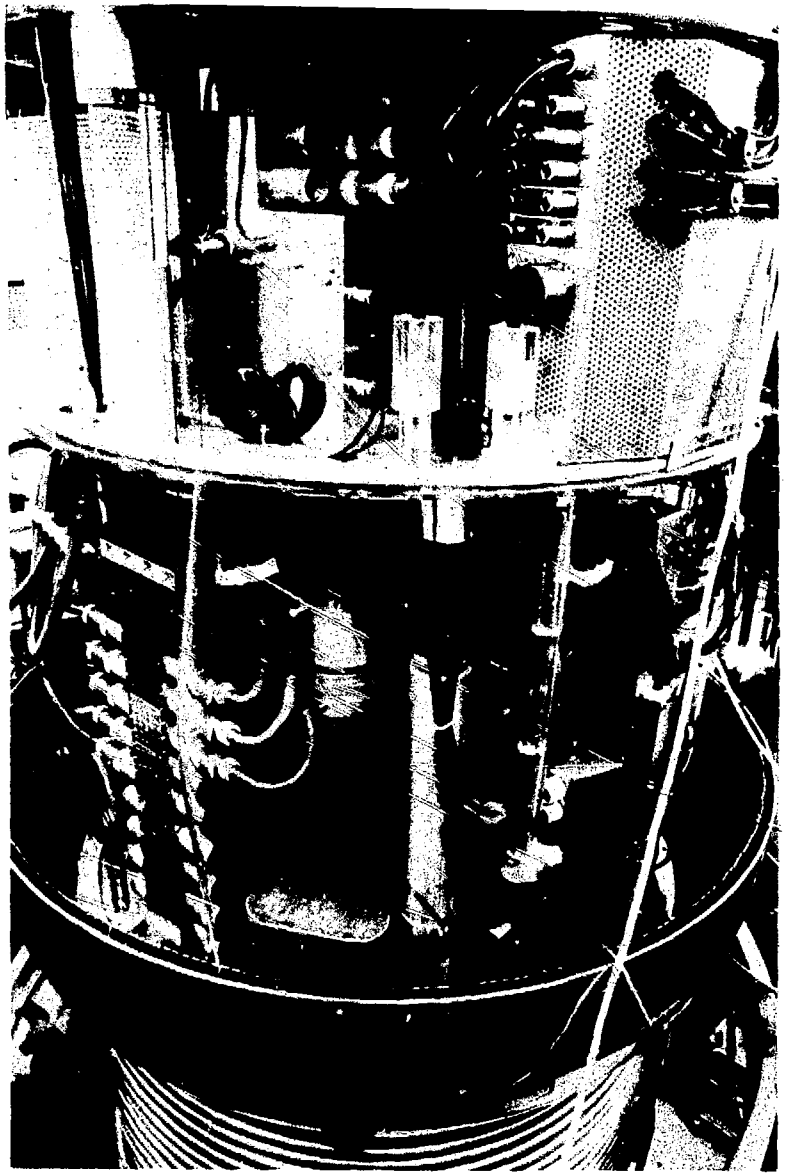


Fig. 9: Main data station at the terminal.

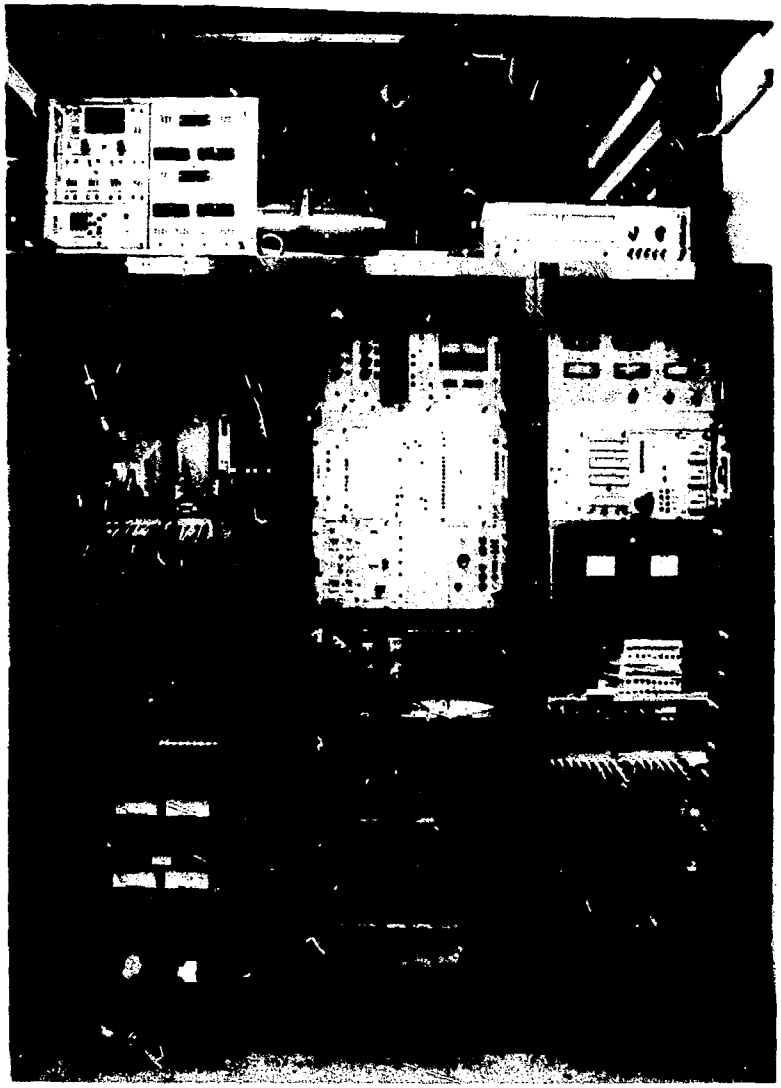


Fig. 10: Cabinets of the control system for the injector.

LIGHT LINKS IN ELECTROSTATIC ACCELERATORS

R. D. Rathmell
National Electrostatics Corporation

Most electrostatic accelerators use mechanical linkages such as strings or control rods for control of parameters in the high voltage terminals. Read-out of parameters from the high voltage terminal is frequently done with a TV camera viewing meters in the terminal. These practices are very reliable and rugged however, they have limitations on space in the column for control rods and space in the terminal for meters. Also, there is limited resolution and response time for control of these parameters.

These limitations can easily be overcome by an optical communication link. Many such links are now being employed in electrostatic accelerators. I have tried to collect information from a number of laboratories using such optical systems, however, I certainly have not included all such installations. I would hope that those listed are representative of the types of systems being used. The first figure shows a table of fiber optic systems. One common sheme used with fibers is the voltage to frequency/frequency to voltage conversion scheme. In this case one fiber is used for each parameter controlled or read out.

The digital multi-plexed scheme uses somewhat more complex electronics to decode device addresses so that a large number of controls can be sent over a single fiber. The Fermi Lab is not an electrostatic accelerator but they have a nice digital communication scheme on their Cockcroft Walton injector.

Most fibers should be able to insulate voltages in the kilovolt range. However, for machines operating in the megovolt range many laboratories have experienced problems with damage to the optical fibers. At these voltages the glass fibers usually do not work because they are fairly complex in construction including reinforcing materials for strength. Plastic fibers such as Lemosa Lumafil or Dupont Crofon have worked better in the high voltage applications. The highest voltages to which fibers have been subjected is 16 million volts in the Munich MP. They run a bundle of fibers from the terminal to ground. The fibers are twisted about each other and tightly clamped in the dead sections to the column. They use the uncoated Lemosa Lumafil. Chalk River has had a fiber in their column for many years,they use the Crofon which is coated with PVC and let the fiber rest on the rings as it passes from the terminal to the ground end. They have not had trouble with spark damage to this fiber even though it has the PVC coating.

The plastic fibers have a fairly high attentuation of about three dB/m for red light, and much greater for infrared light. The attentuation of red light is not a problem, because one is normally dealing with fairly short distances in electrostatic accelerators.

