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Conference Proceedings

SNEAP 77

Symposium of North-Eastern Accelerator Personnel

Held at the Los Alamos Scientific Laboratory

Los Alamos, New Mexico

September 26—28, 1977

LOS ALAMOS SCIENTIFIC LABORATORY
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Compiled by

Richard Woods

FOREWORD

These proceedings contain the verbatim discussions and papers that were presented without written copy. The compiler has taken the liberty to modify and present them in a readable form.

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ABSTRACT

Membership in the Symposium of North-Eastern Accelerator Personnel (SNEAP) is made up of institutions with electrostatic (Van de Graaff) accelerators. The annual symposium is attended by individuals who have the responsibility for operation, maintenance, and development of these accelerators. One or two invited or contributed talks open each session, and then general discussion of the talks and related topics is solicited by the session chairman from the attendees. These discussions are usually very productive and contain the major volume of information in these proceedings.

WELCOME

by

Richard Taschek
Associate Director
Los Alamos Scientific Laboratory

I feel privileged to have a chance to welcome you here on my behalf and on behalf of the Laboratory. I hope you get a chance to look around the unclassified areas of the Laboratory. I am sure some of the local people will make arrangements for you.

I think this is a group that has become very outstanding in representing a narrow but very important aspect of the methodology of nuclear physics and, by now, other related basic sciences. It's an area that I grew up in and am still very fond of, but I am certainly not in the mainstream of activities. Nevertheless, I see many faces here that I still know, and they all look as if they are enjoying it. I hope you enjoy the activities both in and out of the conference. Thank you.

Session I, Chairman, Ken Chapman

ABSOLUTE CHARGE STATE YIELDS OF 20 MeV ^{127}I IONS EMERGING FROM A GAS STRIPPER

Charles D. Moak
Oak Ridge National Laboratory

Over the past several years we have been measuring charge state fractions from fairly heavy ions scattered from various gas atoms both in the so-called single-event region where one is studying scattering cross sections for charge change, and in the so-called multiple scattering region where one is dealing with a gas cell which has fairly high pressure and many scattering events take place for each ion. In general, strippers which are used in tandem accelerators fall in this second class and it's often true that one can take data which are of very little use if one doesn't have a very big computer. I will try to explain. Suppose that one has a gas stripper with no windows, differentially pumped, and fires into this gas stripper some fairly low charge state heavy ion at not a very high energy but high enough that this low state is not typical. When one studies the charge distribution of particles coming out we all know that this charge distribution is scattered over half a dozen or more different charge states and there is a probability distribution of these charge states that one can measure. How each charge state got to be that way though is a complicated thing. For example, an ion starting with a charge 5 and ending as a charge 15 can arrive at charge 15 by a bewildering variety of paths. It could be that charge 15 is produced by fetching 10 electrons off that ion in one single scattering. It's also possible to knock off two and then two more and so forth until you work your way up to 15, or perhaps even one at a time up to 15. It's also possible to knock a number of electrons off, catch back a few, knock some more off and catch back a few. So you have almost hundreds of cross sections, capture and loss, to integrate from single scattering experiments in order to arrive at a prediction of what you would expect for some particular situation. We have lots of data now for single scattering and one can compute things but these computations are very difficult and I think not very reliable. So we decided that we would do an engineering experiment which would be of some use to tandem people and we decided that we would do it in a way that it would give us answers which are directly applicable to tandem engineers work and to physicists who are about to embark on an accelerator experiment and want to know what kind of yields of what charge states they can expect. We have built a special piece of equipment for this purpose and I want to describe that. First, I should say that there are many workers in this field and I can give a complete reference list, but it would take a lot of time to give it. There are excellent reviews of this subject by Hans Betzs and you all know these. The experimental arrangement I would like to talk about which is new for us is shown in Fig. 1. The beam enters from the left and passes through what we call a monitor aperture. I have the details of this aperture written down. The beam is highly collimated coming from the tandem so we use no quadrupoles up to the point where the first aperture exists. Here is the first aperture but there are collimating

apertures back in this direction so we have a highly collimated beam. The particles then pass through a differentially pumped gas cell and then they pass through a quadrupole lens which is on the axis. Then they pass into an electrostatic analyzer which consists of two plates and a position-sensitive detector which can be drawn up out of the way so that this end detector can be used to integrate the entire beam with the gas cell empty. Now we wish to make absolute measurements. By that we mean we would like to calibrate this detector in terms of the total number of particles going into the system. This is a thin foil scatterer and that scattered stuff is cleaned off by the next aperture, so this is what we call a monitor detector. If the gas is left out and the quadrupole is left off, the beam just simply goes straight through to the end detector. If you have a thousand counts in the end detector per count in the monitor then later on when you have ten counts in the monitor you know that ten thousand counts went into the system even when this position-sensitive detector is in place. The central portion of the beam which is sampled is 10 mm in diameter and is uniformly illuminated by a rather large, almost one-inch-square beam. The nickel foil is 0.6-microns thick and the hole in it is 1.14 mm in diameter. The next aperture is 0.5 mm. The remaining apertures are large compared to that so there is no slit scattering from them. Now the collimator is selectable and can be either 4, 6, or 8 milliradians half-angle. It defines an acceptance for the so-called "mock accelerator" which exists to the right of that aperture. Those half-angles correspond to 0.23, 0.34, and 0.46 degrees. These are rather typical acceptance angles for accelerators, perhaps a little bit on the large side. The quadrupole lens can be set to focus only one charge state at a time. You can see what happens in this little diagram where the charge state Q is being sought and the charge state Q + 1 is being over-focused and Q-1 is being under-focused. Now we had to demonstrate that this collected all the particles onto the position-sensitive detector and so Fig. 2 partially illustrates that it does. These figures were all run for the same number of particles into the system for the same presssure, etc. In the top of Fig. 2 you can see that the center peak is bigger than either peak to left or right because in this case we were focused for charge 12, and therefore over-focused for 13 and under-focused for 11. Here we were focused for 13 and not for these two, here focused for 14 , etc. We measured the full widths at half maximum in terms of millimeters and demonstrated that both the lateral and the longitudinal dimensions of the beam as focused were much smaller than our detector. So we are collecting every single particle which comes out of the gas cell as charge 14 and putting it on the detector which means we are being quantitative here. This number of particles per monitor count can be related to the number of particles per total particles entering the system on an absolute basis. We used a computer code to predict the field ratios necessary to focus a particular charge state and these field current scales were determined empirically for one charge state and then they track perfectly for the others. Two detector positions were used, one for the low-charge states and one for higher charge states with a detector partly withdrawn so we could measure charge states 10 through 20. For high-charge-state measurements the procedure was to measure the number of particles-per-monitor count. The product of this number to the ratio of monitor counts to end detector counts then gave the absolute yield. But because of power supply limitations the lens could not focus particles below charge 10 and some data could not be measured, but this turned out not to be serious because at the very lowest gas pressures the scattering angles were so small that all the charge-state peaks were well resolved and totally collected on the detector in spite of the fact that they were not focused so the yields could be measured without the use of the quadrupole lens for very small

scattering angles. We have some data which is shown in Fig. 3 which is given in terms of absolute yield and I have to explain a little bit . The cell feeds into a 4-milliradian half-angle and now this is the absolute yield. At this point we can read a curve. At that black point there is charge 14 at a pressure of 0.1 torr and the total number of particles is 4×10^{-3} of the whole beam. It's an absolute yield so you can read these curves and tell precisely what you really wish to know about what will happen when you put that particular gas in a differentially pumped gas cell in a tandem accelerator and you have about that solid angle of acceptance in the high-energy end of the machine. We believe these numbers will be helpful to tandem engineers and physicists. Now for 6 milliradians, Fig. 4 shows a similar set of curves, and I would like to remark before we go further that it seems very clear to me at least, that the choice of nitrogen would be very poor if you wish to have high-charge states. But worst of all the choice of nitrogen is not really all that good if you want to go for low-charge states. There were many people who said that nitrogen scatters the beam less and therefore the yield will be higher because you lose less of the total beam in the stripper. Apparently, that is simply not true at all. The fact of the matter is that nitrogen is a poor choice for a stripper gas almost entirely across the board but for the higher-charge states it's a loser, a terrible choice. Near the end of the week of data taking we had time to take one additional gas, and in Fig. 5 we have krypton to add to the family, at 8 milliradians. I should remark about this interesting behavior of charge 5, which is the primary beam in every case, which is dropping from unity at 0 pressure. You will notice that it's a bit higher for krypton than for the other three gases. What this means is that the total cross section for charge change to any other charge but the primary charge is lower for krypton, and that means that the survival of the charge 5 beam is somewhat higher and this has been seen in other experiments before, but we have double-checked this and it's a fact. There is one more thing to be noticed. There is not a great deal of difference between, say, argon, zenon, or krypton. So the added expense of using zenon is in some cases justified, and in some cases not. If you are collecting it, of course, it is justified. But as I say nitrogen is low. Now it would also be of interest to the tandem engineer to know for whatever pressure I can twist on my knob, what's the best that can be done. That's not always the same pressure as you can see. Some of these peak up even slightly below where we run and some above. So what is the best that can be done irregardless of pressure? We sort of collected some points from these graphs and put them together in Fig. 6 as a sort of guide. This is not a scale, it is just an ordering of these for different gases and putting in dotted lines just to guide the eye for a particular charge state. You can see here, this number is the best that can be done for krypton regardless of the pressure and the same for the other gases. This number is the best for 19 that can be done for argon, no matter what pressure. This is an interesting guide and perhaps useful as one goes along. Regardless of what pressure you use, what is the best that can be done if you also, in addition, change the solid angle as shown in Fig. 7? Now you can see that for small charge states these are gentle collisions, impact parameters are large, the scattering angles are small, and very few electrons are taken off the ion. The scattering distribution in angle is quite narrow and so it doesn't matter much whether you use 4, 6, or 8 milliradians for the lower charge states. But you see for the larger charge states you have larger scattering angles, these are more violent collisions, smaller impact parameters and you have larger scattering angles. You can see that in the case of charge 20 for zenon, one gets a very large increase in yield as one goes from 4 milliradians to 6 milliradians. If it is possible by means of using a quadrupole lens in the

terminal of the accelerator to improve the solid angle of the acceptance of the high-energy end of the machine, then at least for high-charge states this can be a very great improvement in yield. Now we can compare the disadvantage that gases have over solids for high-charge states by plotting data for carbon foils and for zenon gas as shown in Fig. 8. Here we have direct comparison of the charge state distribution. This is all 20-MeV charge 5 of iodine going in. Shown here is the carbon foil for 3 different solid angles and also for zenon gas. It's interesting that zenon gas appears to have a slight inflection just here, and the slope becomes slightly more shallow just here where the carbon foil peaks up. No one knows why this is true. We can compare the performance of the two types of stripper now for the high-charge states. Obviously, in this region the gas stripper is better for low charges, and in this region the solid stripper is better for high charges. But it is really important to know how much better it is and so we have plotted the ratio of absolute yield for the gas divided by absolute yield for the solid, and that is this curve here, and its scale is on this side. You will note that the ratio starts high for low-charge states and quickly gets rather poor as one goes to high-charge states indicating that the carbon foil is better and the zenon is poorer. Here the disadvantage factor is something like 4×10^{-2}. That would be about a factor of 25. This means then at that charge state that the carbon foil will give you 25 times more beam. But for how long? The gas will last forever and the carbon foil certainly won't, and there is an additional factor that has to be considered. Sometimes the low-energy end of the accelerator is not the limiting factor, the carbon foil is. One could pour more beam into the machine, and in many cases find that the low-energy end of the accelerator system will stand larger beams, but it just makes the life of the foil so brief that it isn't practical to use these larger beams. What I am saying is, if you can use somewhat larger beams with the gas stripper, and if you take into account the long life of the gas stripper, it's quite possible that this factor of 25 would not be such a serious disadvantage. But at least now we know what the disadvantage is and we know where we stand. This was the purpose of this work, to try to develop, or to begin to develop, some numbers which the tandem engineer and physicist could use in making his plan for experiments and designs. Thank you.

Discussion:

Chapman: Did you not have the opportunity to do any of these experiments with any of the exotic gases that, for instance, Daresbury has proposed for their stripper?

Moak: They have used some fluorocarbons, Fomblin, and several others which appear to be promising. But the difficulty there is not the exciting ability of the gas to produce somewhat higher charge states but the conservatism of the tandem engineer about putting that stuff in his accelerator tube.

McKay: I guess we are very old fashioned, because we use oxygen as a stripper gas. I believe we use it because that is what we have always used. I wonder if you have any comments on why you have gone to these other gases?

Moak: We have used argon in our tandem almost from the beginning. We started with oxygen and switched to argon because I couldn't find any zenon, and it was very expensive to buy and I didn't have any way to collect it. It appears that if one uses Nitrogen or oxygen or a lighter gas that one doesn't get as high a charge state. In the case of the Oak Ridge Tandem Cyclotron combination

there are times when you don't wish to have a high-charge state you want to keep the charge down so that you can do your stripping inside the cyclotron for capturing the ions. But we found early on that the heavier gases do appear to give higher yields for high-charge states and we demonstrated this on iodine ions in our own accelerator and we never found any reason to change away from argon.

Schultz: The information that you have given is very valuable in light of post accelerators, but I have trouble relating it to terminal strippers in tandems where we are not coming in with a plus 5 at 20 MeV. Does any of this calibrate back to low-energy beams?

Moak: We have good data which indicates that for the highest charge states it doesn't matter at all what the charge state coming in is, it could be charge 5, charge 2, charge -1, and the collisions are all so violent that those electrons come off before the ions or atoms get very close together. The ion sheds those loosely bound electrons on the way in to the collision. Therefore, what I have been saying applies no matter what the input charge state, but for lower charge states where the collisions are more gentle then, of course, the input-charge state and the output-charge state are affected by each other. Now, the objection that you have to our data that it is not at 14 MeV is valid and we have done the data at 20 MeV for obvious reasons. We do intend to continue with our work and do some more energies so that one will have tables which would apply to the Chalk River or Brookhaven or other tandems as well. I feel that for high-charge states it is not so terribly important what the input charge is, as we have demonstrated. I think these data are useful for ordinary tandem strippers in the terminals of tandem accelerators for high-charge states.

Liebert: Have you tried to fit any of the data with semi-empirical formulae for extrapolation?

Moak: We have done so, the difficulty is that these are neither fish nor fowl in respect to being equilibrium or single event. It turns out that the optimum pressure is not a pressure at which you have multiple scattering in the sense that there are at least a thousand events or so, maybe 10 thousand events going through the gas cell. Nor is the pressure so low that there is only one event or perhaps none for particles going through the gas cell. We are in the so-called plural scattering region. This makes empirical fitting very difficult, especially if you have nothing to guide with. Besides, our data are not sufficiently broad-based yet in energy and in ion species to give us enough parameters so as to feel confident about empirical fitting. I think you should go to heavier gases and larger solid angles if you wish high-charge states, I think those two statements will hold.

Wegner: The geometry of your gas cell system, with its differential pumping, might be difficult to achieve operationally in a high-voltage terminal with reliability. In fact, the geometry in many terminals, especially some of the newer machines, is radically different than this geometry in a sense that the gas cell is very very long with a completely different kind of pressure profile than you are experiencing here. Do you have any evidence that geometrical factors affect any of your data in terms of utilizing it as practical numbers to apply to tandem engineering?

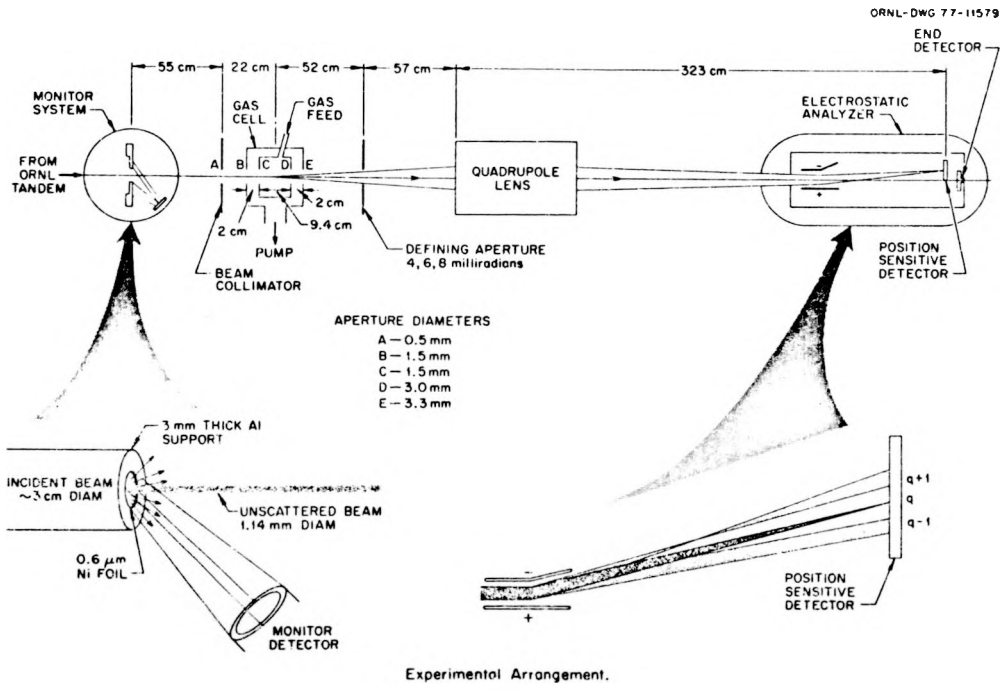
Moak: Figure 1 - Everyone has heard about the density effect on gases. This density effect is clearly known to be true and we have studied this in the present context and have worried about that very problem. Here we have compared the charge-state distribution of our 20-MeV iodine ions 5 plus going in for 2 cell lengths, one short at very high pressure and one almost five times as long at almost five times lower pressure. So the total target thickness is identical in the two cases, as near as we can make it. You will see that the charge-state distribution is shifted. The higher the density, the higher the charge state. In fact we are about six orders of magnitude away from solid foils and we are about six charge states away from solid foils. You almost have the feeling that each order of magnitude in density would perhaps buy you a whole charge state. Now translated into terms of the real world of gases, I think it would be very difficult to achieve factors of 10 upward in gas pressure, but perhaps with some of the vapors it could be done. Now coming to your question, factors of 10 downward would perhaps produce somewhat comparable results 1/2 to 1 charge state and so we feel that one can scale down to the low-density, long-stripper canals and arrive at numbers which are slightly lower in charge state especially for the low-charge state. Notice that they are really getting closer for the higher charge states, you don't make all that much error here. So we feel if you don't imitate our gas cell you do make some error and you do have some indication about what sorts of errors you can get by extrapolating back the other way.

Wegner: In an MP stripper it is a factor of 50 upward in length. It could be 100 cm long, instead of two centimeters and we see a factor of five makes some modest change and you would expect that another factor of 10 might be appreciable. In fact other phenomena could come in and I guess that maybe the scattering effects could be different for that geometry.

Moak: We have some work that we are doing on multiple scattering and gas cells which will shed some light on that. I would like to recommend to you that you think about shortening the cell and making its density correspondingly higher so that you can have a larger accceptance solid angle so that you can utilize the higher charge states that are out there.

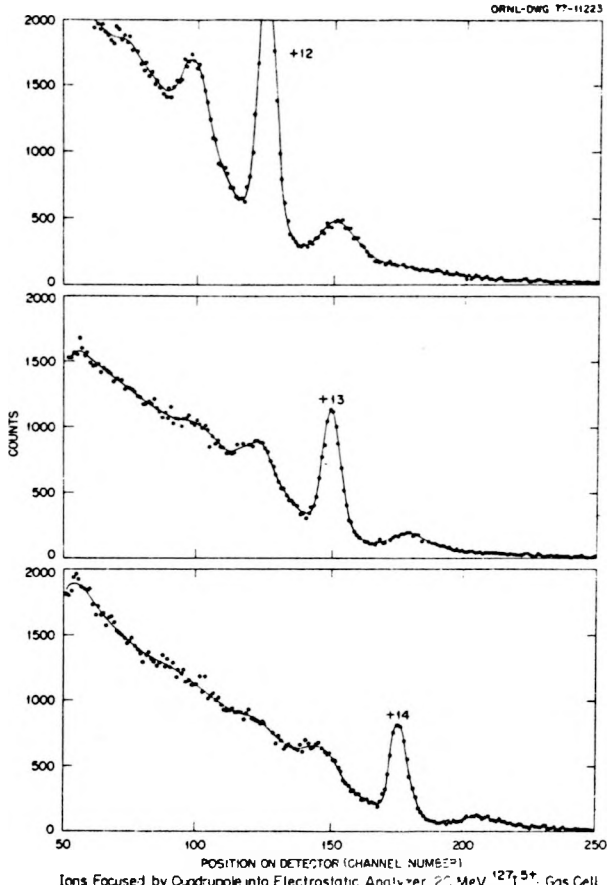
Saylor: I noticed that your charge-state distribution increases with half-angle and I wondered how fast particularly for the higher charge states. I wonder if you have any feeling for where that rapid rise begins to level off?

Moak: The data which you have seen this morning are anchor points in a sense that they are absolute and that they are fairly accurate. There are anchor points for a large mass of angular distribution data which we have taken and they pin down the curves in an absolute way, which we had not had before and we are now going to be able to develop this data on an absolute basis so our angular distributions will enable us to answer your questions directly. That is, how far out can you go in solid angle for a given charge state before you really don't gain anymore. Those lists will be made available.



Experimental Arrangement.

Fig. 1



Ions Focused by Quadrupole into Electrostatic Analyzer. 22 MeV $^{127}I^{5+}$; Gas Cell Length 9.4 cm; Ar Pressure 0.2 Torr; Emergence Cone Half Angle 8 mrad.

Fig. 2

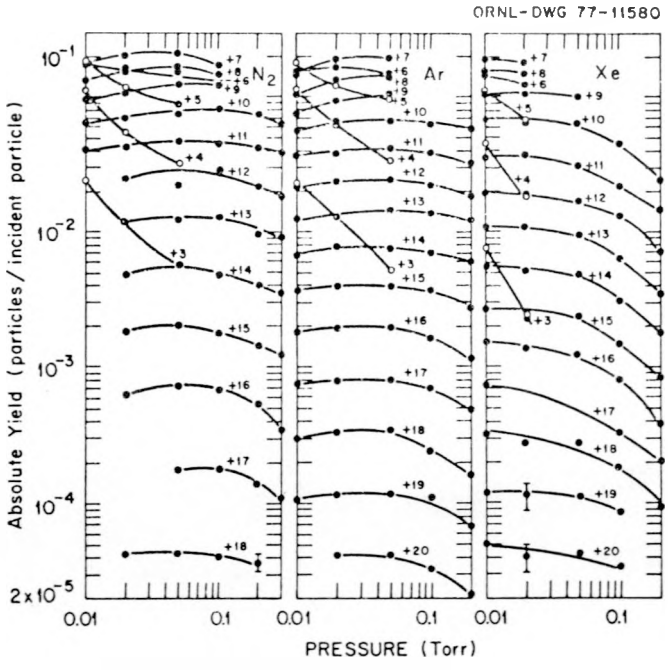


Fig. 3 Absolute Charge State Yields for an Emergence Cone Half-Angle of 4-milliradians, Gas Cell Length 9.4 cm. Input 20-MeV $^{127}I^{5+}$ Ions.

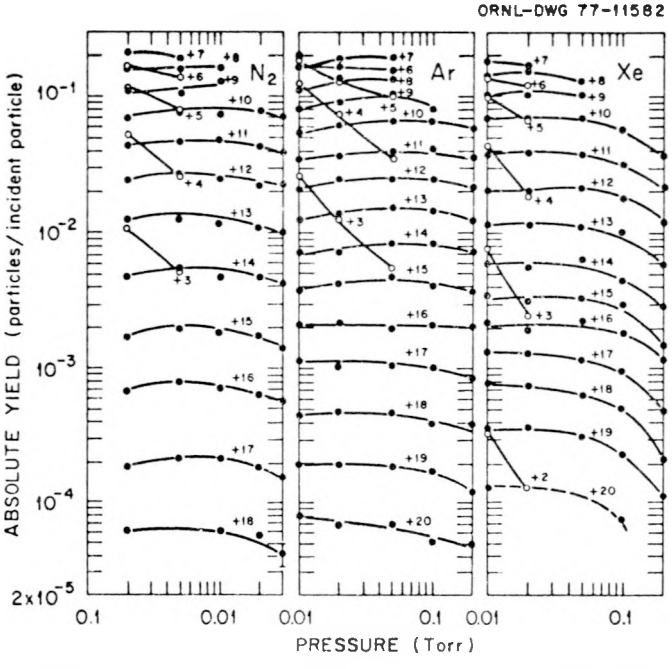
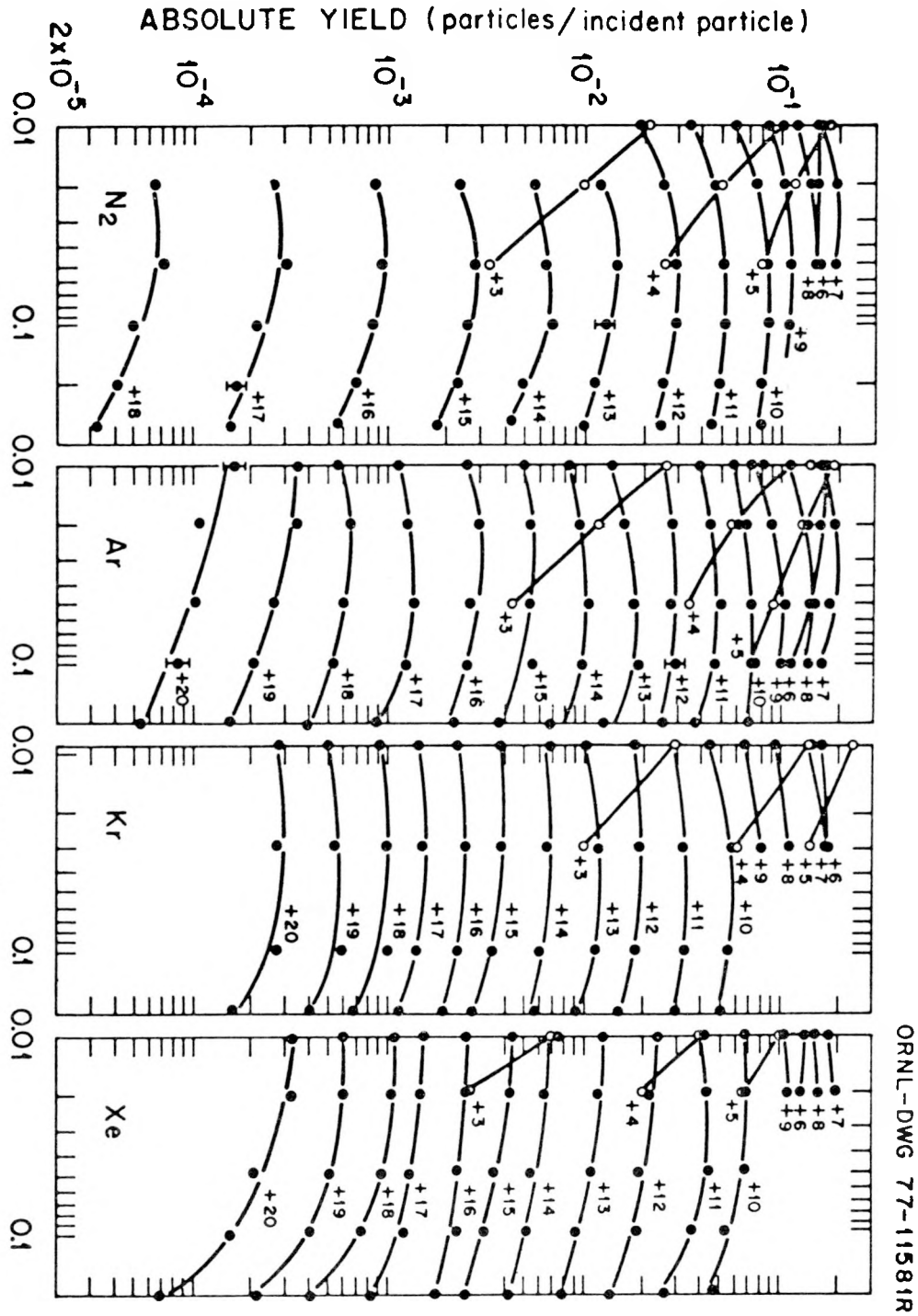
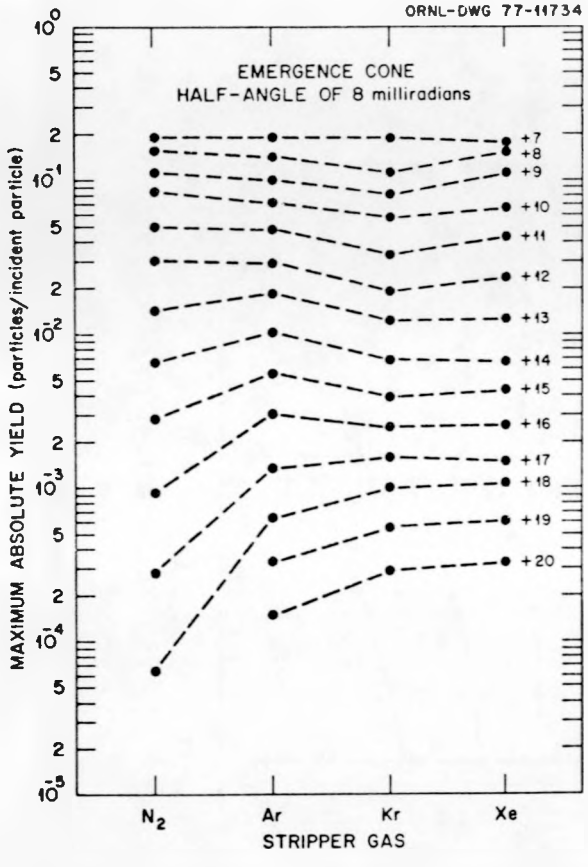


Fig. 4 Absolute Charge State Yields for an Emergence Cone Half-Angle of 6-Milliradians, Gas Cell Length 9.4 cm. Input 20-MeV $^{127}I^{5+}$ Ions.



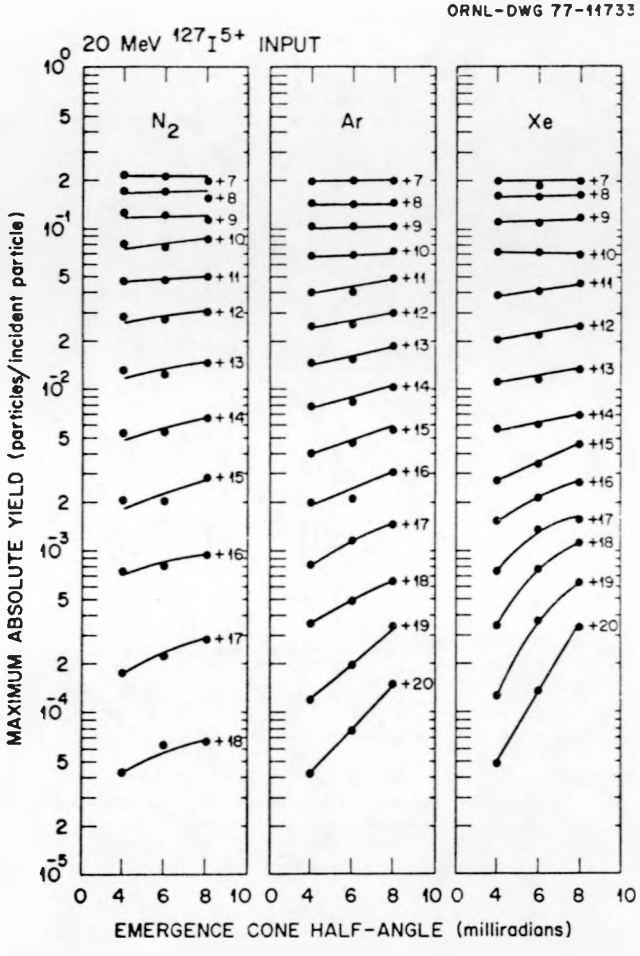
Absolute Charge State Yields for an Emergence Cone Half-Angle of 8-Milliradians. Gas Cell Length 9.4 cm. Input 20 MeV $^{127}I^{5+}$ Ions.

Fig. 5



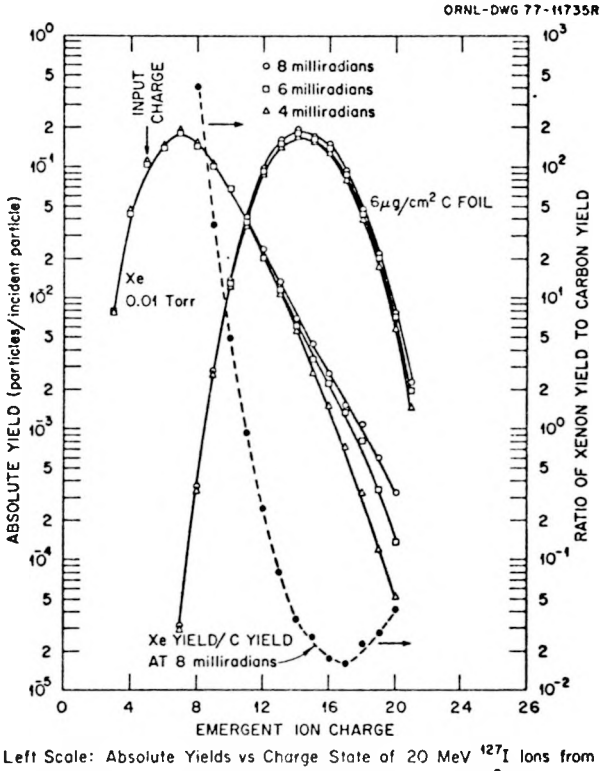
The Maximum Absolute Yield, Determined at the Optimum Gas Pressure for Each Charge State, of 20 MeV ^{127}I Ions from Four Target Gases. Dashed Lines, Connecting Points Representing the Same Charge State, Have Been Added as an Aid in Comparing the Yields from Different Target Gases.

Fig. 6



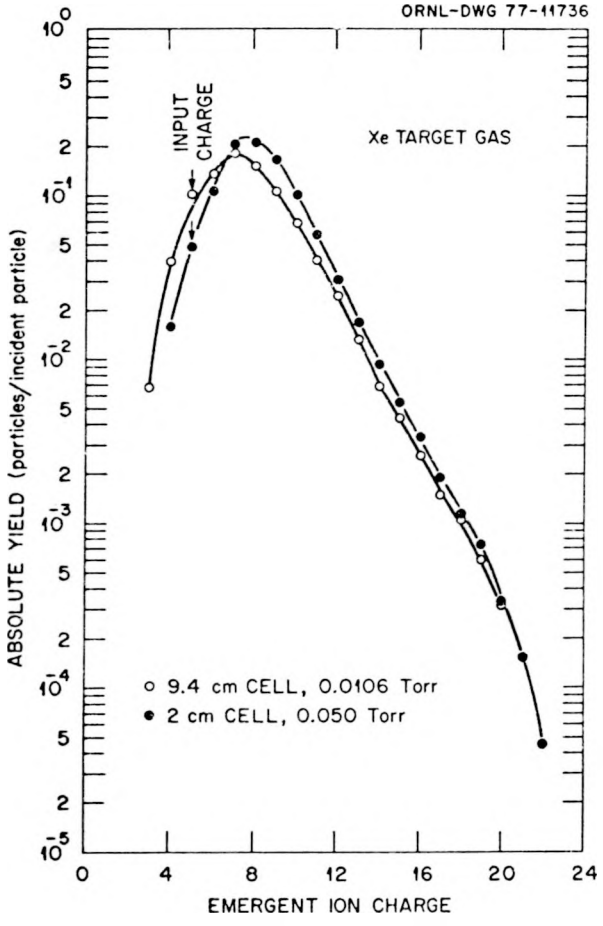
The Effect of Emergence Cone Half-Angle on the Maximum Absolute Charge State Yields of 20 MeV ^{127}I Ions from Three Target Gases.

Fig. 7



Left Scale: Absolute Yields vs Charge State of 20 MeV ^{127}I Ions from Xe (0.01 Torr, 9.4-cm Cell Length) and a C-Foil (6$\mu g/cm^2$) for Three Emergence Cone Half-Angles. Right Scale: Ratio of Xenon Yields to C-Foil Yields vs Charge State. 8 milliradians Cone Half-Angle.

Fig. 8



Absolute Yield vs Charge State of 20 MeV ^{127}I IONS from Cells of Equal Target Thickness (0.1 Torr cm) but Different Gas Densities.

Fig. 9

The Effects of the Coulomb Explosion on the Transmission of Molecular Ions through a Tandem*

R. Middleton

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Earlier this year I learned of the beautiful work of Gemmell, Vager and collaborators[1] at Argonne National Laboratory on the dissociation of fast molecular ions in thin foils. Even after listening to a talk by Goldring, at the Strasbourg Accelerator Conference, on the dissociation of OH^- ions in their 14 UD Pelletron accelerator I still did not fully appreciate the effects of the Coulomb explosion and its impact on transmission. Since then I have performed a few calculations and made some measurements on a variety of molecular negative ions in our FN and it is about these that I wish to talk.

Fig. 1 illustrates the effect of a Coulomb explosion for the extreme cases of an aligned molecule striking a foil and one oriented perpendicular to the beam direction. In the former case, the energy released by the explosion boosts the energy of the leading ion and decreases that of the trailing one. It is easy to show that the energy difference of either ion, depending on whether it strikes first or last is:

*Work supported by the National Science Foundation.

$$\Delta E = \frac{4}{m_1 + m_2} \sqrt{m_1 m_2 \epsilon E} \qquad (1)$$

where m_1 and m_2 are the masses of the ions, E is the molecular energy and ϵ is the energy released by the Coulomb explosion

$$\epsilon = \frac{k Z_1 Z_2 e^2}{r}$$

$$\approx 14.4 Z_1 Z_2 \text{ eV} \qquad (2)$$

where Z_1 and Z_2 are the charges of the ions and r is the molecular spacing (assumed to be ≈ 1 Å).

In the second extreme case the most important effect of the explosion is to introduce an angular spread. It can readily be shown that:

$$\theta_1 = \sqrt{\frac{\epsilon m_2}{E m_1}} \text{ and } \theta_2 = \sqrt{\frac{\epsilon m_1}{E m_2}} \qquad (3)$$

Thus, the net effect of the Coulomb explosion is to produce a cone of particles of half angle given by equation 3 and with a continuous energy spread up to the maximum value given by equation 1. The experiments about to be described basically confirm this simple picture with the exception that the electron wake generated in the foil by the leading ion produces some curious and subtle effects.

Consider the example of $^{12}C_2^-$ ions accelerated to 6 MeV and stripped in a carbon foil. The average charge states would be about 3 and equation 2 tells us that the Coulomb energy is ~130 eV. Substituting this value into equations 1 and 3 leads to an energy spread of 56 keV and an angular spread of 0.27°.

To study this case experimentally an HVEC beam profile monitor was introduced immediately prior to the analyzing slits of our 90° magnet and a C_2^- beam was accelerated to a terminal potential of 6 MV and stripped in about a 5 $\mu g/cm^2$ carbon foil. Fig. 2 shows the x (upper) and y profiles of the analyzed C^{3+} beam. The drawing was obtained by tracing directly from a polaroid photograph of the display oscilloscope.

It will be noticed that not only is the x profile appreciably broader than the y but that it has three distinct peaks. Later we were able to obtain a rough energy calibration of the x sweep and the energy separation between the extreme peaks was determined to be between 50 and 60 keV, which is in quite good agreement with the value of 56 keV predicted by equation 1. The reason that separate peaks are observed rather than a single broad peak is that preferential selection of ions traveling in the forward direction is occurring. Initially we thought that it was necessary to close the entrance slits of the 90° magnet to about ±0.5 mm and to de-focus slightly the high energy quadrupole to observe this effect (this is how fig. 2 was obtained). However, we later discovered that this was usually unnecessary and all that was required was to adjust the high energy quadrupole to produce a narrow image in the x-direction.