Problems with spark damage of fibers can be eliminated by using direct optical systems. These systems employ a simple lens system to transmit light directly across the insulating gap. Figure two shows representative direct optical systems. Most of these employ a digital multiplexed scheme because it is more difficult to arrange for a large number or parallel direct optical links than it is for a large number of parallel fibers. The distance is somewhat limited to perhaps

the order of 10 or 20 meters, but that is adequate for most present day accelerators. The multiplexed schemes are easily expanded to include additional controls. The time to sample all of these controls depends on the bit rate of the optical link and the total number of parameters. While the Oak Ridge column includes about 300 individual parameters all of these can be updated in less than 10 ms, since the light links operate at 2.5 MB/S. Infrared LED's are normally used with the direct optic systems because of the higher intensity available. Background light has not been reported as a problem even for systems using red light.

One has the choice of transmitting light down the column or radially to windows in the tank wall. For transmission down the column one should be concerned about turbulence in the insulating gas causing refraction of the light particularly when the light is passing near the charging chain or belt. Also, the link which is pointing up can collect dust on its lens which has to be cleaned periodically. For radial links the path is shorter and windows must be planned in advance to permit the ground end of the links to be placed outside the tank. Holes in the spinnings or terminal shells can be located in line with the tank ports and placed in low field regions of the terminals. The lips of these holes are radiused to further minimize fields. Tests at NEC with the original windows ordered for the Oak Ridge tank which were sodalime glass showed that the infrared light was very highly attenuated by that glass. Windows were ordered with borosilicate glass to be used at the light link ports. This type of glass has a very low attinuation, perhaps the order of 15% for the infrared light.

The next figure shows the layout of the serial highway and CAMAC crates for the Oak Ridge control system. You will notice that each crate in the column has an incoming and outgoing light link. The light links and crate can be bypassed externally by CAMAC control. The same light links are used to communicate with the injector.

NEC originally purchased light links for this system. Figure four shows an oscilloscope trace of the serial highway signal which is the input to the optical transmitter. This signal is a biphase encloded signal. In this case a bit frame is 2 centimeters wide. If there is a transition within the bit frame that bit is a "1". If there is not a transition in the bit frame, that bit is a "0". Figure five shows the signal after it has passed through the purchased light link. You can see that the zero bit frame has been extended in length and that the transition in the one bit frame is off center. These asymmetries can cause a zero to be decoded as a one and vice versa. The construction of these light links was such that the electronics was sealed in little cans which were soldered shut and filled with potting compound. Therefore, maintenance or modification of these links was impossible. NEC then constructed its own light link and the signal after passing through that light link is shown in figure six. There the asymmetries are negligible and this is at a rate of 5 megabits per second. The NEC light links are of double shielded construction. The printed circuit board is easily exposed even if the light link is clamped in place. Honeycomb windows are used to admit light into the shielded cylinders. These links are shown in figures 7, 8 and 9.

FIGURE I

LAB.	FIBER	COAT.	DISTANCE	SCHEME	BIT RATE	NO SIGNALS	VOLTAGE
JAERI	DUPONT CROFON OR LEMOSA LUMAFIL	NO	2 - 3 m	VF-FV	10 KHZ	17 A.C. 35 A.R. 13 S.C. 1 S.R.	80 KV
MUNICH	LEMOSA LUMAFIL	NO	12 m	VF-FV & DIGITAL	30 KHZ	13 A.C. 41 A.R.	16 MV
FERMI LAB	GALILEO	PVC	6 m	DIGITAL MULT.	1 MB/S 2	18 A.C. 26 A.R. 16 S.C. 24 S.R.	750 KV
U OF WASHINGTON	CORNING	YES	0.3 m	DIGITAL	1 KB/S	7 S.C.	60 KV
CHALK RIVER	CROFON	PVC	12 m	VF-FV	100 KHZ	5 A.R.	13 MV

Fig. 1. Fiber Optic Systems (A.C., A.R., S.C., S.R. stand for analog read, status control, status read respectively).

FIGURE II

LAB.	APERTURE	DISTANCE	SCHEME	BIT RATE	NO SIGNALS	VOLT
ORNL	34 mm	3.7 m	DIGITAL MULT. (CAMAC)	2.5 MB/S	11 A.C. 86 A.R. 103 S.C. 90 S.R.	25 MV
JAERI	42 mm	3 m	DIGITAL MULT. (CAMAC)	2.5 MB/S	29 A.C. 103 A.R. 118 S.C. 126 S.R.	20 MV
U OF WASHINGTON	51 mm	6 m	ANALOG	1 KHZ	1 A.C. 1 A.R.	9.5 MV
LASL	37 mm	6 m	DIGITAL MULT.	2 KB/S	20 S.C. 20 A.C.	7 MV
LASL	37 mm	7 m	DIGITAL MULT.	0.5 MB/S	64 S.C. 64 A.R.&S.R	7 MV
OXFORD	20 mm	7 m	DIGITAL MULT.	1 MB/S	1 A.C. 32 A.R. 90 S.C. 30 S.R.	10 MV

Fig. 2. Direct Optic Systems

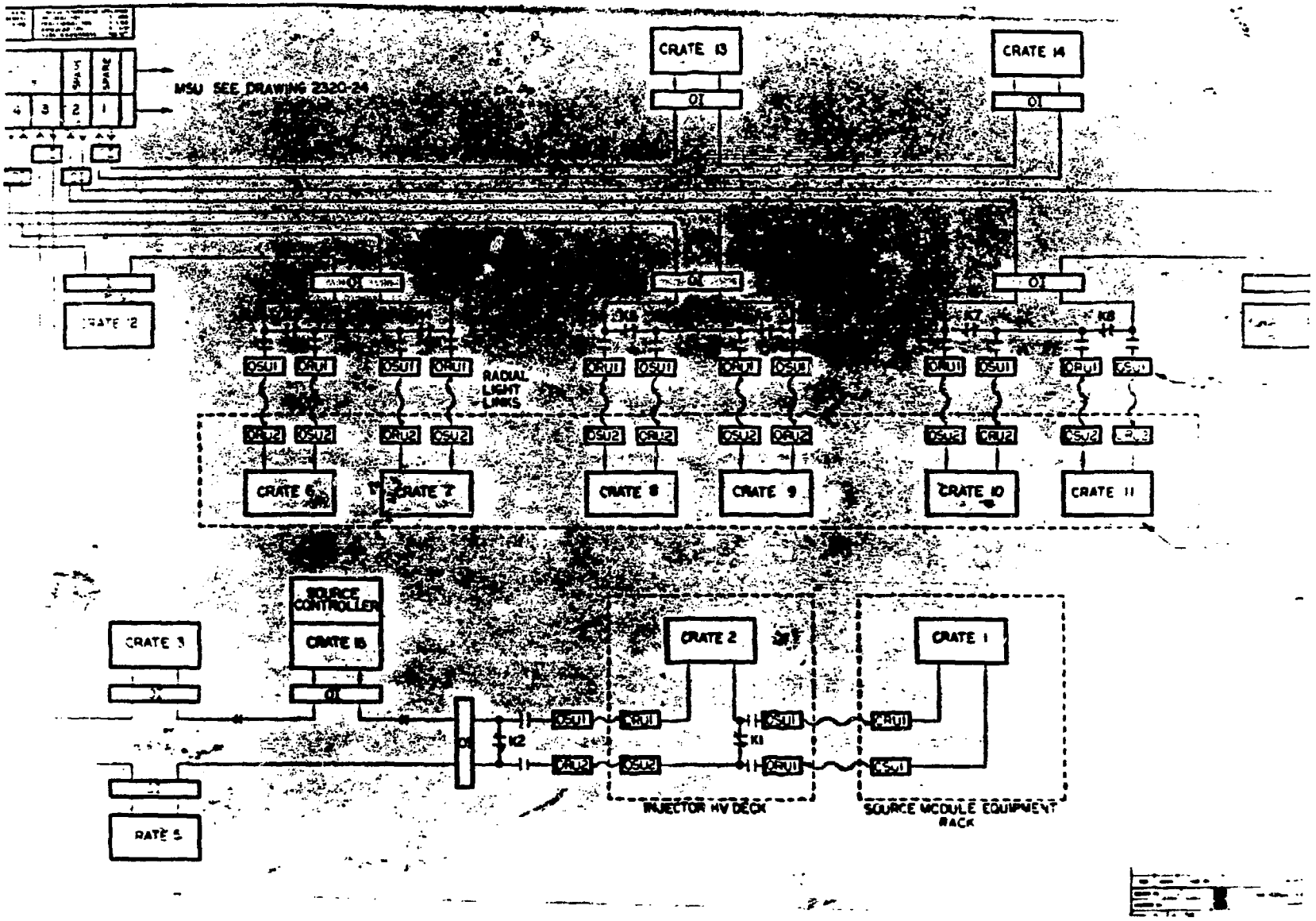


Fig. 3. ORNL CAMAC Serial Highway.