The origin of the center peak became clear when a small amount of gas was admitted into the stripper tube preceding the carbon foil. Fig. 3 shows the effect of a gradually

increasing gas flow on the C^{2+}, C^{3+} and C^{4+} beams (traces corresponding to a particular charge state are approximately to scale but not relating the various charge states). It can be seen that the central peak grew rapidly and by the time the high energy vacuum had risen from a base pressure of 6×10^{-7} to about 1.6×10^{-6} Torr not only dominates but appears to be the only peak. The effect of the gas is to cushion the Coulomb explosion allowing it to develop slowly and the molecular constituents to drift apart prior to striking the foil.

To test the practical significance of using a low pressure gas – carbon foil stripper combination the entrance slits to the 90° magnet were opened to their full extent and the exit slits to ±1.0 mm (our frequent operating values). The C^{3+} beam was then carefully maximized without gas resulting in 1.15 μA. Gas was then slowly admitted and the beam current increased to a maximum of 2.15 μA at a high energy pressure of 1.6×10^{-6} Torr. Further increase in the gas flow caused a slight reduction in the current.

It will probably already have been noticed that the low energy peaks (left side) in fig. 3 are consistently more intense than the high energy ones. This phenomenon has been exquisitely accounted for by Gemmell and collaborators[1] and is a result of the electron wake in the foil. The latter creates an oscillatory electric field which acts most strongly on the trailing ion tending to reduce its angle with respect to the beam direction – hence increasing the population

of the ions in the lower energy peak. It may also be noticed that the asymmetry increases with increasing charge state – presumably due to the larger wake.

The extremely weak low energy peak, visible in some of the traces shown in fig. 3, arises as a result of the inadequate resolution of the negative ion inflection magnet which permits the simultaneous acceleration of ^{12}C $^{13}C^-$ and $^{12}C_2^-$. The peaks correspond to $^{12}C^{3+}$ and $^{12}C^{4+}$ respectively which have been accelerated to the terminal accompanied by a ^{13}C atom. They provide a very convenient energy calibration since the energy separation from the main peak is 120 keV.

Fig. 4 shows some similar results obtained while accelerating BO^- ions generated from a natural boron sputter cone. Here again we were fortunate to have a weak calibration peak, lying some 137 keV above the main O^{3+} peak, corresponding to O^{3+} that had been accelerated to the terminal accompanied by a ^{10}B atom. This enabled the energy separation of the two O^{3+} peaks observed without stripper gas to be determined to be about 60 keV, in excellent agreement with the calculated value of 55 keV. It is not understood why, without stripper gas, the intensities of the low and high energy B^{3+} peaks are comparable while for O^{3+} the lower energy peak is almost double that of the higher energy peak. Measurements made on the B^{3+} beam with the analyzing magnet slits adjusted for normal operation showed that the addition of a small amount of stripper gas increased the analyzed beam by a little over a

factor of two.

Fig. 5 shows some similar measurements made while accelerating NH_2^- ions. Here the simple theory outlined earlier is not applicable since three ions are involved in the explosion and we frankly didn't know what to expect. The clearly defined triple peak structure for H^+ came as quite a surprise and it was only after considerable thought that it was realized that the problem is quite different from the 3-body process frequently encountered in nuclear physics — indeed the problem is exactly solvable providing the locations of the three constituents are known at the time of the "explosion". For example, if it is assumed that the shape of the NH_2^- molecule is a line with the nitrogen atom located at the center, then it is simple to show that for the protons:

$$\Delta E = 2\sqrt{\frac{2m_1}{2m_1 + m_2} \cdot E\epsilon}$$

where $$\epsilon = \frac{2k\ Z_1\ Z_2\ e^2}{r} + k\ \frac{Z_1{}^2 e^2}{2r}$$

and the subscripts 1 and 2 refer respectively to the proton and to ^{14}N. Assuming $Z_1 = 1$, $Z_2 = 4$ and $r \approx 1$ Å leads to a value for $\epsilon \approx 122$ eV and, at a terminal voltage of 6 MV, $\Delta E \approx 19$ keV (close to the experimental value — see fig. 6). However, it may be noted that this model predicts that no energy spread should be introduced into the nitrogen beam which was contrary to what was observed. On the left hand side of fig. 5 are shown the profiles of the N^{3+} beam formed after traversing a carbon foil with and without gas. As expected,

no structure was observed in the latter case, since even if it existed at the 19 keV level, it would not have been resolved. However, the peak observed with a foil alone is noticeably broader than that observed with foil and gas indicating that the Coulomb explosion did introduce an energy spread into the N^{3+} beam. It is noteworthy that in this case the addition of stripper gas caused a 20% reduction in the intensity of the N^{3+} beam. This is believed due to the fact that the Coulomb explosion introduced little angular spread — the major factor governing transmission — and worsening vacuum in the high energy tube inevitably results in poorer transmission.

Fig. 6 shows some very recent results obtained after it was realized that NH^- and NH_2^- ions could be simultaneously accelerated and, by juggling the ion source parameters, their intensities could be made comparable. The figure is a xerox copy of a photographic negative obtained by re-photographing the polaroid photographs of the oscillograph traces — in other words raw data. The top trace corresponds to a foil alone and the lower ones to the effects of gradually increasing the stripper gas flow. As expected, the effect of the latter was to increase the strengths of the two central weak peaks which are just discernable in the upper trace. The bottom trace was obtained with a high energy vacuum of about 2.5×10^{-6} Torr.

The advantage of simultaneously observing the protons from NH^- and NH_2^- is that their energy difference is exactly

known and at a terminal voltage of 6 MV is 25 keV — thus providing a convenient energy calibration. The energy separation of the proton peaks arising from NH^- was measured to be 19 ± 1 keV and is in excellent agreement with the value of 18.5 keV calculated from equation (1) assuming 4^+ for the average charge of the nitrogen ion. The energy separation of the proton peaks from NH_2^- was found also to be 19 ± 1 keV and is in good agreement with the calculated value assuming the molecule is in the form of a line. An approximate calculation of the proton energy difference was also made assuming that the molecule was in the form of an equilateral triangle. This yielded a value of 18.5 keV which is remarkably similar to the value obtained for the aligned case (— the result of a fortuitous cancellation. As the molecule is deformed from a line into a triangle the Coulomb energy increases but the nitrogen ion takes an increasing share of the energy thus maintaining the proton energy nearly constant.).

Finally, some even more complex results are shown in fig. 7 for protons arising from the dissociation of $Mg\ H_3^-$ ions. Although the effects of the explosion in the foil alone (lower trace) are very evident the energy separation of the peaks appears to be less than in the cases of NH^- and NH_2^-. Also, the center peak is much stronger — presumably due to the residual gas in the stripper tube having a larger effect on a slower molecule.

Conclusions

Whenever foils alone are used to strip a molecular ion

beam in a tandem an energy spread ranging from 20 to over 100 keV will inevitably be introduced into the beam over and above the energy spread caused by straggling. It may be noted that this energy spread increases more rapidly than the $E^{1/2}$ term in equation (1) owing to the dependence of Z_1 and Z_2 on E. Intensity losses may also occur as a result of the induced angular spread and might become quite severe in cases where the constituents of the molecule have comparable mass. In the absence of charge state selection in the terminal, a light unwanted ion accelerated accompanied by a heavy ion might acquire a sufficiently large angular spread that most of it dumps in the high energy acceleration tube leading to excessive loading. The present work suggests that most of these problems can at least be alleviated by using a relatively low gas flow stripper located before the carbon stripper foil.

Reference

1 Zeev Vager and Donald S. Gemmell, Phys. Rev. Letts. 37, 1352 (1976).

Discussion:

Chapman: Thank you very much. From a purely practical point of view with the physical dimension limitation in the terminal, you only require a small amount of gas stripping prior to the foil. Is it your feeling that one could manage with a relatively short gas stripper?

Middleton : I am sure you could. Typically when using a gas stripper the high-energy vacuum is of the order of 7×10^{-6} or 1×10^{-5} torr. We found that in most of the cases we looked at that one only needed to raise the pressure very slightly, almost imperceptibly, from 6×10^{-7} to about 1.6×10^{-6} torr. In fact going to a higher pressure invariably started a worsening trend. There was a very distinct optimum.

Chapman: Is it your feeling that shortening the stripper tube too much might nullify the effect that you are trying to obtain?

Middleton: Well, obviously, any gas that is introduced is detrimental in one sense. It's beneficial in that it will cushion the coulomb explosion. One would clearly like to cushion the explosion with a minimum amount of gas. But I think your point is--couldn't I get away with a shorter tube--and I think the answer is probably yes.

Richardson: Where do you measure your vacuum when you talk about the 1.6×10^{-6} torr?

Middleton: This is measured at the usual position with an FN essentially over the pump at the high-energy end.

Larson: I want to expand on Ken's question a little further, have you made any calculation as to what are reasonable distances over which the cushioning effect should take place, that is, how long a distance should one get for the ions to come apart with their low-charge states before hitting the stripper?

Middleton: Maybe C. Moak can answer that better than I . Since a foil is typically 200Å thick, I think anything substantially larger than 200Å will be all that is required. In other words 2 cm or 10 cm, would you agree, Charlie? I mean anything that is a few orders of magnitude larger than 200Å—which means virtually anything.

Larson: Are you suggesting that all the explosion effect occurs within the foil itself or is it happening after the ions come out of the foil?

Middleton: That is quite an interesting question. I think both. The transit time of the ions through the foil is about 10^{-15} seconds. If you start to calculate how the coulombic explosion developed in that time you find approximately half of it is developed. So some of it occurs in the foil and some of it occurs out of the foil. I think that there are some very subtle and strange effects that are going on.

Larson: It seems all that is necessary is to bathe the foil region with a little bit of gas. This need not be a stripper in the conventional gas stripper sense.

Middleton: I think that is a very inefficient way.

McKay: Did you say your normal slit setting is plus or minus 1 mm? I think that is very wide.

Middleton: I think we normally work slightly less than that, I think about 35 mils is what we normally work with on the analyzing magnet.

McKay: We go down to as low 10, but the other day I was running up in that region because our terminal stablizer was out. Do you use a terminal stablizer?

Middleton: No.

Lindgren: P. Thieberger has some work to present later on in the week showing work we have been doing along these same lines. We have made measurements using the oxides of boron, aluminum, iron,and nickel to inject into the machine. We measured the effects of adding gas to a foil and we found that on all of these we have been able to increase the analyzed beams of the higher charge states by a factor of five to twenty-five. On boron for instance, I can remember taking a curve of the various charge states where charge 2 and 3 did not come up at all with adding gas. They were just best with gas alone. Charge 4 and 5 came up,I think,by a factor of 15.

Middleton: We observed in the case of 3 plus boron something like a factor of three under normal working conditions in an FN. I think that in the MP, because of substantially longer acceleration tubes and the distance in the terminal that you very preferentially select the forward direction. I think the angular spread introduced by the coulomb explosion can be much more deadly in an MP than in an FN.

Moak: Did you try different gases?

Middleton: No, all this has been done with oxygen.

Moak: I think that it might be interesting if one could try different gases because the explosion might have a slightly different character and one could perhaps learn more of the particulars and the details of the explosion in this manner.

Middleton: I am sure you are probably right that one might learn some very interesting things with different gases, but this picture here is of poor resolution. If one could take this data with good resolution, each of these peaks should be a multiplet corresponding to the various charge states and the different energies of the explosion. If one had good resolution, this should have a structure like this, where this would correspond to the C^{2+} and the C^{3+} would be the strongest and over here you would be going into the C^{4+} and the C^{5+}. In principle one might really learn some very interesting things if one could get to this resolution which requires about 2 to 3 kilovolts

resolution. The most interesting would be--does the trailing ion behave differently from the leading ion?

Moak: Don't you think that there would be an advantage in doing such an experiment with the negative molecular ions from a single-ended MP?

Middleton: All of the work done at Argonne has been with positive ions from the dynamatron and there, of course, you can build enough sophisticated apparatus at ground potential and do very, very precise measurements. I guess if one wants to study negative ions the Brookhaven facility is close to unique.

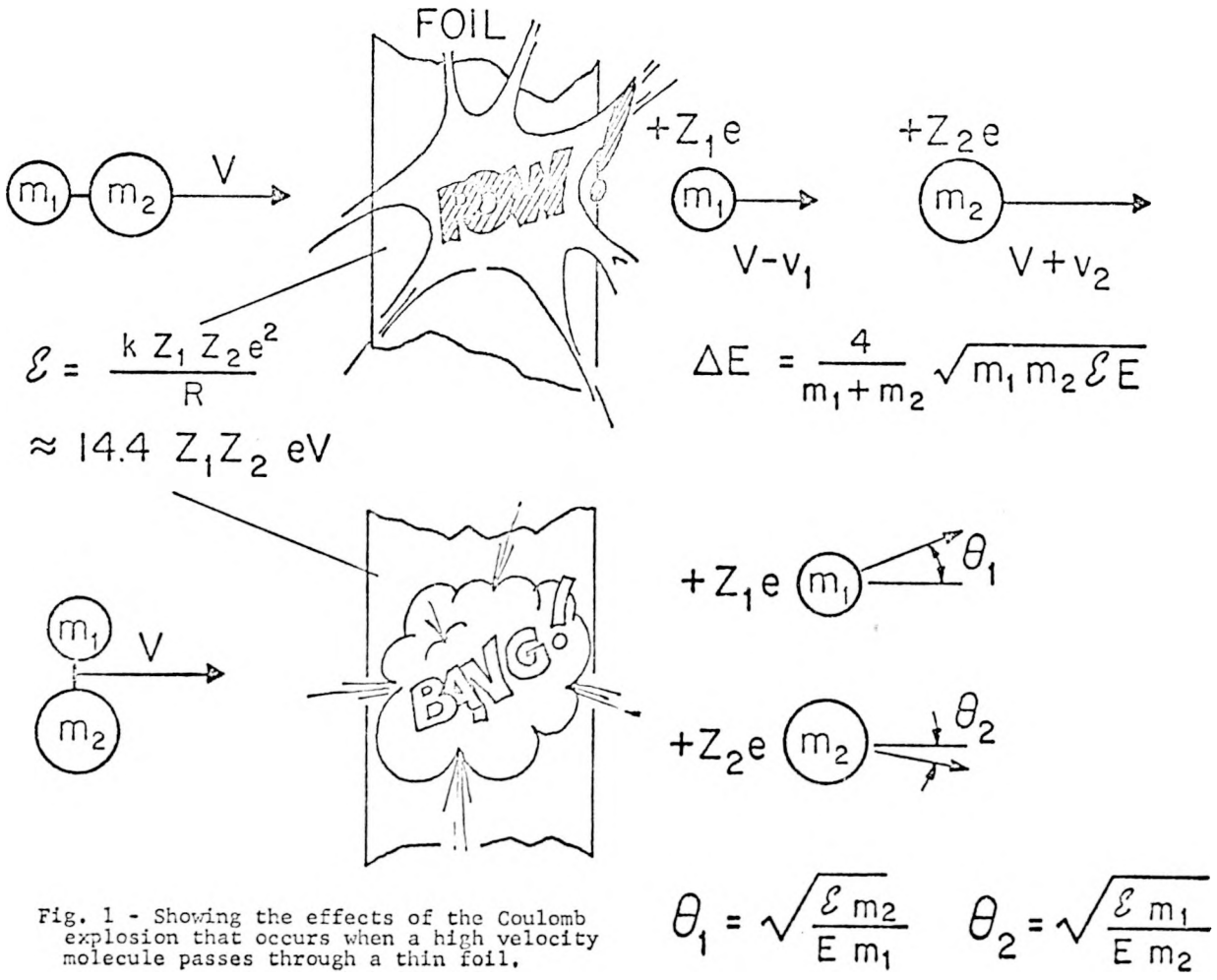

Fig. 1 - Showing the effects of the Coulomb explosion that occurs when a high velocity molecule passes through a thin foil.

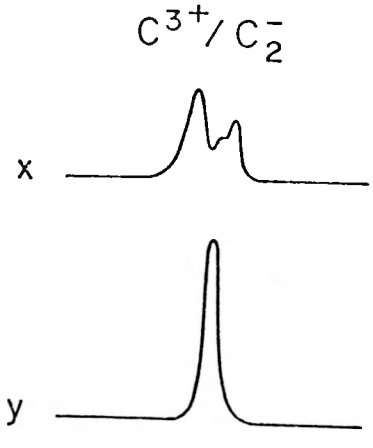

Fig. 2 - The x and y beam profiles obtained from a profile monitor located immediately in front of the 90° magnet analysing slits. The negative ion beam was C_2^- and the analysed beam 21 MeV C^{3+}. Stripping was in a carbon foil of about 5 $\mu g/cm^2$.

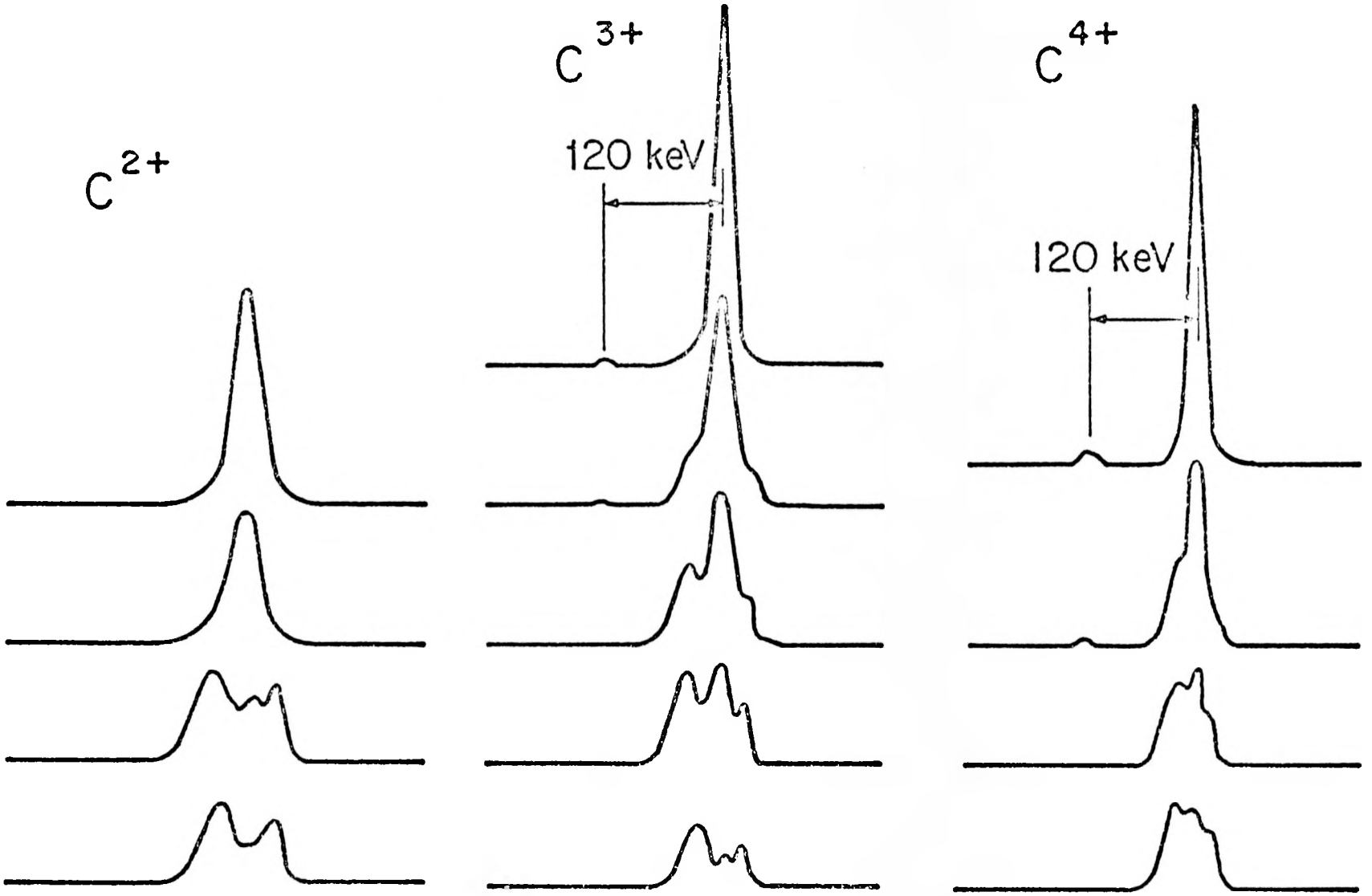

Fig. 3 - The x profiles for analyzed beams of C^{2+}, C^{3+} and C^{4+} from injected C_2^-. All were obtained at a terminal potential of 6 MV using a carbon stripper (lower traces) and show the effect of adding a little gas to the stripper tube preceding the foil.

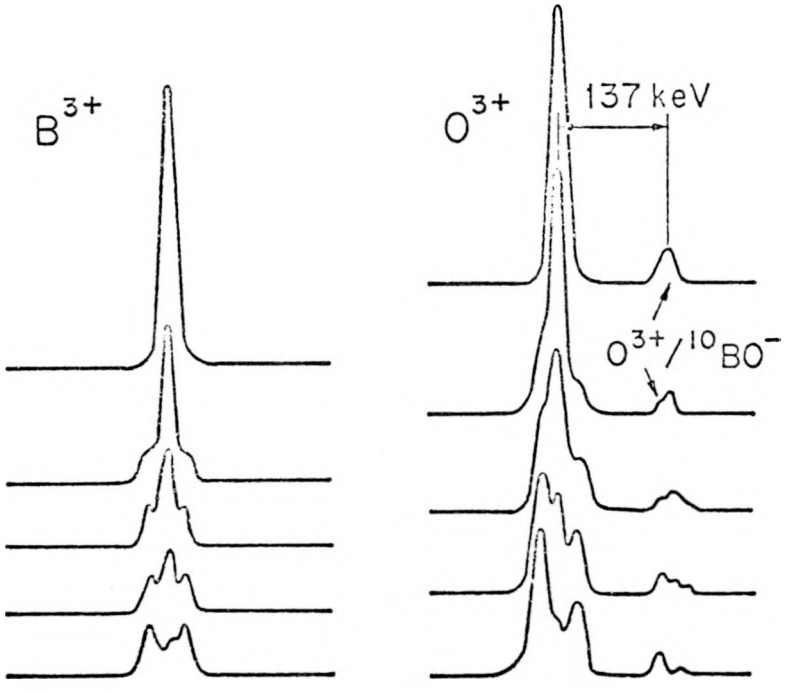

Fig. 4 - Results similar to those shown in fig. 3 obtained while accelerating BO^- ions with a terminal potential of 6 MV.

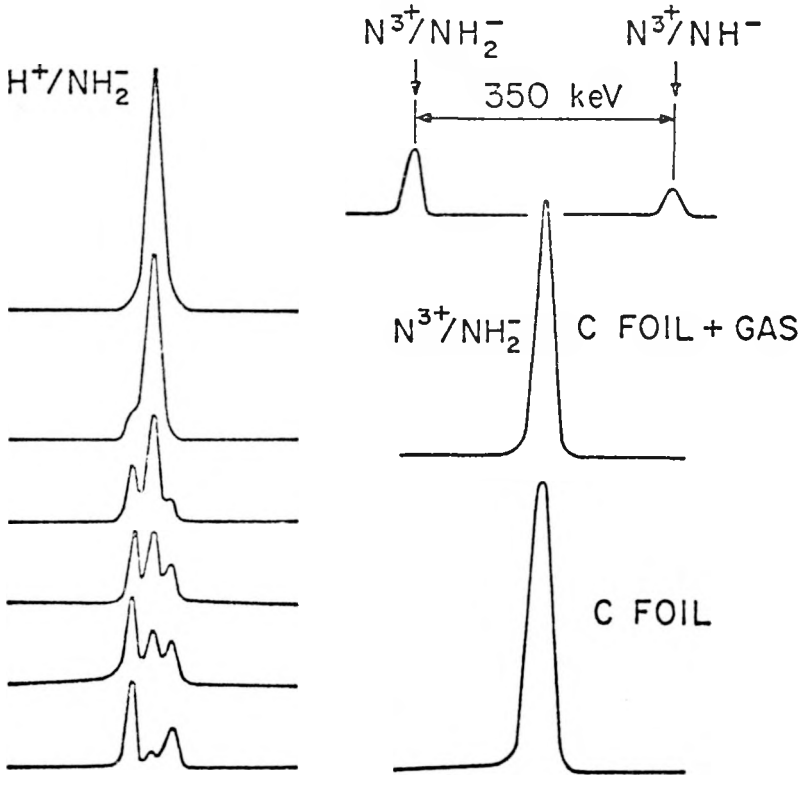

Fig. 5 - Results similar to those shown in fig. 3 obtained while accelerating NH_2^- ions – the terminal potential was 6 MV.

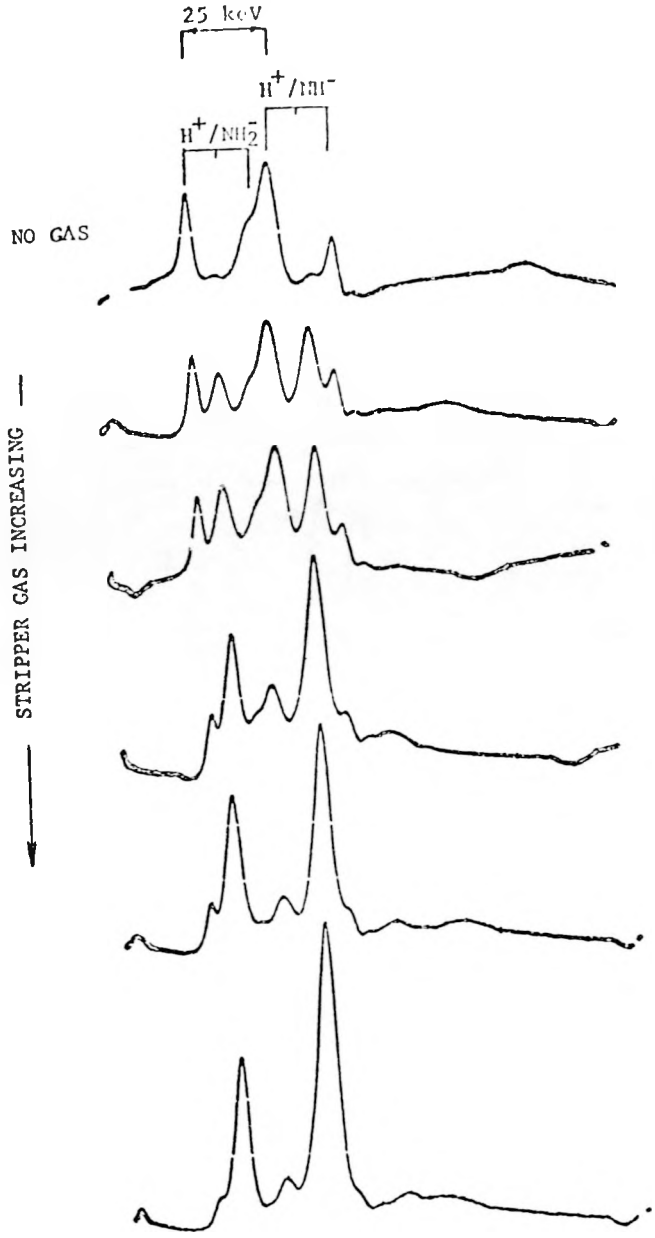

Fig. 6. Negative photograph of beam traces of protons from the simultaneous acceleration of NH^- and NH_2^- ions.

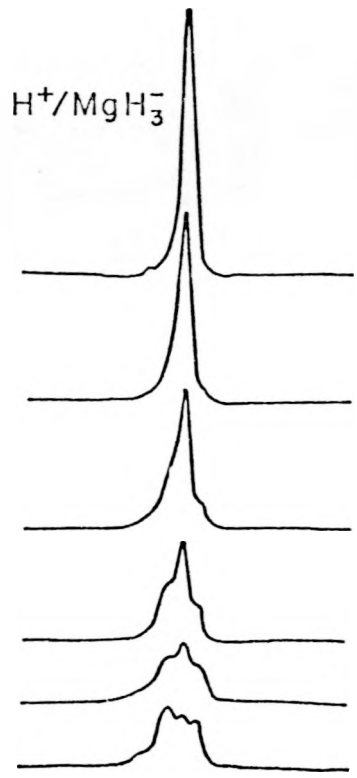

Fig. 7 - Results similar to those shown in fig. 3 for protons arising from the dissociation of MgH_3^- ions accelerated through a potential of 6 MV.

Session II, Chairman, B. Billquist

STAFFING FOR OPERATIONAL SAFETY AND EFFICIENCY

Richard Woods
Los Alamos Scientific Laboratory

I. Safety

With respect to the operation of Van de Graaff accelerators, the general topic of safety can be reduced to three primary areas of concern. They are: radiation safety, personnel safety, and equipment safety. The first of these, radiation safety, will not be dealt with since the rules and regulations concerning radiation limits are well defined and uniform among laboratories. The NCRP has a new book out on operation of accelerators between 0.1 and 100 MeV. The last two categories, personnel and equipment safety, are not dealt with in a consistent manner from one lab to the next and in some cases these safety areas are ignored.

Accelerator facilities have four states of operation as shown in Table I. The first case involves only equipment safety. Here we are dealing either with the catastrophic problems of fire, flood, power failure, etc., or failure of specific equipment, which will lead to damage to other equipment. At LASL, this case is handled by a system which involves sensors attached to critical equipment such as the circulating chilled-water system. These sensors report to a trouble board which is always manned and serves the entire Laboratory. A telephone-call list is posted at the trouble board. The fire monitoring is done through a fire alarm and built-in sprinkler system. In addition we require a complete walkthrough and inspection of the facility once every twenty-four-hour period. This is usually sufficient to find the dry liquid nitrogen trap or the small leaks in the water system.

The second case takes care of itself. The third case gets into basic operating philosophy, namely, who is responsible for the operation of the accelerator. Table II shows the broad categories of operators. This can be an operator who is hired solely for this purpose and whose attention is rarely needed elsewhere. A second possibility is for the staff supervisor or a technician to look after the accelerator but who has other duties requiring long periods of absence from the console. In this scheme the user has minimal responsibility and generally calls someone when the slightest thing goes wrong. The third situation is where the experimenter/user is responsible for the accelerator as well as doing the experiment. I consider all of these to be viable alternatives in descending order of desirability. During normal working hours there are sufficient personnel in the building to guarantee a safe condition as long as someone has accepted the responsibility. The fourth state of operation is the most crucial and really determines the operating philosophy of a given facility. Here at LASL, the minimum requirement is for two people to be present at all times when the belt is running. We have 3 operators who are

specifically hired to operate the two accelerators and they work on a rotating-shift basis during the normal five-day week. The shift assignment rotates every week. There are two philosophies on rotation: one, you rotate often so the midnight to 8:00 a.m. operator does not become completely disoriented or estranged from the operation of the group, or two, this operator stays on the shift until acclimated and then stays on long enough to make the pain of acclimation worthwhile. The second person needed to fulfill the two-man rule at LASL is called a "Qualified User." This person is required to read the standard operating procedures manual, attend a one-day indoctrination course on the facility, and run on the accelerator for 24 hours before being left as the second person. The purpose of this program is twofold. It makes the individual a more useful person in terms of helping the operator if there are problems and it acquaints the person with what the facility has to offer to make it easier to do an experiment.

The weekend, holiday, and vacation problem is common to all of us. Our partial solution to this has been to hire two local science teachers as casual operators. These people are available during the summer to fill in for vacations and one is usually available during school vacations in the winter. On the average they also work one shift per weekend. We also offer 8 hours of overtime to our regular operators strictly on a volunteer basis. In this way we are able to cover 4-5 shifts of the 6 shifts on a weekend with operators. The remaining shifts are covered by a select group of people with the title of "Qualified User/Operator." These are experimenters who use the accelerators on a continuing basis and are very familiar with the operation. They take a special course on accelerator operation. These individuals can also fill in on an emergency basis. In our case the Qualified User/Operator is a member of the team using the accelerator but his primary responsibility is to the accelerator when on duty as the operator. There must be another Qualified User present who is responsible for the experiment.

Our accelerators are scheduled for four-week periods with the scheduling being done on the Tuesday preceding the beginning of the four-week period. The operators are assigned at that time and the user teams know which shifts they must provide Qualified User/Operators for.

With only three operators on a regular basis the training of new operators has been a problem as well as the continuing education of the existing personnel. With rotating shifts and operating seven days a week it is very rare for all the operators to be present on the same shift. The only times this happens is when the accelerator is down for maintenance, and then all available manpower is utilized to get the machine running again. In our case it also means both accelerators must be down at the same time since our operators are responsible for both machines. In an effort to improve the operator's understanding of electricity and magnetism, as well as nuclear physics, I have tried several programmed textbooks which are designed for self-study without any classroom lectures. Unfortunately, the diverse backgrounds in math of the individuals employed as operators have rendered this technique useless without a lot of individual help. At the present time, our new operator training is limited to several weeks of general indoctrination, followed by four weeks of working with an experienced operator, and then the new individual is on his own. Any further training is done on the job as needed with very little opportunity

to impart significant knowledge in a new area.

II. Efficiency

The efficient operation of our facilities is of prime concern to all of us. By efficiency I mean the minimization of the time the beam is not available for use by the experimenter. In the short-haul, breakdowns are inevitable, but over a long period, it is the resources that are available to handle these emergencies and general maintenance that determine the efficiency. The most important resource is the magnitude and quality of the people involved. This room is filled with the leadership people who affect efficiency the most. Assuming we are all perfect managers, let us move on to the kind of staff we should have to support us. The common denominator I have chosen to use to discuss staffing levels is men per MV. Table 3 gives the factors and some examples. To find the numbers for a given facility, add the total maximum terminal voltage available from one or more machines and multiply by the factor given. The numbers include the percentage of time each individual at a facility spends on accelerator maintenance, operation, and improvement. The emphasis on ion source help should be noted and is probably the most difficult to justify to upper management. However, the ion source end of the accelerator is the most important for maintaining a viable and up-to-date facility. Accelerator tube development is conspicuously absent. The availability of a selection of commercial tubes has relieved us of that aspect of development.

How we deploy our resources is the next step toward efficiency. I feel that a regularly scheduled maintenance period is extremely important. Our maintenance period is the first working day of the week from 8 to 4. This is never changed regardless of the "I only need four hours to finish my experiment" syndrome. This is very valuable to people outside the operating group such as building maintenance crews, janitorial staff, health physics personnel, and users who live in buildings away from the accelerator. We find this to be the time to recover from the problems which developed through the weekend and to prepare the ion sources for the coming week. A lithium boiler can easily be replenished and an exotic beam worked up during this time. All routine maintenance is done during this time, and if we need to go into the accelerator tank for a short period such as foil replacement, then we pump the tank out on the preceding shift so it is ready to work on at 8:00 a.m., if we have a project next day. I also find that the users do not get upset if this all appears on the schedule determined by the demand on the accelerator. In our scheduling process we ask for projections two months in advance and this allows us to plan a week or more downtime with minimum inconvenience to the users. During major overhauls we utilize all available people and run two shifts a day if possible. There are so many things which have to be done sequentially and the space is limited, so the utilization of two crews is more efficient.

Unscheduled maintenance or breakdowns do occur even with a program of preventive maintenance. The breakdowns during the normal working day can be handled by people on site. The off-hours and weekends can either be set up on an assigned or if-available basis. Obviously the assigned basis where someone is on call at all times is the best. We have never operated under this system at Los Alamos. We do coordinate vacations and out-of-town weekends so that everyone with the same area of expertise is not gone at the same time. The operator on duty starts by calling the accelerator supervisor and then works down the list of experts for the problem until he finds someone at home. Most

problems can either be dealt with over the phone, or it is a simple decision to shut down. The decision to come in to fix the problem is up to the accelerator supervisor and the individual. We consider the weekend operation as a bonus, and if everything goes well, fine, but if it breaks down, there isn't any obligation to fix it. In over ten years of operation this system has worked well for us.

The standard by which we measure efficiency is to take the amount of accelerator time that is available and divide into the number of hours the beam is available for use for experiments. Another number of interest is the percent of time the experimenters use the beam based on the time the beam is available to them. Our experience gives a number of 73% for this latter number.

I would like to mention one other area which I have referred to several times, and that is scheduling of machine time. We have a scheduling committee which is composed of the accelerator supervisor for each accelerator, three representatives from different experimental groups, and the individual in charge of overall operation as chairman. This committee meets if there are any problems. The final schedule is prepared by the chairman and then posted for comments or changes. When this system started, there was a complaint meeting scheduled where anyone could come and protest the schedule. Over a several-year period no one showed up so they are no longer scheduled. We give people 24 hours to complain and then the schedule is considered set. After that, switching around of the schedule can only be done by mutual agreement of the parties involved. It is our general rule that schedules are not slipped if someone loses time because of machine breakdown.

Discussion:

McNaught: I had a question about your rule that two people must be present all times the belt is running; just how rigid are you? We have a similar kind of rule in our lab, but, for instance, if everything is running smoothly and they don't anticipate making any changes, one person can go for lunch and back. Do you relax it to that extent?

Woods: No, we do not relax to that extent; we relax to the extent that the second person can sleep in the building. He must be available to the public address system, and he may not leave the building. As a matter of fact, the rule is that if the person is going to leave the building the operator must shut off the machine, and he has that right.

Moak: I wanted to ask a question about the users. They say that there was this small industrialist out this way who filled out this affirmative action thing asking what the total number of employees you have broken down by sex, and he said none, but we have a few people broken down by alcohol! Could you breakdown the users group in terms of outside participants, the visitors from Universities and local personnel?

Woods: I believe roughly 70% of the time on the accelerator is used by the experimentalists attached to the operating group, roughly another 20% is used by other groups within the Laboratory, and 10% is used by people outside the Laboratory. In most cases, it's a collaboration with somebody within the group or within the Laboratory. There are very few wholly outside users. There has been some misconception concerning coming to do an experiment at Los Alamos. If

you have an experiment you wish to do here you have to write a small proposal. It's nothing like you have to write to LAMPF. We want to know in 50 words or less what you plan to do and how you plan to do it and how long you think it will take. This can be submitted to our group and it is reviewed by our staff and also our division office. The criterion is that the experiment should have some relevance to ongoing programs at Los Alamos or that the experiment requires something which is unique to Los Alamos, such as a tritium beam.

Liebert: What provisions have you made for working on high-voltage equipment in terms of by-passing interlocks? We have this problem in industrial environments and I am curious to know how you would work with new pieces of equipment that you are putting on line and adequately protect your personnel simultaneously?

Woods: There are rules as to exactly what voltage and current levels people can work on by themselves and I believe it is 300 volts dc or less with 60 milliamps the maximum current, and less than 5 joules of stored energy, depending upon the filtering. A person can work on that type of equipment alone. When you get above these levels you are required to have two people present in sight of each other, or in other words, somebody to get you off the high voltage. Our two-man rule guarantees that there are two people there when any maintenance needs to be done. If the operator has a problem he needs to fix and it is a high-voltage power supply then he has to wake up the qualified user to go down with him and stay with him while he does whatever he needs to do in terms of maintenance. Everybody has cardiovascular pulmonary resuscitation training through the Laboratory safety program.

Stark: I would like to know how you enforce the rules.

Woods: That has to do with the quality of people. It's very difficult. The most recent electrical accident that happened here at the Laboratory is one that scared me more than any I have ever heard of. This involved an individual who was the most knowledgeable possible individual on a piece of equipment he was working on. There were roughly six people standing in the room, one person was even assigned to be his second man, and he was electrocuted. He stuck his head in the cabinet. He knew what was in that cabinet better than anybody else. And it was just the momentary lapse when he suddenly thought, I know what's wrong with that circuit and into the cabinet he went. I think that could happen to me very easily.

Bair: I have a trivial question about impurities--What are Science Youth Days?

Woods: This is a three-day period where roughly six-hundred high school students from the state of New Mexico descend upon this Laboratory and they are given indoctrination movies, talks, and then those facilities which are not within classified areas are open for tours, and the Van de Graaff was, of course, the first facility out of security and so we have always been on the list. We actually only get 300 of them. We tour them in an afternoon in which they come in groups of 40 to 50. We will be glad to have you come out next year and be a tour guide.