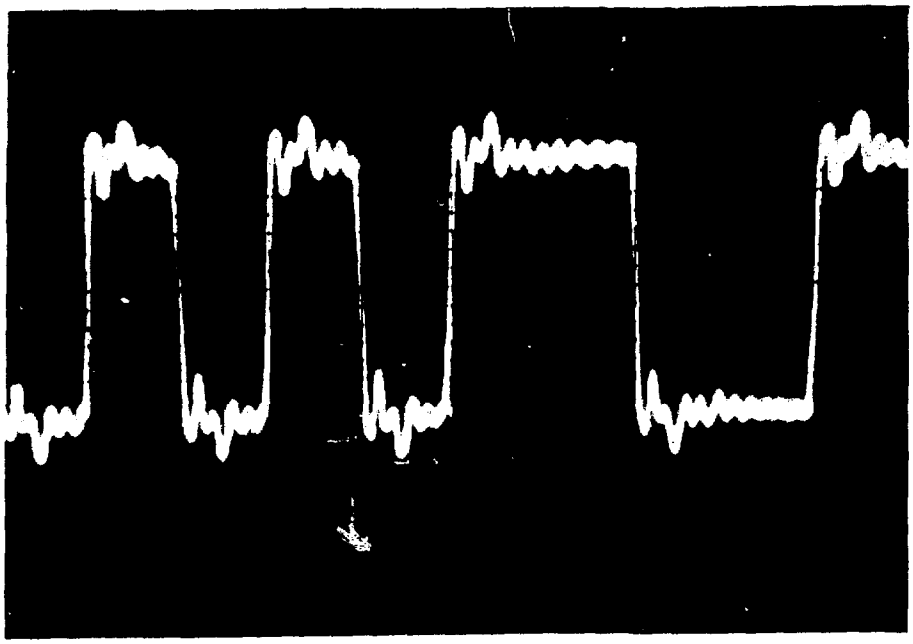


Fig. 4. Oscilloscope trace of the CAMAC serial highway input to the light links at 5 MB/S.

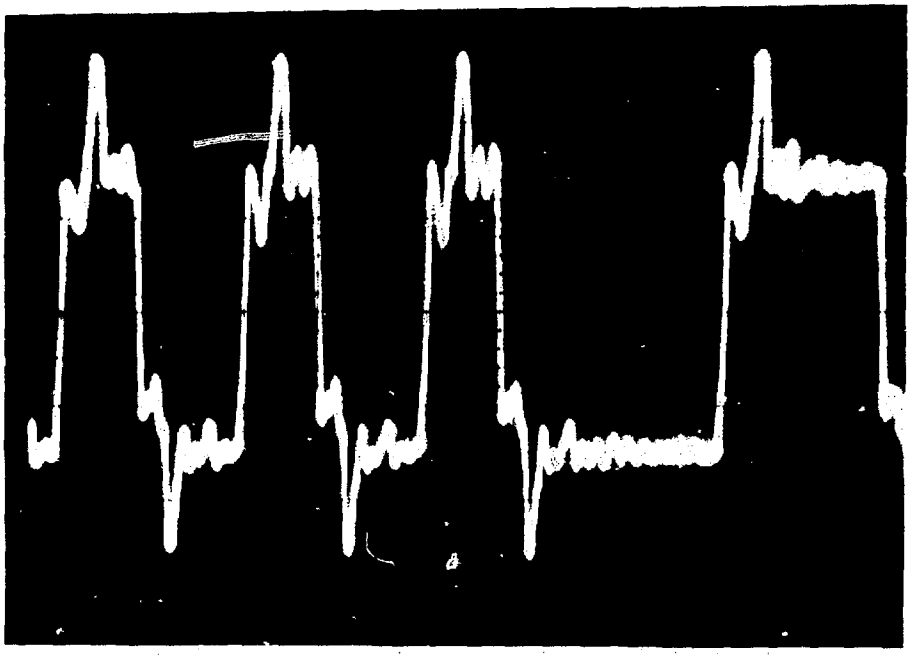


Fig. 5. Oscilloscope trace of the purchased light link output.

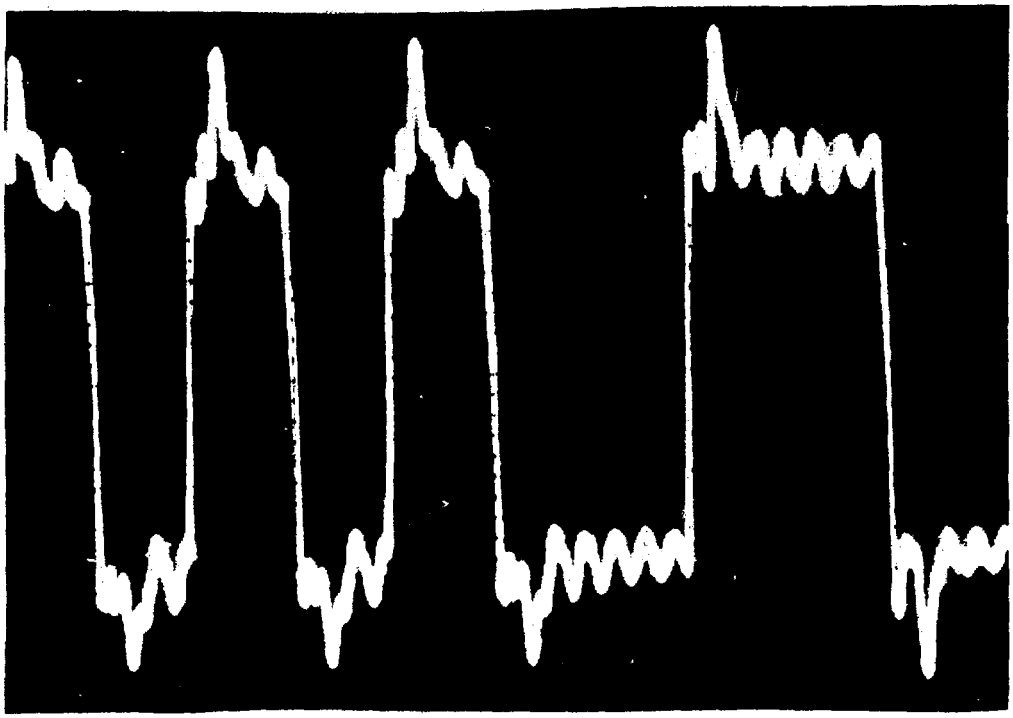


Fig. 6. Oscilloscope trace of the N.E.C. light link output.