McKay: I was noticing on your ideal staff list you do not have any operators, yet you man the place around the clock.

Woods: I guess that I was thinking more in terms of maintenance staff than I was in truly operating staff. I think everybody recognizes that for operating you want an operator there all the time. No matter what the accelerator facility the obvious solution would be operators, but unfortunately I don't think anybody can afford the required number in this day and age. I guess I did not go into it.

McKay: But these people you are talking about here, are they involved in being operators as well as other assignments?

Woods: No, they are not acting as operators.

Schultz: Does your operating staff include responsibility to the end of each beam line?

Woods: We are responsible up to the experimental piece of equipment. When it reaches the entrance of the scattering chamber we are not, in general, responsible for what goes on inside the scattering chamber. But we are responsible up to that point, all the beam line, the beam handling, and the optic system to that point.

Schultz: How does that affect target station pumping, Faraday cups beyond, etc.?

Woods: The usual procedure is that if somebody wants to mount a new experiment or change an existing configuration he must sit down and discuss it with the technical staff as to what he is planning to do and how he is planning to do it. Other people may be the ones who implement it; they cannot just come in and do something indiscriminately without at least discussing it with our technical staff. This applies particularly to vacuum systems at our facility with the tritium contamination.

Schultz: Our opinion is that your staff is a little short.

Woods: I think the gentleman who ought to have heard that is R. Taschek, and I think he has left.

Chapman: I just would like to make two quick comments. We have a two-man rule at Florida State which is probably half-way between that which Richard Woods has detailed and that which Bob McNaught has mentioned. We insist that there should be two people there at any time the machine is running. We do, however, permit one of those two to be away for a short period of time, perhaps to go to his room to get a paper he may need or something of that nature. We do insist while that second man is away that the operator stay at the control desk and not attempt to go into the accelerator or ion source to make any adjustments. If that is necessary he must wait for the return of the second man. The other point is just the obvious comment that Richard was explaining; the most recent accident they had at Florida State as well, and I think we should all bear in mind that the old maxim, familiarity breeds contempt, is very true indeed and it is often the people with the most knowledge of equipment who are more likely to have a momentary lapse. If you have worked with it for years then it's surprisingly easy to rush in when you should stop and think.

Lindgren: BNL was set up under Harvey and the influence from Los Alamos is such that we parallel your set up, including part-time operators and the two-man rule. One difference is our two-man rule is similar to the one Ken mentioned. If a man is alone at anytime, he stays in the control room and he is not to go into the target room or the accelerator room. If he needs to, and there is no one around, he can always call a security guard to come down and accompany him into that room. One question, how did you measure the 73% user efficiency?

Woods: We have a clock on the stop, at the entrance to the switching magnet. The number of hours that stop is in vs the number of hours the beam is available is the way we have determined that figure.

Lindgren: At one time, under Harvey's request, we implemented time clocks on all the Faraday Cups and on the charging belt, but I found that I was trying to influence that number by getting that last cup up as soon as possible. At length, we finally discontinued that measurement since we were convinced that it is not really important. The idea is that the beam is available and if the physicists are not quite ready to use it that's their problem; they lose time on it. We are there to serve the experimenters. It really doesn't make any difference what their efficiency is to us.

Wegner: From an experimental physicist point of view, efficiency of beam time utilization is a very odd number. Trivial experiments which require little or no set up, like making an isotope, are very efficient users of beam time, and in terms of a little administrator sitting some place looking at some efficiency figures, he gives them high marks. A guy doing a very critical, very interesting and challenging experiment on the other hand, uses the beam essentially half the time available because the experiment is very complicated to tune up. He can't even find out how to adjust components or install them in the apparatus properly until he gets some beam, and then he has to go in and modify them and there is no way to predict this. At the same time, you can have people that simply don't plan ahead ever and just throw away time. All the way around, it's a criterion that I think is a bad one to put in the hands of an administrator who doesn't understand what physics is all about.

Woods: We try to encourage people to take, say, a single shift for setup and shake down to learn just exactly the kinds of things you are saying, so the number of modifications that they have to do during the beam time is reduced. What it eventually shows is that some experimenters can plan ahead and some can't.

Wegner: But even the ones that are planned ahead can still end up with 20% use of the beam if the experiment is complex enough.

Woods: These statistics are over a very long period of time and it's not something that you can develop in a month's period. These statistics come over a 10-year period and should not be used in reference to a single experiment.

Moak: At Oak Ridge we don't really have much money on our EN tandem so we quite often find ourselves with limitations on personnel. There are even times when a run gets started on the tandem and everybody goes to Oak Ridge for dinner and the whole building is completely empty of all people. I suppose our safety requirement is well obeyed when the building is evacuated and the beam is

running, and the data taking. But one certainly has different situations in different laboratories, and I think that we are perhaps influenced by the fact that more than 70% of the experimenters who use our accelerator are from outside Oak Ridge National Laboratory. This makes the problem of so-called training almost an impossible one because you can't catch these people to train them.

Billquist: I have a comment. About the time I was asked to be chairman at this session, I got a little blurb in the mail from our safety people and it said something like, in the final analysis the supervisor of the operation is primarily responsible for knowing all the applicable rules and applying them in all fields of safety, and someday we ought to go into that.

TABLE I

STATES OF FACILITY OPERATION

1. ACCELERATOR(S) NOT OPERATING AND FACILITY UNOCCUPIED
2. ACCELERATOR(S) NOT OPERATING
   A. FACILITY OCCUPIED BY ONE PERSON
   B. FACILITY OCCUPIED BY TWO OR MORE PERSONS
3. ACCELERATOR(S) OPERATING DURING THE NORMAL WORKING HOURS
4. ACCELERATORS (S) OPERATING DURING OTHER THAN NORMAL WORKING HOURS

TABLE II

ACCELERATOR "OPERATOR

1. PERSON(S) HIRED SOLEY FOR THIS PURPOSE
2. PERSON WHO HAS OTHER DUTIES RELATIVE TO THE ACCELERATOR
3. ACCELERATOR USER/EXPERIMENTER OPERATOR

TABLE 3

STAFFING OF FACILITY IN MEN/MV

	FACTOR	9MV	14MV	25MV
ACCELERATOR SUPERVISOR	.07	.6	1.0	1.75
MECHANICAL ENGINEER	.07	.6	1.0	1.75
ELECTRONICS ENGINEER	.07	.6	1.0	1.75
ION SOURCE ENGINEER	.14	1.2	2.0	3.50
MECHANICAL TECHNICIAN	.14	1.2	2.0	3.50
ELECTRONICS TECHNICIAN	.14	1.2	2.0	3.50
HEALTH PHYSICIST/RADIATION SAFETY OFFICER	.07	.6	1.0	1.75
TOTAL STAFF	.7	6.0	10.0	17.5

Qualified Users

Machine Request Form

Machine

Who

Which Particles

Bunched ___Yes ___No

Energy

Tritium Gas Target ___Yes

Number of Shifts

How Split Up

Which Beam Tube

Computer ___Yes ___No

Which Computer Program

How Much Data Storage Required

Personal Available By Phone

Non-LASL Personnel _____ Name _____ Affiliation

Participating Aliens _____ Name _____ Affiliation

Science Youth Day Guide Preference

_____

Projection for April 25 - May 22, 1977

Who:

Machine:

Which Particles:

Number of Shifts:

Projection for May 23 - June 19, 1977

Who:

Machine:

Which Particles:

Number of Shifts:

Fig. 1

Session III, Chairman, R. Woods

TELEMETRY SYSTEMS: A REVIEW AND A FORECAST

R. McNaught
McMaster University

When we were discussing the problem of controlling things in the terminal of an accelerator at the first SNEAP meeting I attended, several people were contending that the only reliable way was with belts and rods. Last year two people reported that they had installed microprocessors in the terminal. Things have changed.

When Dick asked me if I would give this talk, I said that we hadn't done anything very significant in this area during the last year at McMaster. What I plan to do is to summarize the developments in telemetry systems and see if it throws any light on the direction we should go in the future. A better title might be: A Review and a Proposal.

First let us define our terms. I am not sure why people have called these systems telemetry systems. The dictionary defines telemetering as, "the transmission by electromagnetic means of a measurement over long distances." In our case, the distances are not very long, and we do not ordinarily use electromagnetic means to transmit the information. The voltage barrier of a few million volts makes the distance seem long, and it does require that electrical signals be converted to some other form, e.g., a light beam. I suppose, strictly speaking, a light beam is an electromagnetic means of propogation.

I shall confine any remarks to the problems associated with sending information across this voltage barrier in the environment of an accelerator. This includes the problems of sending information to and receiving information from the terminal as well as to instruments, such as ion sources, at elevated voltages outside the tank.

As operations staff for accelerators, our first responsibility is to keep the accelerator operating at its best so that nuclear physicists can do basic research. When it appears that a new gadget will improve the performance in some

way, we set out to design and build it using our own experience and the experience of our colleagues in other labs as a guide. Almost no one has the opportunity to do thorough research and development, with the possible exception of the accelerator manufacturers. Each of us has had some successes and some failures. At this time, if we put it all together, we may have enough information to point in a sound direction for future projects.

I shall begin by summarizing the work we have done at McMaster.

(a) We have had a beam stabilizer operating on our F.N. machine for about seven years. It uses an amplitude modulated light beam (i.e., an analogue signal) to transmit the signal to the terminal. It requires no return signal. The transmitter consists of an LED outside the tank at the low-energy end. The receiver consists of a photomultiplier tube. The only semiconductors in the terminal are selenium rectifiers in the power supply. After some initial problems we rebuilt the terminal electronics in a doubly shielded box with spark gaps and surge suppressors at likely points. It has now operated for several years with acceptable reliability.

(b) About four years ago, I designed and built an instrument to provide meter readings of currents and voltages that were generated at 150 KV. For this I used fibre optics light pipes. The signals were transmitted from the 150 KV level with LED's. The information was transmitted as a pulse width modulated signal using the 60-Hz power line as a source of clock pulses. The technique worked very well. The circuits at H.V. consisted of op-amps and TTL logic devices, all semiconductors. In spite of careful construction using shielded boxes and surge suppressors, it failed almost every time a good spark occurred. As a result it is not used at all now. Meters are located on the H.V. deck and a T.V. camera and monitor has been substituted for it.

(c) Recently we installed a microprocessor on our polarized ion source which provides a direct method of measuring the quench ratio. This measurement requires actuating a relay remotely on the 100-KV deck, a very simple task. Again we used a fibre optics light pipe. The receiver is a phototransisitor at the 100 KV level. The signal is digital, either ON or OFF. So far it works, but has not been operating long enough to prove its reliability.

We could have done this job reliably with a lucite rod to actuate the relay and a calculator to provide the answer. However, we are proceeding cautiously in this direction in the hopes of monitoring and controlling various other functions with the microprocessor.

Since I can find no report in the SNEAP minutes of the work done at Daresbury, I think it is appropriate to summarize what they have done and are doing. This information is gleaned from their bulletins which we receive regularly at McMaster. Their work seems to me to be more significant in many ways than others I am aware of.

They plan to use a modulated light beam to transmit information in both directions to components inside the pressure vessel. There will be receivers and transmitters along with some instruments at each dead section along the column. The data consist of temperatures, pressures, speeds, currents, and voltages that are converted to digital form and transmitted serially using time multiplexing techniques. The transmitters will be infra red light emitting diodes. There will be separate high bandwidth channels set aside for control of the down charge and for stripper modulation. Their system is designed with TTL logic devices throughout. An interesting feature is that it will be possible to interchange ADC's by control signals so that if one fails, another one can be used to perform its function, perhaps at a reduced data rate.

What is significant about their work is that each step in the development is

tested in an existing 6-MV accelerator. Since it is a real R and D program they can be reasonably confident of the instruments when they are finally installe in the 30-MV machine. Since quite a few instruments are involved they have developed a standard module to contain the circuits with all the necessary shielding, etc.

I shall summarize what was reported at SNEAP last year.

(a) At least two people reported having microprocessors in the terminal and these survived even when other equipment was destroyed by sparks.

(b) Two people reported having fibre optics bundles explode when they were used inside the tank and the machine was at operating voltage. In contrast to this, others, particularly at Chalk River, have been quite successful using fibre optics. The difference seems to be in the light pipe used. The successful ones have used Crofon fibre optics by Dupont.

(c) Some people have been using laser beams as the communication medium.

Putting all of this together, may I suggest the following guide lines for future design.

(1) Simplicity is the key to reliability. Electronics engineers have known this for a long time, but have tended to depart from it recently because electronic devices have become so reliable that one can use large numbers of them and still have good reliability. This may not be a good approach for instruments in the terminal.

(2) Proper construction, grounding, and shielding techniques are essential. If the construction is very carefully done it is possible to put microprocessor and other LSI devices in the terminal. This will be necessary to perform the complex tasks that will be required in the future. A microprocessor is probably better protected than appears at first sight. Every I/O port is isolated from the outsic

by some other device such as an ADC or a transistor driver. Special care will be required in the selection or design of these components as they will tend to be damaged first.

(3) Metal oxide semiconductor devices may withstand surges caused by sparks better than bipolar devices. At this point I would design around MOS logic rather than TTL.

(4) There seems to be little reason to use fibre optics up to the terminal when a light beam in the open will serve perfectly well. However, the path of the light beam should not be past the drive motor. If it is necessary to use light pipes, for lack of space or some other reason, be sure to use Crofon and lay it along the column to provide a uniform voltage gradient.

(5) A photomultiplier is an excellent receiver for either a visible or an infra red light beam. It is extremely sensitive, very fast, and very reliable. Its only disadvantages are its physical size and the requirement for a H.V. power supply. If you choose to use a photomultiplier and an infra red light source, be sure to check the specifications for infra red response. You will probably have to specify a special glass.

(6) If the instrumentation is complex (i.e., there are many points to be monitored) it is worth while to build in some form of duplicated instrumentation such as Daresbury is doing.

(7) A single channel with good frequency response can be multiplexed to handle large amounts of digital data in serial form. Two channels are required for two-way communication. A separate channel may be required for terminal stabilization or control of the down charge.

(8) For communicating with things outside the tank that are at much lower voltages fibre optics light pipes make ideal links. The only problem that inhibits the immediate implementation of them is the lack of hardware to put a system together. However, several companies are working on this and I am sure the parts will soon be available off the shelf.

(9) Avoid the temptationto re-invent the wheel. The techniques and components for reliable data transmission have been developed for many other uses, and it behooves us to use them even if they don't seem to fit our application exactly. The only factor that makes our problems unique is the environment, operation in the terminal of an accelerator.

I make these suggestions tentatively in the hope that they may be helpful. Let us have your comments.

Discussion:

Liebert: What have you found about the need for using double shielding on the IC's?

McNaught: I think that one should consider building a double-shielded box for anything you put in the terminal. You probably can get away without it for simpler things with tubes and we have some of that kind of equipment in ours. But I believe it's the best policy just to use it right off.

Haberl: Just a couple of comments on the devices. We have used two preliminary down links of V-to-F telemetry. These are single-shielded and have worked without any difficulties at all. The only devices that gave us trouble were the CMOS devices but TTL held up quite well. We do use Crofon fiber optics that are not bundled tightly, and it's worth noting that Crofon doesn't pass infrared; you do have to work in the visible.

McNaught: The comment about the CMOS devices failing is interesting. I have noticed that in the Daresbury work, they are using TTL logic throughout, and yet they seem to have reliable performance. My own feeling is that they have done such a good job of making the boxes that they probably protect them pretty thoroughly. Yours is the first comment that I have heard about CMOS devices being destroyed when others weren't.

Woods: Bob, how thick do you make your boxes, do you make your boxes similar to Daresbury or do you use ordinary sheet metal?

McNaught: We only have one and it's crudely built using 1/16-inch sheet metal, doubly shielded. For the future we should design a box that is just sheet metal and of standard construction. I am interested in getting the information from Daresbury as to what theirs is like. I have seen pictures of it but that is all.

Woods: Their system uses a 1/4- or 1/8-inch-thick wall on both boxes. Of course, it is completely rf gasketed with many screws.

Wegner: It's not just the boxes; the boxes have to hook up to things to be of any use in the terminal. One box can have quite a bit of logic in it for handling 10 lines or something. Daresbury has gone to a lot of trouble to build double-shielded connections to all of the peripheral components, and it is done very elaborately. Now the only place they break the connection is when they have to bring power into the box, because you just can't create it inside. You, in principle, could run a generator inside the box with a shaft from outside through complete shielding, but they bring in power, and that looks like the only soft spot in their whole design. They bring the power in through two independently shielded boxes on the side of the main box, which supposedly strips out all surges that might come through the power line, so that nothing, in principle, can get all the way through into the inner box. Once they are in the box, then all of the logic or whatever is coming out of the box for whatever they are running in the terminal has its elaborate double-shielding system in the cabling, and everything is consistently shielded everywhere. You pay a price if you want to do it right.

McNaught: Yes, I noticed that they do have filters and elaborate filtering on the line voltage going in, and I think that is just as important as the box itself. In fact, where you ground the box may have significance, and I don't know what the right place is.

Wegner: One comment that may be germane is that Munich has an elaborate surge protection system on their telemetry and they use fiber optics and all the rest of it. Many of the modules along the column have self-contained power in the form of batteries because they are running very low power consumption units that can run for about a year on these contained batteries. These little units are sending out on fiber optics, to the control console, information such as pressure at the dead sections and so on. They haven't been running any higher than 11.3 to 12 MV because of two problems on the machine, but under these real running surge conditions, these units that have no penetration through the shielding walls with power, have not failed. The ones that have failed are the ones that run in the terminal and run off terminal power. Even though they have gone to a lot of filtering care to get the surges out, that still is the only failure point. It is not a trival problem to get power into a box.

McNaught: Do you know what they do with the batteries, do they put them in a pressure vessel of some sort or will they stand the 90 psi?

Wegner: They are sealed up against pressure as I understand it.

McNaught: For several reasons I feel that CMOS or metal oxide semi-conductor devices might be preferable. First of all, the power supply can be very simple and brute force because it doesn't have to be well-regulated power. Power supplies should fail less frequently on that account. Second, they are much more tolerant to voltage variations than TTL. If you had a system designed to run off 5 volts, it could surge to 10 without hurting them, in fact, 15 without hurting them. But I think you are right, you have to take extreme care at every point if you are going to have the reliability we require.

McKeown: We have an operational amplifier in the terminal of MP-6 to measure the Faraday cup current of our terminal source, and that means three leads that we had to get out. One is the meter lead so the TV camera can read it, the ac

power for it, and the Faraday cup lead itself. We put elaborate filters including inductors, varistors, and diodes, on the input where there are no big voltages and on the meters, and never had any trouble with the diodes or anything like that burning up, but the operational amplifier would blow up everytime we had a tank spark, and we didn't know why. We finally added ferrite beads on the inputs and the outputs of the operational amplifier and have not had a failure since. This circuit is all double shielded and everything else. As Harvey says, you have these leads coming in from the outside that bring all these terrible tank sparks and surges into this system. But it is still possible to get rid of them.

McNaught: These were beads put on the signal input leads, correct?

McKeown: That is correct and on the power leads as well.

Larson: I would like to comment about some of the experiences at Brookhaven, and I invite the Brookhaven people to correct me or to update the information. Initially, on the power supplies that operated the first terminal ion source, the thinking was to first provide a direct spark gap to ground at the point where the leads penetrated the main box enclosure. These leads went out to ion source loads such as the extraction voltage and the magnet in the duoplasmatron. These included fairly high currents such as the filament current and also high voltages in the kilovolt range. Then the wires were snaked around in the box and went up to power supplies, which in turn had some local protection as well. If I am correct in my recollections, one of the early discoveries was a lot of glass lying around in the bottom of the tank from the glass-enclosed spark gaps which had shattered during excursions. Yet power supplies did not suffer failures from this, and I think that open gaps were used to replace the glass-enclosed spark gaps. So some rather curious phenomena can occur, such as this case of blowing up the protecting gaps and yet not having the power supplies damaged. A lot has to do with the available path to the high-voltage transient when it comes into such a circuit and how much inductance. Mike's comment about leads is very germane. How much inductance is provided in the way of either indirect paths of the wires or inductors placed in series in those leads before you get to the power supply is important.