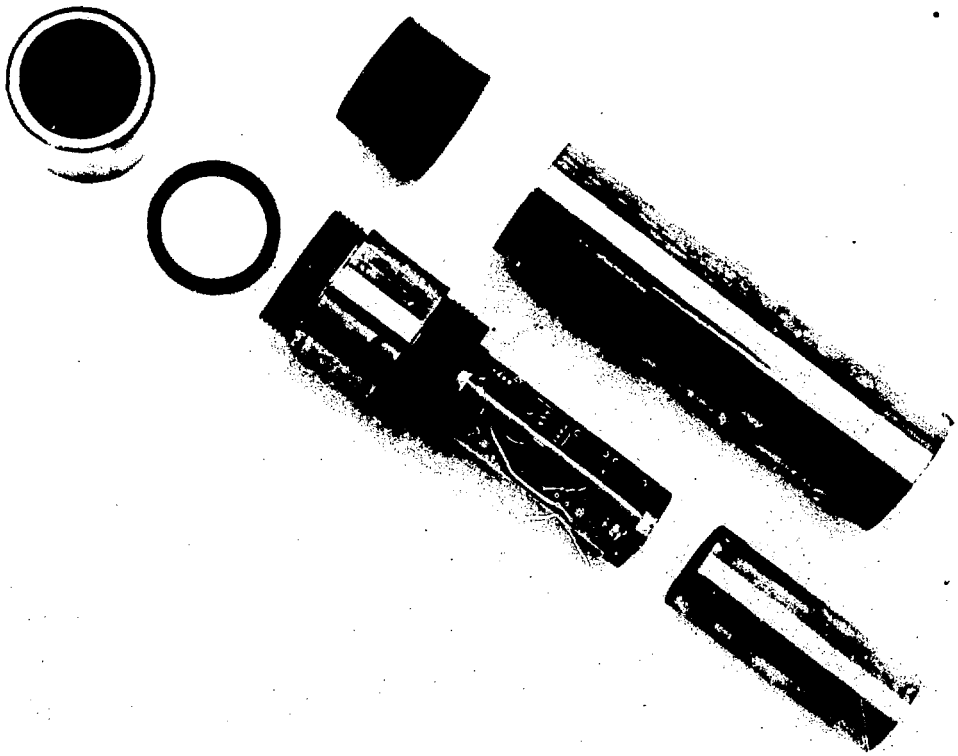


Fig. 7. N.E.C. optical transmitter.

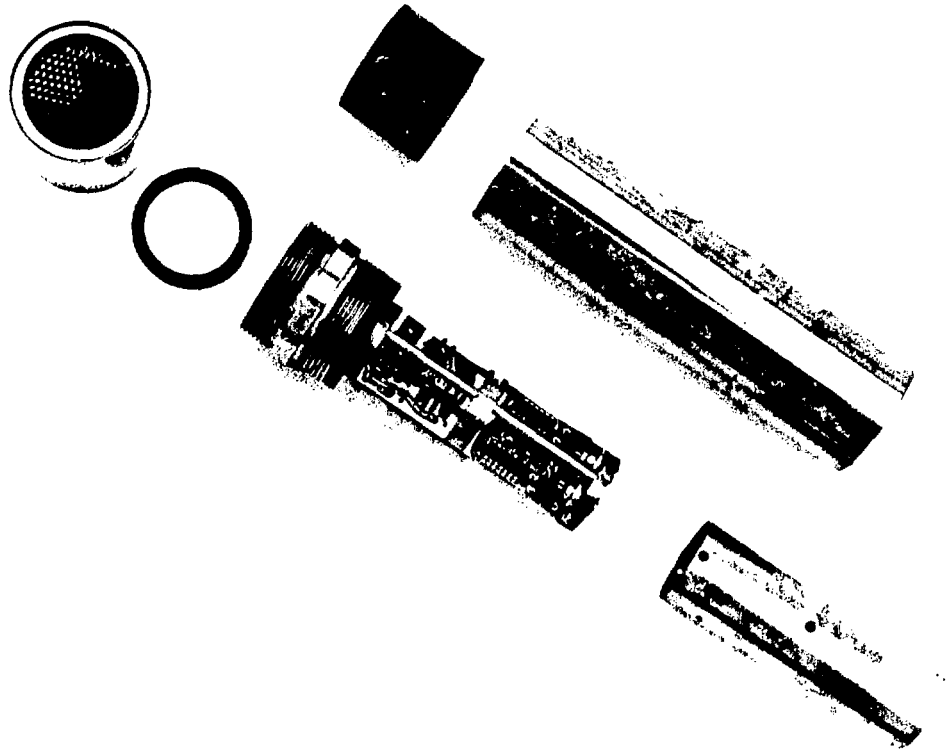


Fig. 8. N.E.C. optical receiver.

Fig. 9. N.E.C. assembled optical links.

THE HOLIFIELD HEAVY ION RESEARCH FACILITY 25-MV TANDEM ACCELERATOR CONTROL SYSTEM*

N. F. Ziegler
Oak Ridge National Laboratory

Several members of the HHIRF staff just recently visited NEC to observe some of the acceptance tests on the 25 MV machine. I think it is more appropriate to tell you about some features of operation of the control system than about the buncher, for which I am not prepared anyway. During our visit the console appeared as shown in Fig. 1. This is one of the consoles of the 25 MV machine in the NEC plant and we'll consider first the three oscilloscopes in the rightmost rack. The upper scope is for the display of NMR fluxmeter signals. The middle one is for the display of voltage ripple along the column. It displays the various CPU signals. The lower scope is for the beam scanner display. There are at least four beam scanners in the system and any one can be selected for display on this scope. Beneath the scopes, on the horizontal surface, are the track-ball and a so-called "do it" button. These are very important to operation of the control system and will be discussed later.

In the upper part of the center rack are the dedicated meters for the machine and beneath them are the three assignable meters. A more detailed view of two of these meters is provided in Fig. 2. When one of the meters is assigned to a certain function, the label of that function appears above the meter, and corresponding units and range are displayed beneath the assigned meter. Immediately beneath the assignable meters are the assignable knobs (Fig. 3). These are incremental shaft encoders and, like meters, can be assigned to various functions in the accelerator for control purposes.

The color display CRT is mounted in the leftmost rack of the console (Figs. 1 & 3) and directly beneath it appears a key-pad used to select a particular page of information for display. A more detailed view of a CRT page is provided in Fig. 4. Labels for various components of the accelerator appear in the left-hand column of the CRT followed by a column of analog readings. The third and fourth columns contain binary control and status information. In the third column and third line of the display in Fig. 4, there appears a white rectangle termed the cursor. This cursor may be positioned to any section of the screen by the track-ball. Positioning the cursor to a particular line on the page enables the component listed on that line to be assigned to either, or both, an assignable meter or an assignable knob.

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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.

The cursor is used in conjunction with the "do it" button to provide binary control. For example, in Fig. 4, if the cursor were moved to the ON field of the third column of line 13 and the "do it" button pressed, then HVS M1-1 (high voltage supply M1-1) would be energized. The ON label would be brightened to indicate status. Other status indicators may be color-coded or caused to blink to indicate condition.

In general, the control system performed well during the acceptance tests. It was convenient to use and operation was rather easily learned.

Discussion:

Wegner: Do you have any number of dedicated buttons on the console that get all high voltage without going to any pages or trackballs or anything else? You just wind it all up. Can you get it off with a button? Master overriders, actually.

Ziegler: I don't think there is. Incidentally, I should point out that I had nothing to do with the design of this. John Biggerstaff wrote the specifications, and NEC designed and developed it primarily under Robert Rathmell's direction. I mean it is a good system.

Fauska: Whose NMR is on your console? Is that NEC's or is that another product?

Ziegler: Scanditronix.

Rathmell: The scope on the console is a remote scope. The actual NMR is out near the magnet and that is a Scanditronix 751.

Larson: I saw a number of labels that I believed were labeled dummy. Is that some kind of feedback from the computer system reflecting on the capabilities of the operator?

Ziegler: We have also three knobs there. That is for East Tennesseans who go barefoot.

Jones: I wanted to ask Spellmeyer to give us an evaluation of his system; that is, you told us about it but you did not tell us whether you liked it or not. I think that you are the only laboratory who is using a system of this general type who actually finished it and used it. Could you give us your impressions of its weaknesses and its strengths and the things you might change if you did it over again?

Spellmeyer: Well, I started my business with VICKSI at the injector doing some electrostatic work and electrostatic people are normally very conservative, and at the beginning I was really afraid of having the computer running the CN. If you remember when I showed you the slide of the control console, you saw some buttons where you can switch off the belt charge where you can switch off the drive motor which are directly connected to the interlock system, not by the computer, and we would like to recommend it also to the machine here because sometimes even now when our computer system is in a very stable situation, it is of great safety to have such a button. Now coming back to our control system, I would say that one of the big advantages of the machine is a control system indeed because you can do so much, and we have not yet reached the limit really to use all the facilities that the control system offers to the machine people. And one example of where they did the most work was when the programming was done at the cyclotron where they made lots of programs of setting the magnet going through the injection element or going through extraction element. It is not far that this will be done just by putting a button at the touch panel and it does all itself. You can even evaluate the graphic knob which is displayed on the graphic display. For example, if you have to go through some slits, you may use the current on the right and left slits and send the beam over by a steerer. Planning those appropriate settings of the steerer can be done by our computer system. I would really say it is a great advantage and it is a must for our machine because it is a rather complicated project coupling machines of this type together.

Jones: It is a general character of the systems that you must proceed some degree more or less sequentially; that is, you must give up information or access to it to go on to the next step. Cast another way, you can only look at one page in terms of the NEC system at a time.

Spellmeyer: This is not very true in our system. As you have seen, we have some smaller tv screens too, so one of the smaller tv screens is now dedicated to the computer control system where it can see whether it is active and where it is active. The second one of the small tv screens will be dedicated to the injector parameters, and the big ones (color tv screens) are for common use. If we are running all the machine, we have on one tv screen the critical parameters of the injector and the injection beam line and on the other tv screen the critical parameters of the cyclotron.

Homeyer: I, perhaps, can make a comment to that. In the beginning the system is really much slower than a manual control system because you have to touch lots of panels, and you have to do it sequentially. So to start off, it is slower but right now after 1 1/2 years working with it, you don't do it sequentially any more. You develop the programs and set a large number of parameters automatically and you don't touch simply one quadupole or one thing at a time; you must move the total beam line up and down according to the magnetic rigidity. So right now these more complicated programs are coming up, and now you can really speed it up. What you have lost in the first time, you gain after one year, especially if you have this interpreter like we have--this multi-task and multi-user interpreter. It is very easy language to talk to our program. The operators learn how to write programs, and they invent more programs to help them out of the difficulty and to reduce knob touchings. So after a while you really speed up and you are much faster than when you control.

Biggerstaff: Let me speak to Harvey's question here not by answering but by pointing out that we were aware of the problem. In fact, there is a nomenclature that we developed when we were first specifying these controls and that was called the concept of the comfort control. It is a control you don't really need, but it makes the oldtimers feel a lot more comfortable to have it there. In a sense the third knob that Norval said was for barefoot operators, in fact, was envisioned at that time as you turned down the charging system in a hurry. You essentially leave one of those knobs permanently assigned to this thing you want to turn down. The other comment that I would add to that is that if some of these things turn out not to be comfort controls, the extreme flexibility of the system and the room we have allowed for expansion makes it almost a trivial job to add scram controls.

Chapman: This is concerning your computer room housing your electronics. When we were looking in there the other night, I saw what appeared to be a water sprinkler system over your computer area. This surprises me a little. Is is not much more customary to use a fire protection system that is not likely to be so expensive? We have already written off all the electronics of our electron microscopes by just such a system that decided to go off by itself.

Biggerstaff: We have problems with fire protection in this area. I'd rather not go into that. Except let me make one comment. A few years back Control Data contended that they were as happy with sprinkler systems as with any other fire protection system. They had had computers damaged by CO_2 systems, they have had computer systems recover from getting

wet, and they have had computer systems fail because of the quick freeze you get from CO_2 system (at least one CO_2 system) so Control Data once upon a time told me that did not care, and our fire protection people say that's what you're going to have.

Chapman: I see. I believe that there are nowadays other gas systems which are effectively, completely safe for electronics. They are expensive, but so is the electronics.

Biggerstaff: Robert Rathmell will be happy to know that his computer will be in such a system. There is a halon for the control system room, although I think that it actually is in the underfloor area, and I think overhead you would still get water.

Are there any other questions?

Broadhurst: I just want to ask the very obvious question. Although the computer will never fail, suppose it deep ends with everything set up; how long does it take to turn everything off manually?

Rathmell: You can shut down the charging system manually in the time it takes you to get from the control room to the motor starters. I don't remember the building well enough now to tell you what that is. If it is a problem and if it is going to be a problem, it's likely to be a problem in the early going. We'll implement a way to do that easily.

Biggerstaff: Let me remind Robert. He keeps forgetting this --that there is, in fact, a charging system shutdown interlock provided to us and also a beam stop in the injector line that are connected to our radiation interlock system and that wiring is in the control room. So it should be possible to add a simple switch to shut down the charging system and part of the radiation interlock system to put in a cup in the injector beam line. Robert keeps forgetting that, and I keep reminding him.

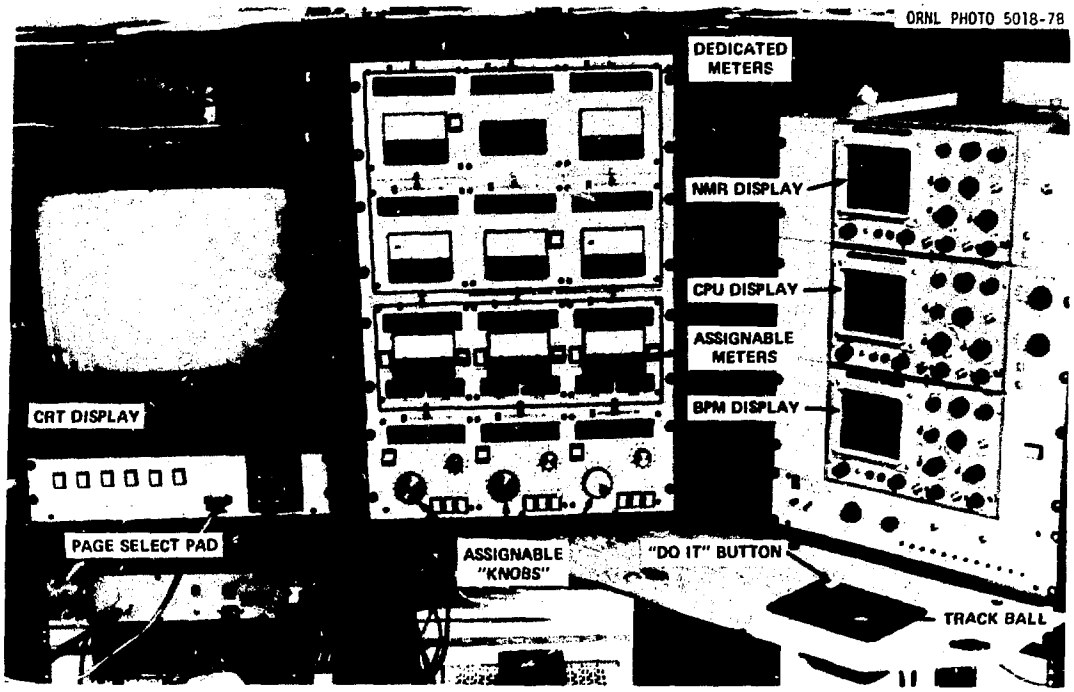
ORNL PHOTO 5018-78
DEDICATED METERS
NMR DISPLAY
CPU DISPLAY
ASSIGNABLE METERS
BPM DISPLAY
CRT DISPLAY
PAGE SELECT PAD
ASSIGNABLE "KNOBS"
"DO IT" BUTTON
TRACK BALL