Woods: What kind of spark gaps are you using at McMasters?

McNaught: We have a homemade one, which is not a glass-enclosed one, on the output of our terminal stablizer. We also brought the signal output leads through an inductance which again is a home-made thing, a few turns of wire wrapped on a piece of insulating rod so that it will have a few microhenries. It does have some inductance in the lead and it does have a spark gap on the output side. That seems to have been fairly effective. In that case it's coming from the plate of a tube anyway, which is not the most sensitive point. But strange things happen when things are blown up with sparks. A 100-ohm grid stopper resistor burned right in two, yet other things are undamaged, and you wonder how in the world it can happen.

McKeown: We still use the glass spark gaps, but we have found that if we put a one-ohm resistor in series with the spark gaps, that protects the spark gaps from over loads and still gives you the protection of the spark gap. We use both open homemade spark gaps and the glass. The nice thing about glass gaps is that you can operate at atmospheric pressure.

Haberl: I mentioned our protection doesn't use spark gaps anymore at all. The glass type we had made by Seimens I believe take 20 nanoseconds before they will fire. During that time you can have kilovolts of transients at least. We have gone entirely to MOV's which, as far as I can tell, don't have that problem. We generally run wires first to an MOV which go to an external ground, then into the box through a conveniently small Miller commercial inductor. Following that, we use Zener diodes which, in some cases, are simple Zeners, and in more recent cases, we bought the type that are intended as spark protection. So there are three elements, one heavy shunt of an MOV, followed by an inductance, followed by a Zener diode protector.

Moak: What is an MOV?

Haberl: A Japanese firm developed the metal oxide varistor. They have almost as sharp a knee as Zener diodes. They are bipolar and they will stand very large surges. They are not quite as sharp as the best Zeners but they are very good at taking this heavy surge that spark gaps should be taking. The significant capacitance they have isn't necessarily good on high-frequency circuits, but for dc I think they provide a much better technique for the initial large surges.

McNaught: You can also use them on ac. We have used these MOV devices quite successfully, especially when we first installed our polarized ion source with 100-kilovolts injection, and it was giving sparks that blew up various instruments, including commercial instruments, and we put these surge supressors on all of the line inputs and some other places. You buy them from GE.

Chapman: I wonder if you have any comments on the advantages and disadvantages between LED and laser as your light source for such a telemetry system?

McNaught: I can't really make any comment about the laser because I have never used it. It seems to me that the LED is a simpler approach. They certainly work very well. It's just a little cheaper and simpler then the laser.

Woods: Pete, could you bring us up to date on the status of the Argonne laser?

Billquist: In the last year we have abandoned lasers. The reason we have is (1) size of the laser inside the terminal and (2) the lack of any remaining straight path from terminal to ground.
McNaught: That means you have to use fiber optics.

Roth: I just want to make a quick comment about batteries. That is our most serious problem, getting power into little telemetry devices, and we use batteries right under the 225-psi nitrogen tank pressure. The only problem that we have had is that certain batteries have very cheap connections made between the terminals on the top and the actual battery itself. We pressurize various brands and the ones that quit producing voltage under the high pressure are the brands that we do not use in the terminal, otherwise we have had no problem at all.

McNaught: Can you tell us the brands you do use?

Roth: University Central Stores buy Burgess and that's the wrong one, and we use Eveready, and also RCA work very well. This was the 22-1/2 volt battery and

it's about 3x5 cm. These are ordinary batteries, not alkaline. They withstand an external vacuum without any problem.

Woods: I would be interested in what kinds of spark gaps other people are using. Are you people using any commercial varieties of spark gaps, and if so, what kind?

Larson: This is not expertise but I would remind people, and I think that this may have been mentioned in the previous SNEAP meeting, there is a type of glass-enclosed spark-gap design that has a pressure-dependent voltage rating because the mechanical construction is such that the gap decreases under pressure. That is something to watch out for. All these devices really should be pretested under pressure to see what they are going to do before you install them in the chamber.

McNaught: I have heard, and I don't know who told me, that some of these commercial ones change with time if they have been sparked a few times so they spark at a different voltage.

Larson: John Benjamin made quite a number of tests on the gaps that were selected for use at Brookhaven at the beginning and I don't know if these are all being used now. It is certainly true that some of them indicated an erratic behavior, either a change in the breakdown voltage with time, or just erratic breakdown voltage. The tests were done in a pressure vessel with a simple mechanical drive on a Variac operating ac to a very simple power supply. The voltage was raised to beyond the breakdown point of the device and a chart record of voltage across the device was kept. The simple cycling system gave quite useful information as to which devices seemed to work reliably and which had erratic or undesirable operating characteristics. Let me make one other comment with respect to another device which showed a curious property. At Brookhaven, some hermetically sealed circuit breakers were selected and, although they seemed to function quite reliably in all the tests, they still gave some trouble in actual operation in the machine. The case deflected enough under pressure to affect the operation of the breaker. I think they didn't reset, but the solution was to break the hermetic seal so that the case didn't deform.

Woods: The subject of circuit breakers in terminals is relevant. Are people still tending to use circuit breakers and other devices that can be either reset automatically or externally?

McKeown: We use our own design circuit breakers. It's a 6-volt ac relay that handles the load current and that opens up when the current gets too high. Then we have a holding relay and that makes it very nice, because when we turn the power off to the terminal all these relays then have to be reset. They are very simple to make with standard products and have been very reliable. The diode that we use to supply the dc to the holding relay is shielded well enough by resistors and condensers that we have no problem with that diode at all. We used to use the Heineman breakers but mechanically, they are like a tumbler switch, and mechanically you have to switch them with some kind of control rod which is the weak link. The arms tend to bend or break or something like that.

Wegner: I was curious whether anybody had ever considered just using a moderate-power CO_2 laser beam to transmit power to various points along the column by

going through a germanium window. These are extremely transparent to the right frequency laser and this might be a means of getting power into any old place you want along the column, possibly to recharge a battery that's all sealed up with a germanium window on it,or direct power to a point. It's a possibility; I just wonder if anyone ever considered it.

McNaught: You could turn the power off and on that way, too, couldn't you?

Lindgren: I wanted to make a point about spark gaps which I haven't heard mentioned. We observed a phenomenon in the early days of the direct extraction source in the terminal where we put spark gaps on everything. They were glass-enclosed spark gaps. They caused us more trouble then anything else by shorting out and we finally realized what was happening. A transient would trigger the spark gap, it would fire and short out a 300-volt, 4-or 5-amp power supply and that would pour power into the electrodes of the spark gap until they melted down. One has to limit the current in that kind of discharge. Maybe that's what Mike was talking about, putting a 1-ohm resistor in series with a spark gap. You want to prevent the power supply from burning up into that short.

McNaught: I think that kind of problem is one that you have to be careful of, that in trying to protect things, or in trying to make them fail-safe, that we in fact, don't introduce other unreliable components or unreliable methods of operation that make it just as bad or worse than if it were on its own.

Haberl: It's worth making a plug for the MOV again. Since they don't clamp and short out they simply clamp and hold the voltage to the limiting value. When the spark goes away, the power supply, which is below that level, is not going to pour power into the circuit.

Woods: Has anybody tried anything new in the way of fiber optic materials with or without success,or are we all settled on Crofon at this point? The Munich group is using something that is manufactured in Germany. I have the information for those who can read German. But it's available only in Germany as far as I know,and they're using it successfully.

Billquist: I am not really going to be much help, but a year ago,I told you what the material was. We had a single strand fiber optic cable that we successfully ran voltage tests on,and it's the same material we have since run and will be running. Two LED light links are used, one up and one down, but they are single-strand 40-thousandths-diameter plastic.

Woods: This general subject was one of the subjects that was mentioned most on the preregistration forms which you sent back,and therefore,I assigned a 2-hour period to it. You still have an hour and 15 minutes to talk about it. Is there anybody that has a particular aspect that they wanted to talk about that we have not discussed so far?

Roth: Over the past year we installed one of these Texas Instrument sequencers on our vacuum system and we have been quite pleased with the results. We have taken this enormous rat's nest of wires which exists under every accelerator and reduced it to something like 50 to 100 wires going either to input modules or to the output modules. We have been able to do everything that we did before with those relays and wires, and so forth, plus some very interesting new things such as putting timers on our flow switches so that if there is a surge in the water

you have 30 seconds or so of a grace period before shutting off the accelerator. Also we have timers on certain pumps, such as titanium sublimation pumps which will come on after a certain amount of time and the fore vacuum will run until the vacuum is restored to a suitable level. I was wondering if anybody has had any experience with these.

Woods: Anyone have any comments? Would you like to describe the system a little more in detail, Gary?

Roth: The system is sort of a step down from a microprocessor. It is programmed by a keyboard which looks something like a calculator, and you have input circuits and output circuits, and then you have software relays, timers, and counters built into this device. It has a 1024-word memory, which is basically an input and output or a control relay, and your various circuits can then be coupled to each other through the keyboard. For example, if you take apart some of your vacuum interlock, or program out a certain valve, or if you change to an unusual configuration, you can program these things in, in just a matter of minutes. The inputs and outputs are solid state relays, the inputs being triggered by whatever sensing devices you have. The outputs will deliver up to 3 amps of power switched by the output cirucit. The device is programmed by its own logic system. You can and/or select outputs as inputs to circuits somewhere else in the system, and in such a way, devise the program which will control any on/off functions in the accelerator. In fact, we have been able to reduce racks of something like 100 relays, and virtually thousands of wires, into about a factor of 10 smaller. Reliability appears to be much greater than either relays or, in particular, with a large number of wires and trying to find faults in the system. This has made it much more efficient to operate this system.

Woods: What about external monitoring signals from the system so that you can monitor what's going on in the vacuum system?

Roth: You can pick up the output module signal which we normally send right to the solenoid of the valve or to some small motors or whatever. That can operate a light on the indicating panel. You have the option of using the keyboard itself to monitor the timer or the counters, and also there is a power flow light such that, if you have ten inputs in series in an and/or situation, and then some outputs, you can trace through this with the keyboard and observe when the power is all of a sudden not continuous and that tells you that a certain sensor or certain output is open rather than closed. In that way you can actually monitor much faster than you can take a voltmeter and go around from cabinet to cabinet looking at terminals.

Woods: What is the relative cost?

Roth: We have two systems. We have the large one that has the 1024-word memory. The memory unit itself is about $900, the keyboard is about $250, and the board with 16 modules of either input or output plus the board and the interface is around $250.00 per board. We have about $2000.00 invested, which has converted the entire vacuum system of the accelerator. The second unit that we have is on a large scattering chamber which has automated the roughing and pump-out such that there is one switch on the control panel where you select air or vacuum and the chamber automatically roughs itself, converts itself to a high-vacuum system, and then monitors all the vacuum sensors.

McNaught: I just wanted to ask if you would tell us the component numbers of your memory and your unit for our records.

Roth: This is a Texas Instruments Unit called the 5TI 2000, I believe. There are a number of other companies that build these, including the old relay standbys like Allen Bradley. But Texas Instruments seems to have the handle on the smaller size and what appears to be the easiest programming and so forth, over about six or eight other companies that we have particularly looked at.

Wegner: Just to add a little more confusion to the discussion here. I don't think one has to consider telemetry or control, in and out of high-voltage enclosures to be one way or some other way. You can actually use mixed systems very successfully. There is nothing wrong with using control rods for some simple mechanical manipulation that has to be done that could be very complex to be carried out by signals that originate from micro-electronics, for example, and still maintain integrity against surging. If there is not a reason to do it with micro-electronics and it is convenient do it some other way, you might prefer to do it that way from a sheer reliability standpoint and cost. It is interesting that the ion source complex of the MP tandem at Munich, which is the most complex ion source package I have ever seen in my life, Kutchera was having great problems with the fiber optics part of one of the communication systems and discussed abandoning it. It was a different group, I guess, that put in the ion source system than put the stuff in the machine which is a very reliable package. He put in a pneumatic control where you just use tiny hoses about a 1/16 or 3/32 of an inch in diameter. There is a whole logic system built up for the pneumatic control of things. They operate electrical switches and all sorts of things. You just have a little bundle of hoses going to an enclosure, inside of which you have your power and so forth, and you manipulate switches and so on. Dave Wiser at Canberra made a great point in the fact that they eliminated all of the electrical control systems in their terminal, which now has given them reliability in their machine which they have never had before, by just running a bundle of hoses up the column full of pressurized SF_6. They just arrange the pressurization system on these hoses so it's always above the tank pressure and tracks the tank pressure so it won't blow things up. They manipulate things in the terminal by just pumping up some SF_6 through different hoses that operate pneumatic devices. For instance, you can move a Faraday cup in and out with a pneumatic device and read the current off of it with a micro-electronic device that you perhaps can't read any other way. Maybe the sensitive telemetry could be reserved for these sensitive kinds of measurements, and cruder, reliable mechanical systems can be used for the gross motions of objects.

Janzen: On this problem of pneumatic controls in a terminal, I think I suggested this once some years ago and since that time I have tried it. But I find the tubes don't survive our machine. I brought a sample this time of a Teflon tube which I had run from the base to the terminal. It was not being used for anything, only a test. It had tank gas in it at tank-gas pressure and yet this tube did not survive. It has holes blown through the wall and tracking-like conductive tracks, presumably. Our machine, which is an upgraded three to four million volts single-ended, possibly runs at much higher gradients than other machines and this may be a problem. I would love to run tubes, but clearly from what I have seen on these tubes I don't think the tubes themselves are going to stand the environment.

Woods: Were the tubes dressed to the column to get a gradient on them?

Janzen: Not entirely. I ran it through a hole which normally takes a control rod. We still have the Plexiglass control rods in the machine which survive quite well. This tube simply went through one of these holes. It was a Teflon tube with a fairly thick wall but it certainly didn't survive. I have a section of it with me, if anybody is interested in seeing what happened to it. All I can conclude is that on tank sparks the propagation of the disturbance through the hole in the tube is quite different in mode or in velocity or in something than what happens outside and you can get gradients across the worst spot where the tube went through a column shield and this is where the biggest hole appeared, but it wasn't the only place where holes appeared. We also got holes through, along the tube between the column shields.

McKay: We tried running pressurized gas up to the terminal of our K machine and had failures. Do you remember, Jim, what pressures we were running and what gases?

Stark: We tried running hydrogen or helium at 150-250 lbs. It would stand up until you had the first big breakdown and then the whole tube was gone from one end to the other. There would be pin holes all along it.

Larson: I wonder if somebody from Brookhaven would comment on Brookhaven's experience with hollow Lucite control rods.

Wegner: They blow up!

Lindgren: What was the size of the tubing that Henry Janzen had blow up? Your sample looks like 1/2-inch OD with a 1/8-inch hole. We have successfully used Saran tubing which I don't believe is made any more. It was 1/4-inch OD with a 1/16-inch hole through it that withstood the 3.5-million volts on the research machine. Helium would have to be pressurized to about 500 psi. We were running 160 psi in the tank insulating gas. We found with helium we had to go to extremely high pressures but hydrogen and deuteriumwould run without breaking down at tank pressure. We are not using Saran anymore, I believe it is lucite with about the same dimensions.

Janzen: Do you know what your gradients along the column are, Bob?

Lindgren: I think it's about 30 to 35 kilovolts per inch.

Janzen: I think we are quite a bit higher in an upgraded three and this may be the whole problem. I think we are up around 50 kilovolts per inch or more.

Woods: Do Jack Shaw or Charlie Goldie know the gradients in those upgraded machines?

Goldie: Divide 4 MV by 67 - about 60 kV.

Berners: We have a nominally 4-MV single-ended machine that we have put some polyethylene tubes into for pneumatic control. We have only run it up to about 3 million volts since we put the tubes in, which would work out to a gradient of about 25 kilovolts per inch and the tubes have survived for quite a long time at that gradient. We have seven of them and they are the kind of polyethylene tube

that you buy for about a nickel a foot which is just a standard commercial item. We pressurize them with nitrogen to anything from 20 to 100 psi above tank gas pressure and they work just fine. The installation at Canberra that was mentioned earlier by Harvey Wegner uses nylon tubing. I meant to say that the tubes are lying on bars spaced about 1-1/4-inch apart all the way along from ground to terminal, so they are pretty well graded.

Chamberlin: We are talking about the same kind of gradients that you do at the Van de Graaff, but we are at 750 kilovolts at the injector at LAMPF. We had to run a pair of helium lines up the leg to the polarized source to operate our cryo-pump. This was running at about 250 psi. There was quite a bit of worry about the helium breaking down so we spiraled the stuff up the leg. We found some hose that was being used by the magnet group at MP Division, which is called Sinflex and it has a 1/2-inch ID and handles 2000 psi. It will withstand 50 kilovolts per foot with less than 1 microamp of current drain and we haven't had a lift problem since we put it in. We did stretch the path by spiraling it up the leg. We put one turn per foot of rise and the ID of the tube was 10 inches, so you figure out what the length was.

Norton: It's not my area of expertise, so I hesitate to comment on the construction of the boxes. Daresbury has gone to 3/8-inch aluminum with drilled holes for ventilation, and NEC has gone to 1/8-inch steel with honeycomb for ventilation, and both of us are using rf gaskets and other kinds of fancy stuff. Munich, where everything is surviving, is just using sheet metal boxes with no r-f gaskets. The strong impression is that the double-shielded box construction is not a critical thing. With respect to Canberra, I thought I remembered a slide where he showed the tubes running along the inside of the support posts which are graded, so in that case, his tubes would be graded.

Woods: My feeling on the Daresbury system is similar to yours, Greg, in the sense that I feel they started their design from the maximum, which was almost guaranteed to work, rather than starting at the shallow end and working up to find out what was the minimum required. I don't think anybody has proved where in between is the right place to be.

Norton: If you do skin thickness calculations for fields, it turns out that aluminum does not stop the magnetic part of the wave and 3/8-inch aluminum is equivalent to 1/8-inch steel, so we are actually using equivalent systems, but ours is a little bit smaller. The advantage they have is that they don't have to spend money on the honeycomb, which is expensive. They have nice ventilation holes where they don't have to worry about this honeycomb business. The light links are also covered by this honeycomb which is a good protection.

Woods: Is that the Rolls Royce honeycomb that Daresbury uses, I believe, or is it something else?

Norton: It's another brand, we get it domestically.

Larson: This raises a question in my mind of what's going on. Greg was talking about shielding the electromagnetic wave coming through the box. We also know that conductive transients come through the wiring where you penetrate the enclosure. Is there firm evidence that enough rf, in wave form, goes through to cause trouble or is it more likely that these are conducted currents just

passing through the shell going from one place to another, but are surface or bulk currents?

Wegner: A comment that Wiser made that on a cylindrical machine when you terminal spark, you send a beautiful electromagnetic wave down a tuned line, essentially to the base of the machine and to the top of the machine. On one occasion he claims there was a spark several inches long right next to his head outside the machine, presumably produced by the electromagnetic wave which roared down the column and hit the end. It made a large enough potential gradient externally to the machine to make this spark a very frightening experience. How true or accurate that is I don't know. These electromagnetic waves are potent.

Norton: That is for real and I have seen it myself in Japan on the 12 MeV machine. Underneath the tank, a spark came from the base of the machine and arced straight down near a forepump, which was a four-foot distance. Lights were out and I couldn't see for a while. We have noticed that when there is a spark, there is a significant shock wave which will go in a straight line. We have these columns supported by I-beams which go off to the side, but that doesn't matter. These shock waves go straight down and ignore these I-beams where conductance is very good and destroy anything in their path, such as beam monitors, or Faraday cup microswitches, and so on. To avoid this on top of the column, we put copper straps which go straight out and this protects everything on the beam line, but you still get sparks coming out of the top of the tank but they don't do damage on their way, as long as you are not standing on top of the tank.