Fig. 1

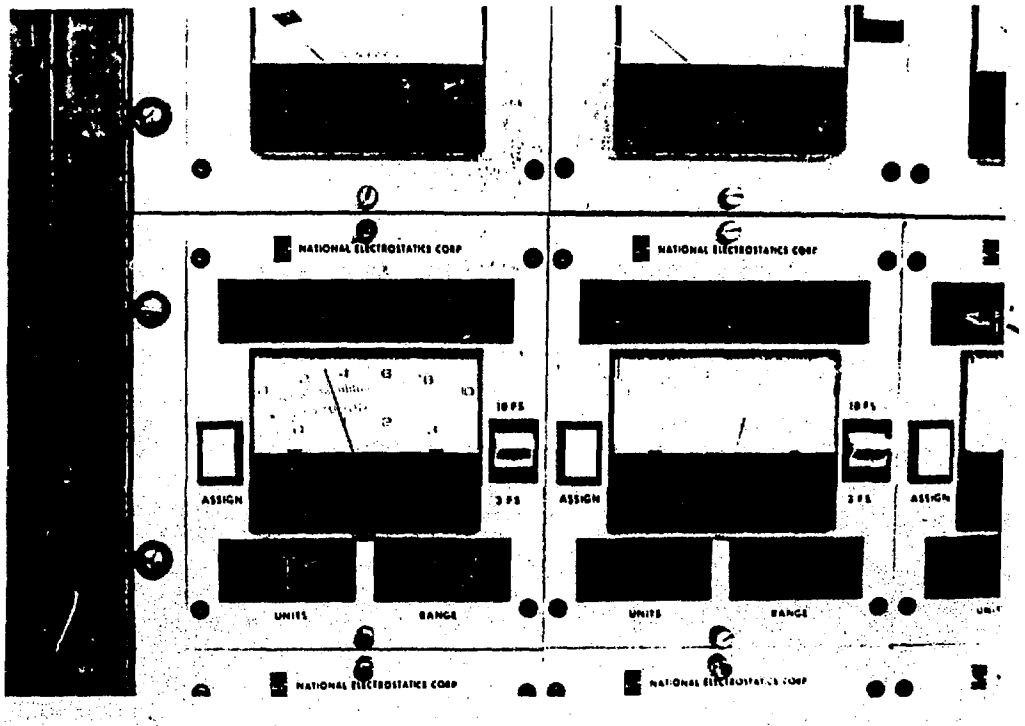
NATIONAL ELECTROSTATICS CORP
ASSIGN
UNITS
RANGE

Fig. 2

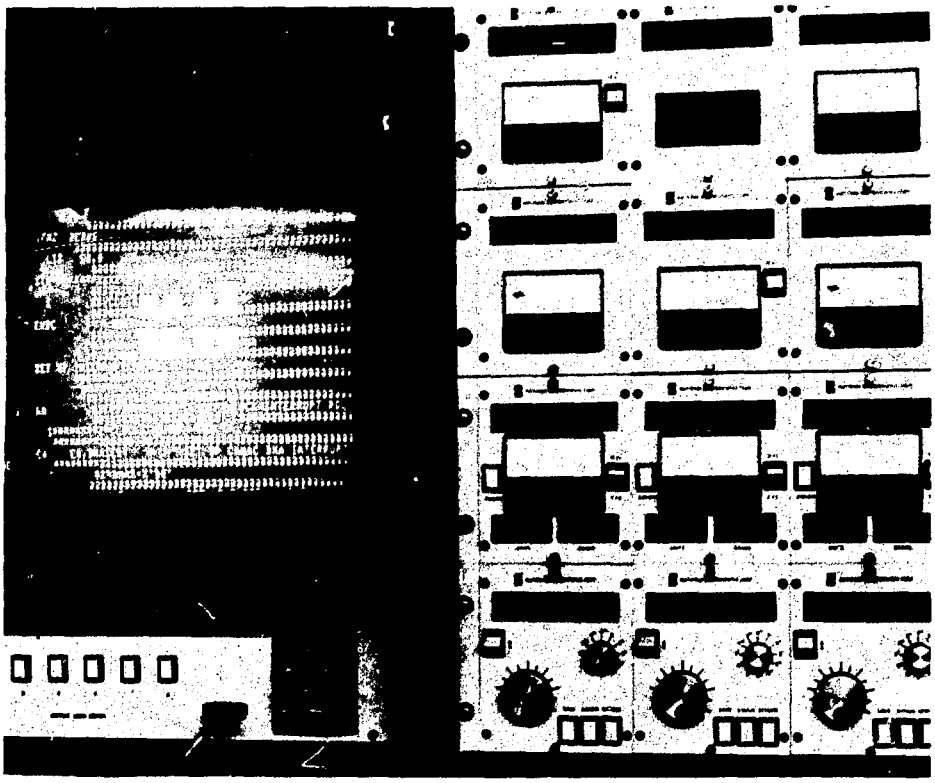

Fig. 3

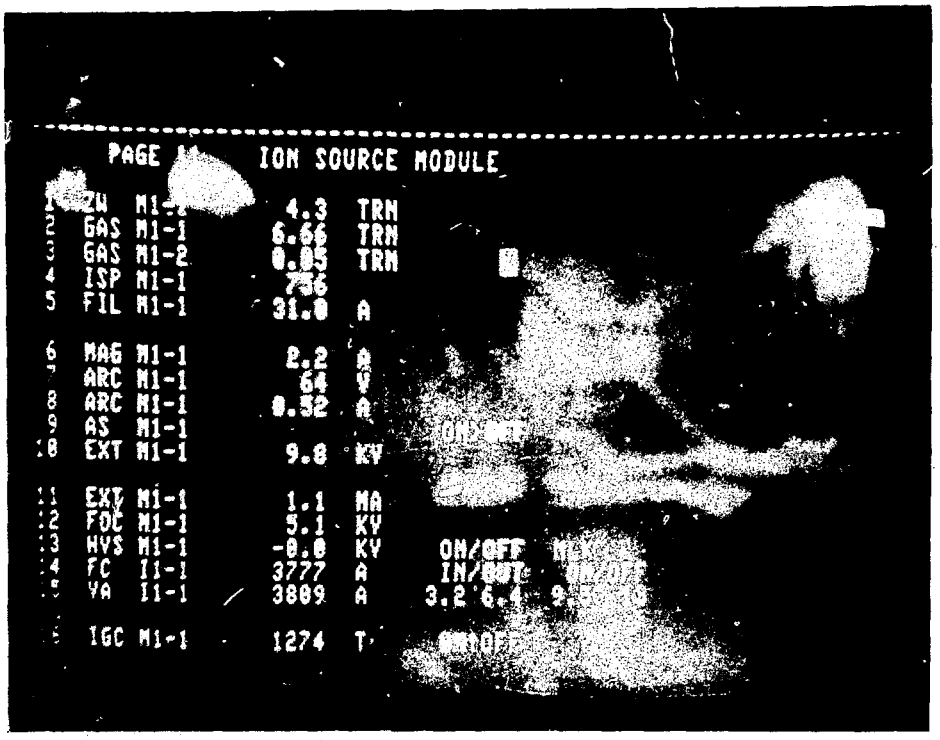
PAGE
ION SOURCE MODULE
GAS M1-1 6.66 TRN
GAS M1-2 TRN
ISP M1-1
FIL M1-1 A
MAG M1-1 A
ARC M1-1 V
ARC M1-1 A
AS M1-1
EXT M1-1 KV
EXT M1-1 MA
FOC M1-1 KV
HVS M1-1 KV
FC I1-1 A
VA I1-1 A
IGC M1-1 T

Fig. 4

Business Session

Chairman: John McKay

I'll try and make the business session fairly brief. If you look in the past minutes you'll find that it takes at least an eighth of a page. We may make it up to a quarter of a page this time. The first bit of business is maybe the most important. I would really like to thank Oak Ridge for having the meeting, and not just in general; but in the organization of everything, the outings, the meetings themselves, the way everything's gone almost on schedule, which is closer than I think we've done in most meetings. And having done it before, I know how much work this is. So I'd like to start the business session with a real vote of thanks to Oak Ridge, because I know they've done a tremendous job. (Applause)

The second item of business is our large financial statement. As of August 4, we had $400.85 Canadian in our bank account. That's the end of the financial report. I'm collecting money for next year's memberships, and this will go into a fund which, I expect, will probably be spent by one of the smaller labs when they have the meeting. I think this is probably an appropriate use for the funds. It pays to keep up the mailing list, and so on, as well.