Ziegler: Last year at SNEAP, I mentioned the data acquisition system which we have at Oak Ridge on our EN tandem and I will not go into any detail. We do use it for logging purposes and for fast data acquisition when there is a spark in the machine. I would like to mention some of the unusual things which we have found during this past year. We do have a fast link operating which will log about five channels roughly 300 times a second. On top of that we can also look at very fast transients with a capacity pick-up unit and a sampling scope. The first thing that is a little unusual is the generating voltmeter signal. Let's say a spark occurs here and we get a signal out of the generating voltmeter which looks something like that, and this is on the order of a tenth of a second. The other signals that we get out are also a bit unusual. Anytime there is a spark, the generating voltmeter gives us this sort of an output. We also monitor the high-energy vacuum on this fast scanning and it is different for different types of sparks, I think. Let's say this would be a perfect vacuum and, of course, the ionization gauge puts out a logarithmic signal but it is normally on one decade and we think it stays on that decade. We do not monitor the range, we only monitor the meter reading. One very odd type of vacuum variation that we get would look something like this, where it actually goes below zero. If we got that consistently with every spark, we might say that it is an instrumentation error, that we were really picking up noise, but we don't always get that. At other times the vacuum may remain perfectly constant when a spark occurs. That would tend to rule out the instrumentation error. Of course, when there is really what you would call a tube spark, this is the high-energy vacuum incidently, it would look more like that. I think we can explain this one, but what happens in the vacuum system? I would be happy to have any suggestions. This, I think, is probably due to gas which is ionized in the tank. The actual voltage on the terminal is really going down, very

fast, to zero, but there are a large number of ions formed in the tank gas when a spark occurs and the generating voltmeter, which is sitting there rotating, picks up a charge from these ions which are migrating towards the wall. Really, this rise is probably due to ions in the gas. I suspect that this type of thing may be due to ions generated in the beam tube but I have nothing to substantiate that. One other thing we have added is a new channel that we put on the data acquisition system for a tank ionization gas monitor which was suggested in the original instruction manuals for the EN, and it's proven to be rather helpful in the conditioning of the machine. It reads 100 nanoamps full scale and it's a signal taken off a capacitive pick-up unit. The capacity pick-up feed-back loop of course is capacitively coupled to the pick-up unit so you don't have to worry about the dc signal. The probability of a spark occurring in our machine increases very drastically as this ionization current goes above 50 nanoamps. If anyone has any explanation for this, I will be glad to hear it.

Lindgren: I don't have a solution, but why monitor the GVM? When you have something that happens on a spark you can't explain, monitor the column current and you might see if there is something of interest or if this rise on the GVM is important. Maybe it isn't important. It could be instrumentation.

Ziegler: We do monitor the column currents and we do not see them rise, like the increase that you see on the generating voltmeter.

Lindgren: That's probably significant then. It says the voltage doesn't go up.

Ziegler: True, I would not expect the voltage to go up on a spark, but how do you explain what does happen on the GVM?

Adams: We definitely see a spike on the column current when we get a spark on that half of the column, whichever column it is, and the other one just drops off to zero. If we have a spark on the high-energy column, we get a spike up and then it drops down, and the GVM might see that.

Janzen: I think you can see the rise followed by a fall in a capacitive pickup on the tank.

Chapman: It is not surprising that in many instances, on the column currents you get a rise because you are having a column spark which is effectively shorting out part of your column resistance. It's much less surprising that you get a spark on the column currents than that you get on spark on the GVM.

Moak: I was going to ask if there's a possibility that the spark occurred not to the terminal but a little down from the terminal in the case you have the rise.

Ziegler: No, this always happens on any spark, Charlie.

Hurley: It could be just the spark-inducing problems in the 60-cycle current in the instrumentation or the instrumentation itself. We were trying to monitor power-line disturbances during spark downs at Chalk River, and the first thing that was zapped was the power-line disturbance monitor which was plugged into a wall outlet. We had a variety of equipment get busted in the machine room if it were plugged into any of the outlets along the side of the machine or within about 20 feet of the machine on the high-energy end or anywhere between the low-

energy and the ion source cage. The most recent thing that got done in was a little gas sniffer we use to search for SF_6 leaks during tank pressurizing. The thing had been left plugged in to charge the battery, and at about an 8- megavolt spark, it wiped out this SF_6 sniffer. It could be just the power going to the instruments getting shocked by the spark.

Haberl: Somehow it escapes me that there is a problem here. If the charge is dumped from the terminal and approaches the GVM, the capacitance goes up and the signal is going to rise. It seems that you would have to test whether the spark was oriented toward the GVM or elsewhere, but it doesn't seem too surprising.

Woods: I would like to mention briefly one system which we recently put into operation which is a radiation door interlock protection system. I think most facilities have some method of interlocking doors so that people cannot have access to radiation areas. We implemented ours with a microprocessor when the operator inserts a number through a thumb switch which corresponds to radiation in certain areas and it will interlock all of the proper doors and give him all the proper indications on the console of what's going on with respect to those doors. Of course, this is a very flexible system because you can change the configuration very easily by programming and it offers a very large number of configurations. The microprocessor is far enough away from the tank that there is no problem.

INSTALLATION OF NEC TUBES

IN THE ARGONNE NATIONAL LABORATORY FN

Peter Billquist
Argonne National Laboratory

Right now we are in the middle stages of what we hope will be an upgrading project on our FN. Three years ago we installed an NEC Pelletron charging system with an enclosed corona system replacing the column resistors, and right now we are installing NEC accelerator tubes with a parallel enclosed corona system for the accelerator tube, and an all-metal-to-metal vacuum system from the inflection magnet to our first analyzing magnet. As of Saturday night, I unplugged all the heaters on the accelerator tube and what happened since Saturday night I don't know. But this shows you a little bit of the details of some of the things we have put together. Figure 1. This is for those of you who have never seen such a thing hanging in an FN. This is a Pelletron charging chain in an FN tandem. We have two of them, one from each end. Figure 2. This is the terminal with a chain from the high-energy end here and with a chain from the low-energy end here. Figure 3. There sit two new 6 sect. modules of NEC accelerator tube. Figure 4. These are the connections to the heater plates. In between each section of tube are heater plates for bake-out purposes. Figure 5. That's 12 sections of tube ready to go in. They came in six-section pieces and NEC decided we should hook them together first. Figure 6. These are the support pieces for the tube. The brackets are mounted on the column and aligned with a disc the same diameter as the flanges on the tube, and then the tube is set in place on the brackets. The tube itself is then assumed to be on line. Figure 7. There is our first piece of NEC accelerator tube on the side. Figure 8. This is the high-energy end with the whole high-energy accelerating tube installed. We will have corona tubes top and bottom, alternately.

QUESTION:

Wegner: How do you supply power to the heaters?

Billquist: Eighteen very large transformers about 80 amps at 1 volt per heater plate. Figure 9. That is our homemade stripper box, which doesn't look much like a box. It has, in the back side, an NEC 115-foil drive mechanism and two Mini-Ti Ball titanium sublimators. The entrance aperture is of the order of 3/8 of an inch and the output aperture is 1/4 of an inch. Figure 10. This is a close-up of the entrance to the high-energy tube. Figure 11. This is a permanent magnet electron trap with a titanium sublimator hanging out the bottom. Figure 11. The nude ion gauges are here. Figure 12. This is where the ion sources will again be. In order to make room for every thing that had to go between the inflection magnet and the low-energy base of the tank, we had to move our 1961-vintage charge exchange source back for the fourth time along with our direct extraction source and our high-energy injector. The coils to the magnet are new; these are supposed to be good for 7KG. This is the original high-voltage switching magnet we use for an inflection magnet. Figure 13. This shows the 1X-1Y deflector, old 260-liter-per-second turbopump, and the first pieces for the low-energy injection system. Figure 14. This shows the electrostatic quadrupole triplet, 2-1/2-inch Einzel lens and the 2X, 2Y steerer

is inside the tank right up against the base plate. Figure 15. This is the same section, now, completely assembled and being baked out. It is interesting to note that the NEC vacuum flange system doesn't like it when you get it too hot. The technician that set that up got it a little bit warm and we developed a leak in it. It turns out that when you disassemble it you leave half the aluminum on each flange and then you rub for about three hours. This is a 1-inch, fast-acting valve that is supposed to, hopefully, keep any oil that won't, but might, come from a turbopump out of the rest of our system. Figure 16. This is from the low-energy end again but from the opposite side showing the 400-liters/sec Ultek DI pump and all systems assembled from this point. Figure 17. The high-energy end showing the initial assembly. Figure 18. This is the high-power Faraday cup which will be water cooled and again a 1-inch, fast-acting valve. These fast-acting valves are strictly spring-loaded, and it's kind of unique having to find the handle and figure out how to get it inserted into the thing, and you crank the thing around and hope you don't slip. Figure 19. Our rough-out system for the NEC tubes uses an oil-free pump, that we pump down to maybe 15 inches or so and then we use a pair of absorption pumps consecutively. Figure 20. The entrance to the number 1 analyzing magnet includes magnetic steerers from ANAC and an NEC entrance slit system.

DISCUSSION:

Adams: Pete, you said you lined up the brackets that hold the tubes with rings and then you hung the tubes on there. Did you realign after that?

Billquist: No, sir.

Adams: You don't worry about column sag or all the weight put on near the terminal?

Pete: We simulated the weight of the assemblies at the terminal. There is really no way, with any precision, to align the accelerator tubes themselves. There is no way to make decent targets that go into the accelerator tube itself.

Schultz: Did you eliminate terminal steering or wasn't it too visible,

Billquist: We eliminated terminal steering also.

Schultz: Does the foil stripper hang with the band sideways, I assume?

Billquist: The foil stripper hangs at about a 30-degree angle toward the inside of the terminal.

Schultz: I saw no accommodations for pulsing, is that future?

Pete: The buncher itself is being rebuilt, i.e., repackaged in the all-metal system. The original one was conventional construction. We have a dummy in there right now. We will be back with our bunching.

Schultz: What caused you to eliminate the terminal steering?

Billquist: We feel we shouldn't need it with straight tubes.

Hurley: Concerning the NEC foil changer, we have doubled the number of foils in our foil changer by putting a foil holder in the inner spaces and changing the size of the roller where the foils break the beam.

Chapman: The heaters between the tube sections on your installation will only be available for use on initial assembly presumably or any other time you chose to make a tank entry. You have no power-driven generators through that column length which will enable you to operate any of those heaters as you can in the normal NEC machine.

Billquist: We will not have power available to the heaters, except when the tank is open.

Middleton: How much insulating length have you lost?

Billquist: There is not a direct correlation in size between the accelerator tube and our column. We have lost probably 8 to 10 inches in insulating length on the accelerator. I don't know the number precisely, but it's about 2 inches on each of our four sections.