Now in terms of the next meeting, we have three offers. Places that have offered to have it: Chalk River, Argonne, and the University of Pennsylvania. I have a note here from Charlie Adams, which I haven't read yet, so I'm not sure what. He hasn't said as much as he said last night. "The University of Pennsylvania would be pleased to host the 1979 SNEAP Conference if you so desire. We make this bid and submit $1 as good faith deposit. You can use the buck to buy everyone a beer."

I think that it's time for a little bit of a discussion on the nature of the meeting. We've had some discussions over beer. I think it would be useful to tell people a bit about the evolution of SNEAP. Some people have forgotten the definition of symposium. It was suggested as the name for the organization by Neil Burn. It's an after-dinner session with drinking, conversation, and other entertainment. And the reason this was chosen as the title is, the theme of this whole thing is supposed to be informality. And we've been around about 10 years now. The first meeting was in '67 and it was held in Dr. Rochet's (sic) office at the University of Montreal. So we've grown a bit since

then. But generally speaking, it's been a small, informal meeting. The Northeast has expanded a bit. We don't have anyone from Asia yet, but that may be in the works, apparently, according to the Minnesota people. So with the growth in coverage all around the world and the size of the meetings, we've gone from the original concept which was one of no formal presentations whatsoever, quite a few awkward silences, to a period where we had brief introductory presentations to get things rolling, to a few more papers, and to the sort of meeting we've had this time with many very, very excellent formal papers. I think there is a question of just what we want this meeting to be. And I don't think that we all want it to become a sort of small version of the accelerator conference. The meeting has oscillated in character from time to time. I think this is good because you never know quite what to expect when you come, and that keeps interest up. But there was a question, talked over beer, about whether it's time to go into one of these cycles and change the type of organization a bit for the next meeting so that we cover some of the more informal aspects as well as the formal ones, and I wondered if other people would like to comment on it a bit.

Connelly: I know there's quite a f w of the other people here, we've had some conversations on single-ended machines. Sitting through some of the sessions, they're very interesting, but they don't really pertain to us. We would like to feel there is, somewhere in the meeting, room for a number of single-ended people to get together in parallel sessions, perhaps, with something that's going on, and have our own little chat in an adjoining room or somewhere adjacent to the auditorium, all within the same building, of course, so there'd be no travel problem, - just to keep the people in single-ended machines coming, and keep them interested in the SNEAP Conference. This by no way means trys to divide anything; it's just that I think we could have a few, one or two, parallel sessions where we could just have a chat. If it becomes of a little more formal that would develop, but it would probably start out as something informal. Several people I have talked with have this feeling, I believe.

McKay: This is one possibility that we have talked of before. And I think that's one solution to the problem. Another solution is to have a session on single-enders. The single-enders seem to be very shy about talking with the tandem people and I think there's a lot to be learned from the single-ended machines for the tandems. But there are the two solutions, the possibility of having a session where maybe the tandem people keep a little quieter and listen to wisdom from the other side, or the parallel

sessions. Are there any other comments?

Levesque: It is true that the small machine people are a little shy, I think, as someone said over a glass of beer last night, "When somebody comes up and make a report of 40 mikes of copper, it's a little bit disheartening to come up and say, 'Well, yes, I got two'." I think that it is important that we, the small machine people, stay with the tandem people. My feeling is most of the tandem people have been or are still in some way attached to single-ended machines and the expertise ought to be able to come down hill. In my particular case, I haven't been in this business very long and I look up to people who have been in the business for a long time and have fought a lot of the simple problems that I'm running into, who could make life a whole lot easier for me. I found that this conference, although very interesting, did not give me much working material to go back to my laboratory with.

Berners: I am a card-carrying, single-ender, as well as a card-carrying, double-ender, and I would hate to lose contact with either of those two groups, even for one short session. I think even if I had only a tandem I would want to know what the single-ended people are doing, and the other way around also. I really would hate to see parallel sessions develop at SNEAP, because I think the technology of electrostatic accelerators is the technology of electrostatic accelerators.

Wegner: I think possibly one of the problems is that certainly a modest number of the papers presented at this meeting as you said are very good, would be quite appropriate for the International Conference on Electrostatic Accelerators, which is a different kind of subject than how you live with an accelerator from day to day and what the nitty-gritty problems are, and what are some of the interesting, mundane solutions to these problems and they tend to be mundane because a lot of them look obvious after you've found them, but it might take two weeks to find that obvious solution. And so, I think the original character of the conference was more on some of the tricks of getting the research out of the machine more than of the esoteric problems of ion sourcery, except when they pertain to a specific job on a machine. It is certainly nice and interesting to hear about all these things but I think a lot of people would like to hear about how you survive from day to day and keep everything running.

Larson: Maybe some of the people from both the single-ended facilities and the double-ended facilities could be encouraged to get up and bare their souls and say we can't

do certain things and how would the other members suggest to go about finding solutions.

McKay: I think that in the early days there were a number of people who go up and said 'here's my problem,' rather than 'here's my solution.' And part of it is a question that, as an organization stays around, changes character and I think that we cannot compete with the big accelerator conferences and I don't think we should try to. I know a lot of people have talked to me that maybe it is time to deformalize, if that's a word. There were a few suggestions that I heard over beer last night, and may or may not be accurately recorded. There was comment by Charlie Adams that he would like very much to have it at Penn, specifically to take it away from national labs, either Chalk River, or Argonne, or anyone else, because it will be on a smaller scale. The national labs give us one type of character and help us in many ways and I think the smaller institutes give something else to SNEAP. I think we need both, but Charlie's comment was that we should have it at a small place next time because we haven't had it at a small place for a while. He also suggested that our first session may be one of a series of very short reports from every lab that's represented, one page with a summary at the top and the summary gets read. Just to sort of get things rolling, to find out what people have done well during the year, and if you're brave and honest, what you haven't done well during the year. I think there's some merit to that. The organization of it, I'm not sure how it would go, but I think there's something to be said for it. There was also a suggestion that perhaps it would be better to go to something like the Gordon Conference arrangement, where instead of going to a lab, you go to a resort. And at this time of the year I know there's a lot of places you can organize a conference at very low cost. This builds up the informality of the session. We've never tried that, but I know the Gordon Conference people say it's great.

Wegner: The Gordon Conference doesn't turn off the recorders - there aren't any. There are no proceedings and that makes it very informal, because people aren't on a record and when you aren't on record you can call a spade a spade, if you like.

McKay: If you'll read some of the old minutes, you'll find that that was done. One more comment which I have heard from people, they find the microphones intimidating. Now if you're a ham like I tend to be, I don't mind microphones, but in some of the earlier sessions we had a couple of very sensitive microphones hung in the room and in the minutes you got 95% of what was said. One of the comments was that

maybe losing that 5% is worth the gain you get from the informality of the session, where people can really talk back and forward and you don't have to wait to move microphones around. Now, I think that perhaps what I would like to do is decide where the meeting is going to go, and that decides some of these other questions we've talked to, and leave it up to the next organizer to act on the suggestions that come from the floor.

Larson: I want to put this on the record. You made a comment to the effect that this conference could not compete with larger operations like the International Accelerator Conference. In formal structure, I certainly agree. But in information content, I think this is the place. I attended SNEAP last year and also the Strasbourg Conference and I would say that SNEAP was probably the better information source.

Schow: I guess I've come apologizing to these conferences because I have only a 2 1/2 MV machine, and yet, I would remind you that these machines run at very high gradiants, and it's basically a 2 MV machine that's been upgraded to 2 1/2 that we've run at 3 1/4 which would push your tandem that great a percent over design voltage. But our problem that really intimidates us, is those of us on the small machines find ourselves having to fix the computer, lift the drive motor in place, tighten the belt. We don't have an imposing group of experts, and therefore, we're fairly reluctant to speak out. I believe I would vote for a little more informality. There's an awful lot of knowledge to be had here if you're willing to stick your neck out. But it's not much fun admitting that you pulled a stupid booboo.

Billquist: I tend to certainly agree with a lot of this operational kind of information but I would kind of make a motion. Charlie Adams has some strong feelings in this vain, I move that we go to his establishment next year and see where he leads us, and see if we like it or not.

McKay: He was suggesting Puerto Rico last night. Okay, we have that as a motion. Neil, do you have some comments?

Burn: The original idea, or the original intention of SNEAP was, as John has just said, basically informality, and the reason that the formal presentations have crept in was that we found in the first few sessions that people would need some kind of a guideline. So it started off that we picked the session chairman to be an expert in communications, or whatever it may be, and it used to be up

to the session chairman to say a few words at the beginning of the session to get the ball rolling. And I think that the way that the formal presentations got in simply was an aid to the guy that had to do the editing. It's very nice for the people issuing the proceedings and so on to have all this stuff on paper. I agree, and I would like to see the informality brought back again. I agree that it ends up not being the kind of information exchange that we want - that the majority of people want.

Chapman: I'd like to just make two points. One, it seems to me at least that both the national labs and the small labs have something to offer, and perhaps we should consider trying to alternate between the two, maybe not necessarily on a one to one year basis, maybe two in one and one in the other, but at least try and alternate, so that we maintain the advantages of both. The other is in connection with your remark that perhaps there should be a short presentation from each lab that attends. At the Ebeltoft Conference, which I would call the European SNEAP, effectively they wrote to all the labs that were participating, and a few others, and asked them effectively to complete a questionnaire before the meeting. Now that may not be quite the right format but we could similarly ask them to write such a report. Then they were collected together and duplicated and were available to you at the beginning of the meeting. This was very useful indeed.

McKay: This is what Charlie was talking about and those of us who hadn't been to an Ebeltoft Conference were wondering how workable it would be, but were interested in the concept. I agree with you that I think it should oscillate between the big labs and the small ones because I think all nuts and bolts after a few years tends to be very repetitive. And I think cycling from some of the formal talks to informal talks on a fairly short basis makes the meeting work.

Clegg: I just wanted to say that although this is my first SNEAP Meeting and I found it very enjoyable, I do believe that the idea of having a 5-minute presentation at the beginning from each lab would bring one thing that I have gained only by sitting here 3 days, and that's to get to know people quickly and know who represents what lab. If the first day you had somebody from each lab standing up for 5 minutes then you would be able to orient yourself and ask these people involved.

Moak: It may be that this is a phenomenon not so much associated with big lab, small lab, as the proportion of time devoted to breaks. It's quite possible that people come to these meetings knowing things that they don't

realize they know. And it never would occur to them to speak on the subject. And other people come here needing to know things that they don't know they need to know. And it may be that interactions, perhaps over coffee; I guess what I'm saying is that if the coffee breaks were longer and the sessions were shorter, it might be that the interactions you want would work better, whether you're in a national lab or in a small institution.

McKay: I think that's a good point. If there are no other comments, then we have a motion on the floor that the next meeting be held at the University of Pennsylvania. All those in favor? Those opposed? I think the Aye's have it. I'd like to thank Argonne and Chalk River very much for their offers and we're going to remember them. I think that's pretty well all the business I had except dues and memberships. I would like to get them because I want to keep the mailing list up to date more than anything else. The other question which I think should be asked: We sort of assume that McMaster will continue to be the filing drawer for SNEAP and that the person who gets the meeting the next year will be the other chief executive. I'm willing to keep the mailing list up if people are happy with that. (Applause.) Okay then. And I think that's all I have to say. I'd just like to repeat again my thanks to Charlie and all the people here at Oak Ridge for a very fine meeting and wish them luck on that huge silo out there.

PARTICIPANTS

H. F. Abendroth	Brookhaven National Laboratory
C. T. Adams	University of Pennsylvania
G. D. Alton	Oak Ridge National Laboratory
P. Ashbaugh	McMaster University, Canada
J. K. Bair	Oak Ridge National Laboratory
J. A. Benjamin	Oak Ridge National Laboratory
E. D. Berners	University of Notre Dame
B. Best	Air Products and Chemicals, Inc.
J. A. Biggerstaff	Oak Ridge National Laboratory
J. H. Billen	University of Wisconsin
P. J. Billquist	Argonne National Laboratory
J. H. Broadhurst	University of Minnesota
N. Burn	Chalk River Nuclear Laboratories
K. R. Chapman	Florida State University
F. Chmara	Peabody Scientific
T. B. Clegg	University of North Carolina
C. E. Connolly	University of Lowell
E. de Vries	McMaster University, Canada
P. K. Den Hartog	Argonne National Laboratory
D. C. Elliott	National Research Council, Canada
S. Elliott	University of Pittsburgh
H. Fauska	University of Washington
L. R. Fell	Dowlish Developments, Ltd., England
A. Filevich	Brookhaven National Laboratory
A. W. Haberl	State University of New York, Albany
H. Homeyer	Hahn-Meitner Institut, Germany
H. Janzen	Queen's University, Canada
J. W. Johnson	Oak Ridge National Laboratory
C. M. Jones	Oak Ridge National Laboratory
R. C. Juras	Oak Ridge National Laboratory
G. M. Klody	National Electrostatics Corporation
W. Kutschera	Argonne National Laboratory
R. Lapointe	Laval University, Canada
J. D. Larson	Brookhaven National Laboratory
R. Levesque	Bell Telephone Laboratory
B. Z. Levy	State University of New York, Stony Brook
R. A. Lindgren	Brookhaven National Laboratory
J. E. Mann	Oak Ridge National Laboratory
J. A. Martin	Oak Ridge National Laboratory
J. W. McKay	McMaster University, Canada
R. A. McNaught	McMaster University, Canada
R. Middleton	University of Pennsylvania
P. D. Miller	Oak Ridge National Laboratory
W. T. Milner	Oak Ridge National Laboratory
C. D. Moak	Oak Ridge National Laboratory
J. L. Moore	Oak Ridge National Laboratory

S. W. Mosko	Oak Ridge National Laboratory
L. L. Nichols	Batelle Northwest Laboratory
G. A. Norton	National Electrostatics Corporation
G. Oso	University of Ife, Nigeria
C. Pauley	State University of New York, Albany
J. J. Perona	Oak Ridge National Laboratory
W. L. Polvent	State University of New York, Stony Brook
R. Prechter	University of Rochester
K. H. Purser	General Ionex Corporation
D. J. Raso	High Voltage Engineering Corporation
C. A. Ratcliffe	Batelle Northwest Laboratory
R. D. Rathmell	National Electrostatics Corporation
E. G. Richardson	Oak Ridge National Laboratory
L. Rowton	Rutgers University
K. Sato	Yale University
R. O. Sayer	Oak Ridge National Laboratory
O. E. Schow	Oak Ridge National Laboratory
S. Skorka	University of Munich, Germany
B. Spellmeyer	Hahn-Meitner Institut, Germany
J. W. Stark	McMaster University, Canada
P. H. Stelson	Oak Ridge National Laboratory
J. D. Sturbois	Ohio University
J. R. Tesmer	Los Alamos Scientific Laboratory
P. Thieberger	Brookhaven National Laboratory
B. Thorburn	High Voltage Engineering Corporation
P. E. Tykesson	University of Aarhus, Denmark
H. E. Wegner	Brookhaven National Laboratory
W. G. Weitkamp	University of Washington
G. F. Wells	Oak Ridge National Laboratory
R. Woods	Los Alamos Scientific Laboratory
J. L. Yntema	Argonne National Laboratory
N. F. Ziegler	Oak Ridge National Laboratory

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