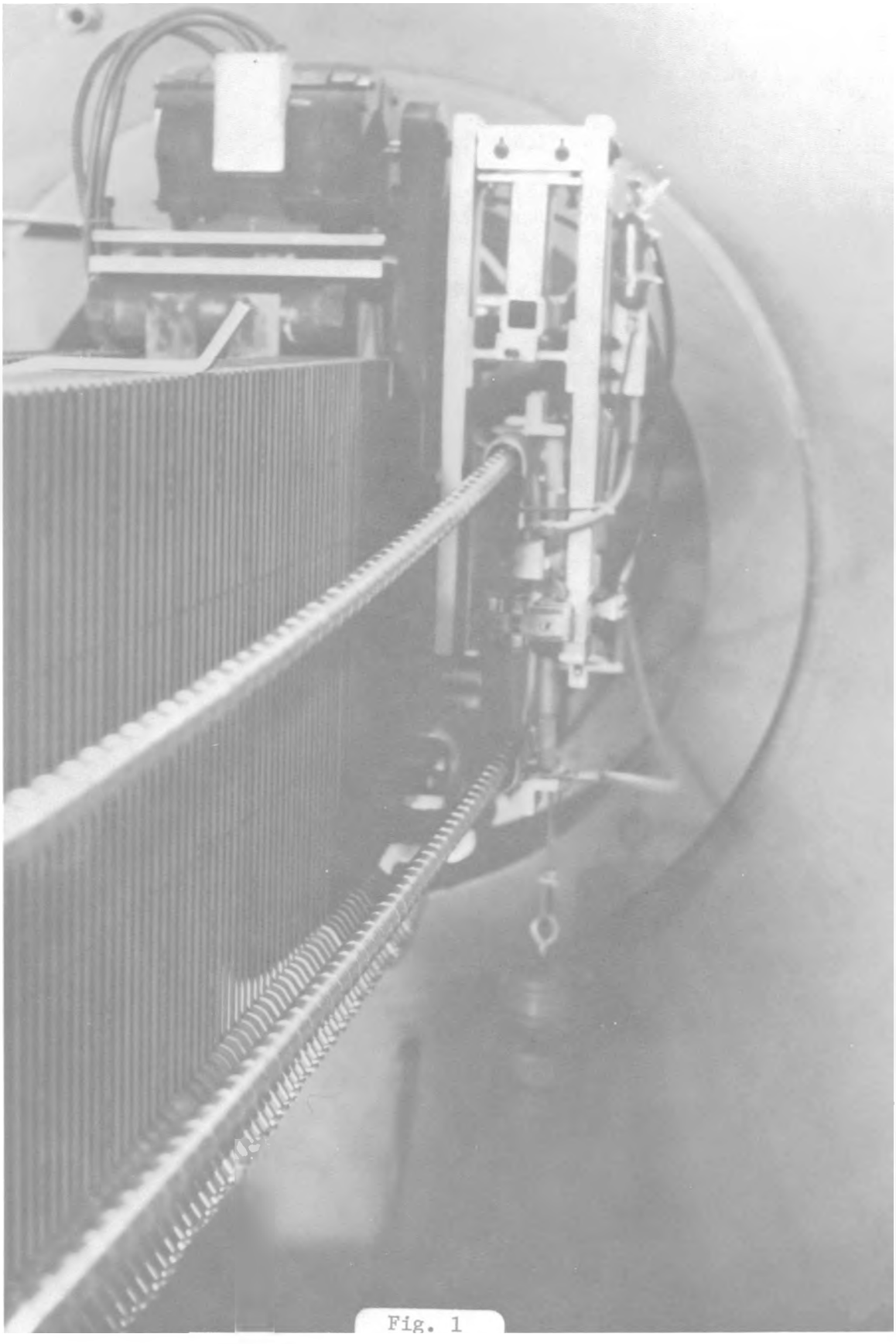

Fig. 1

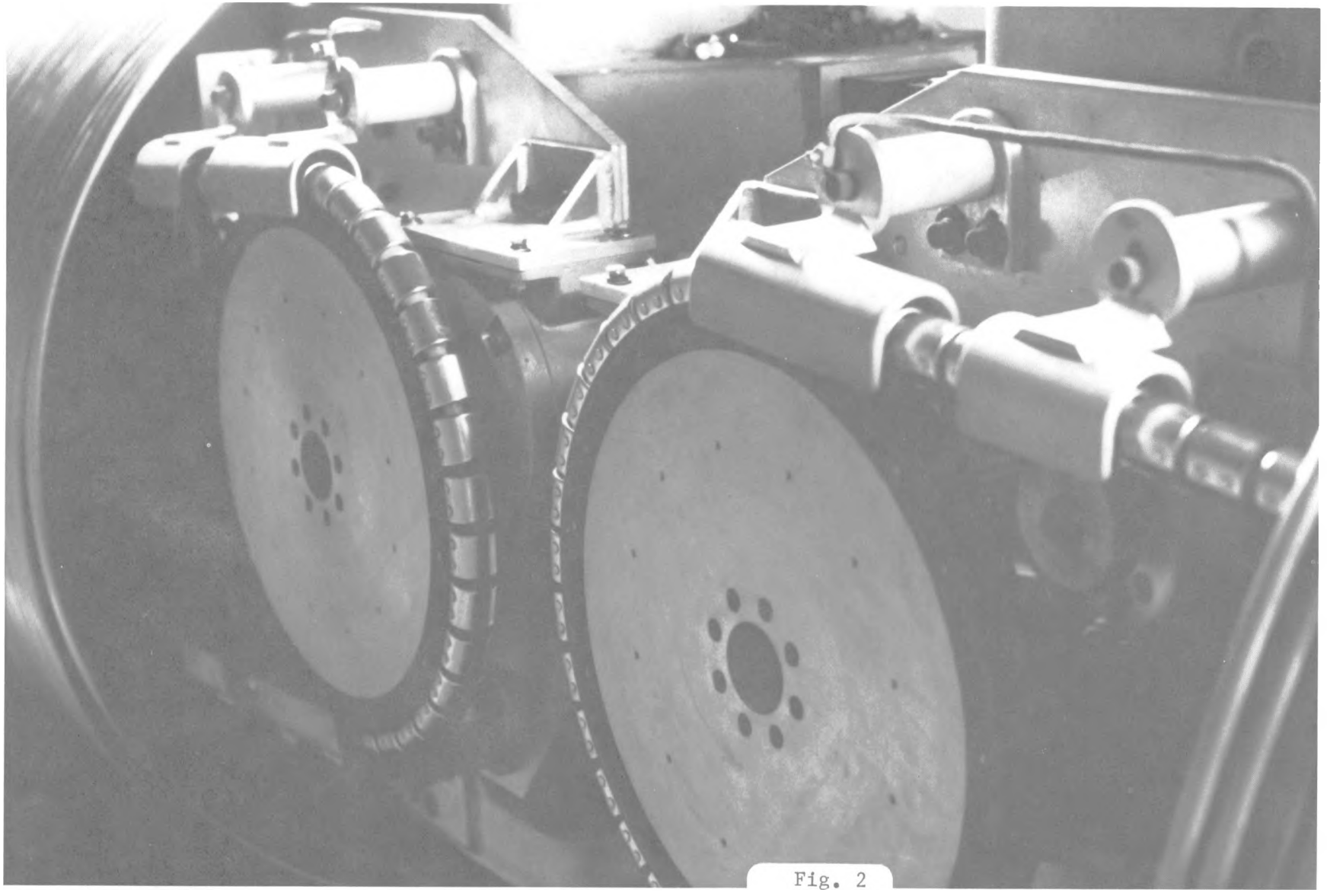

Fig. 2

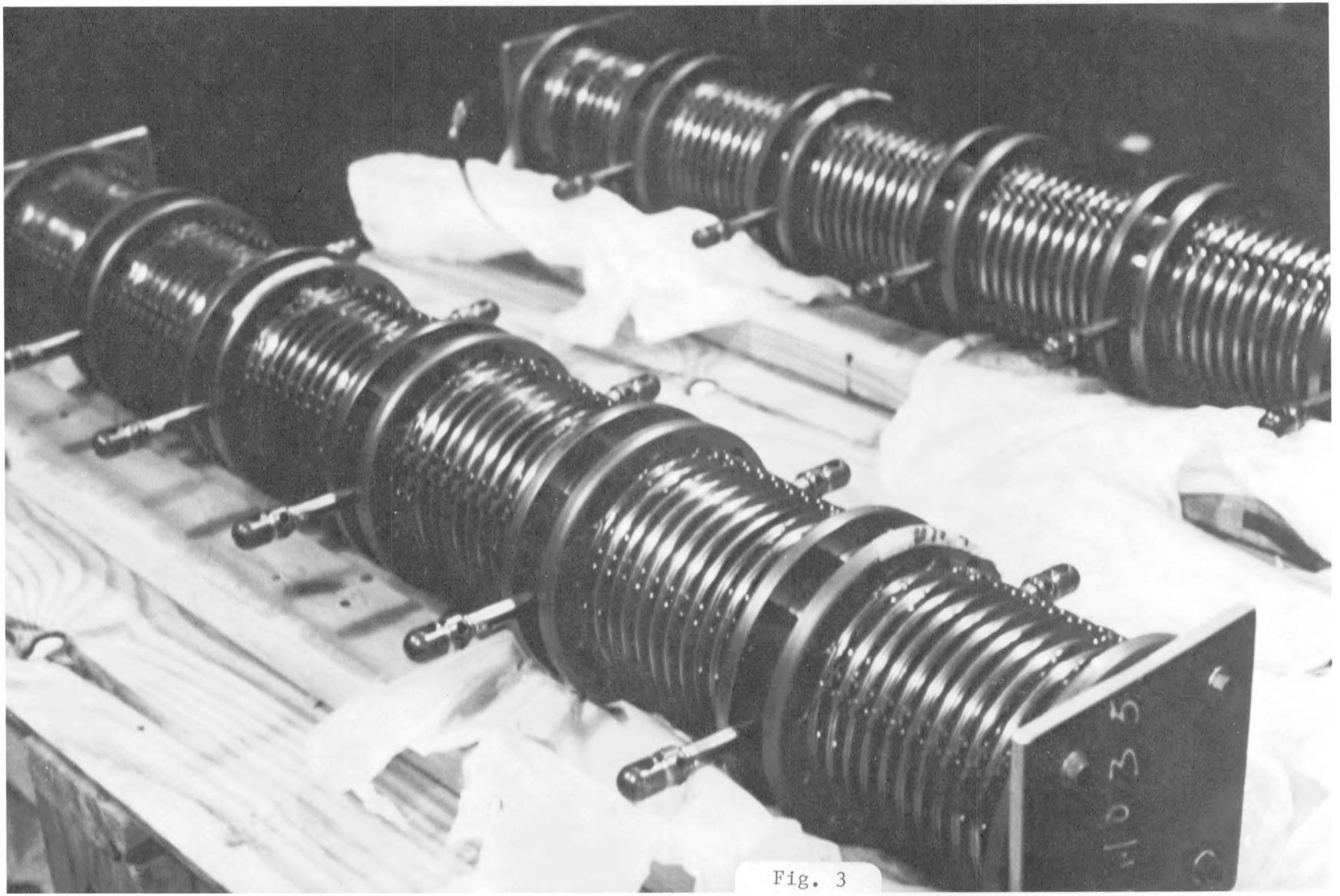

Fig. 3

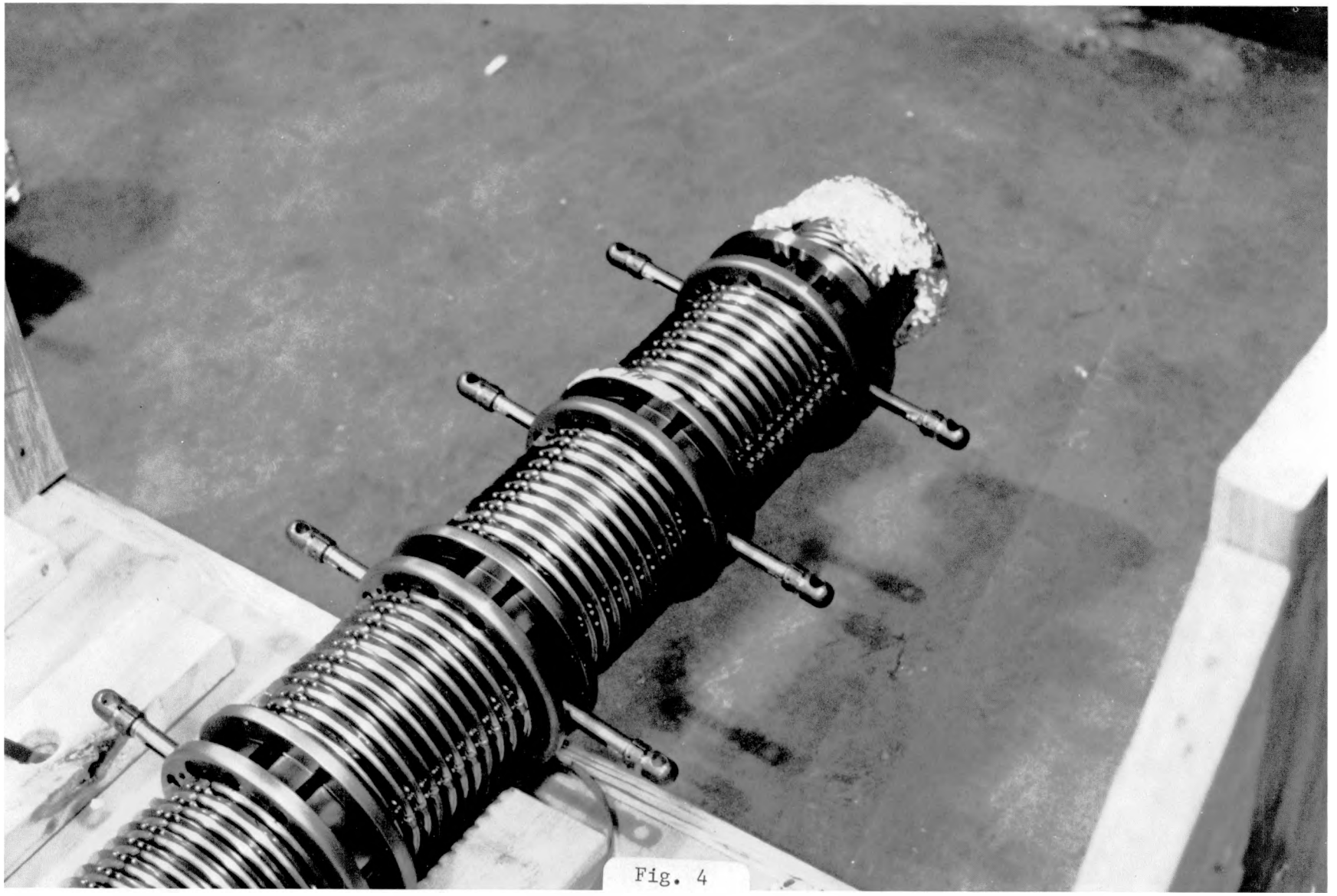

Fig. 4

Fig. 5

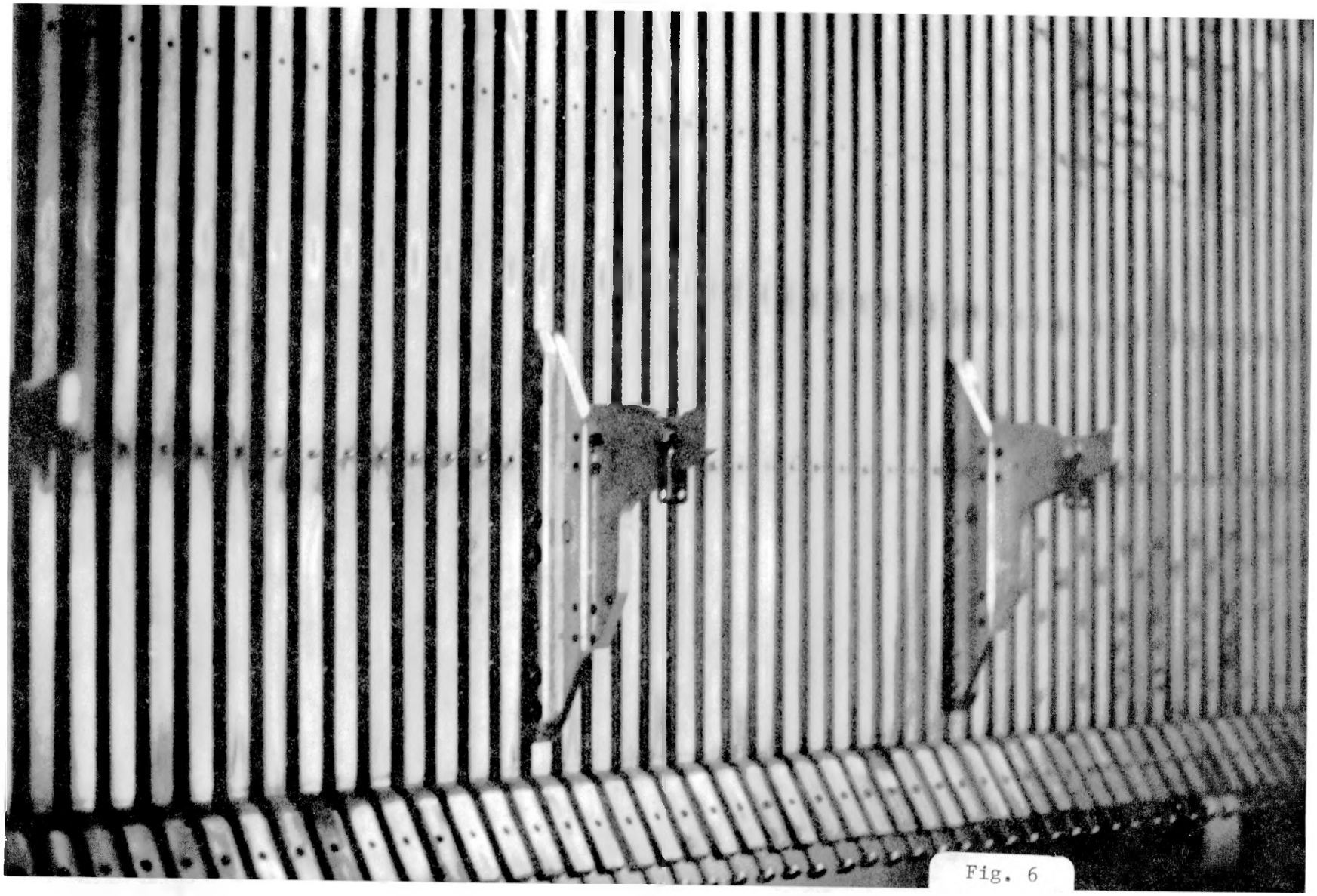

Fig. 6

Fig. 7

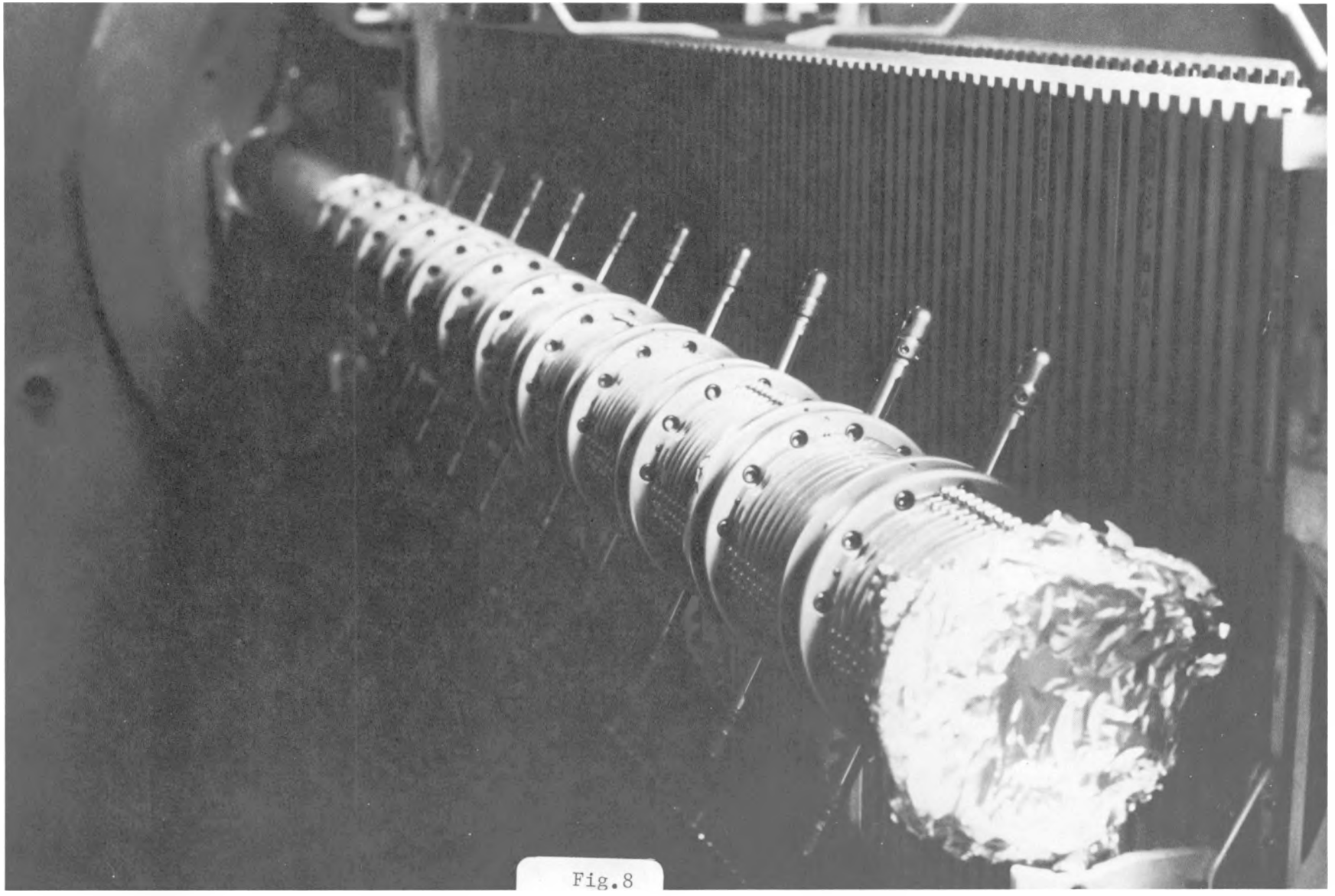

Fig.8

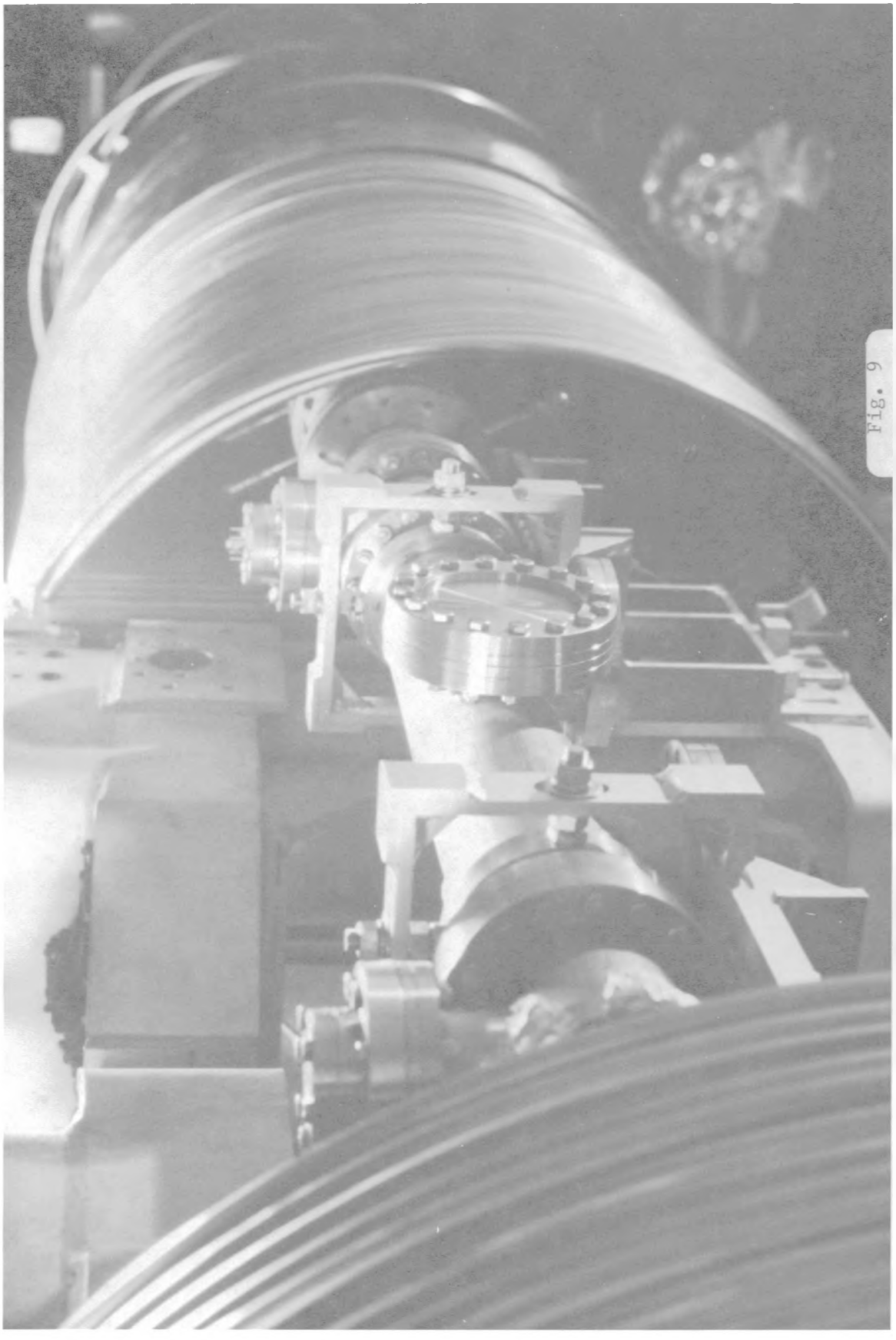

Fig. 9

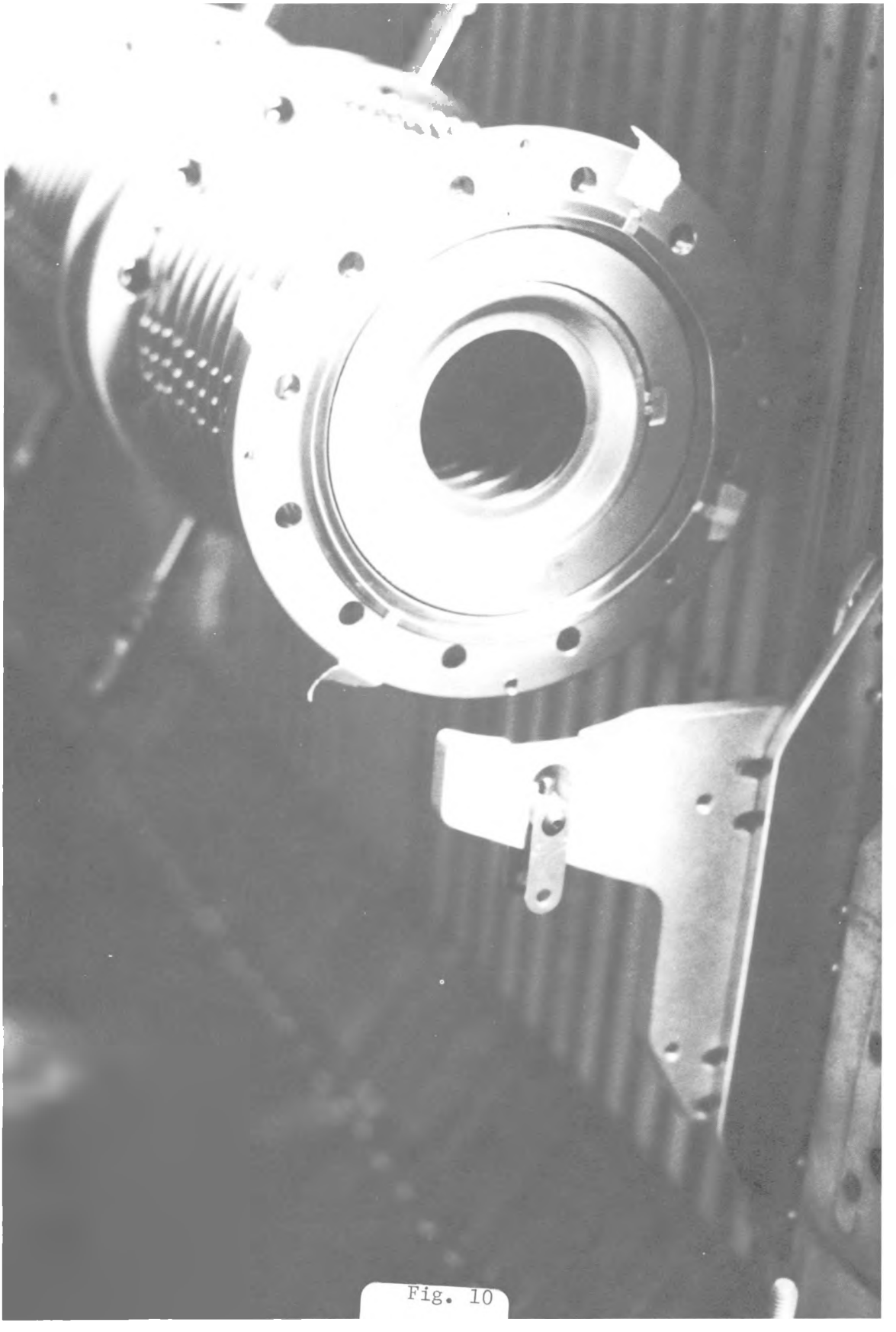

Fig. 10

Fig. 11

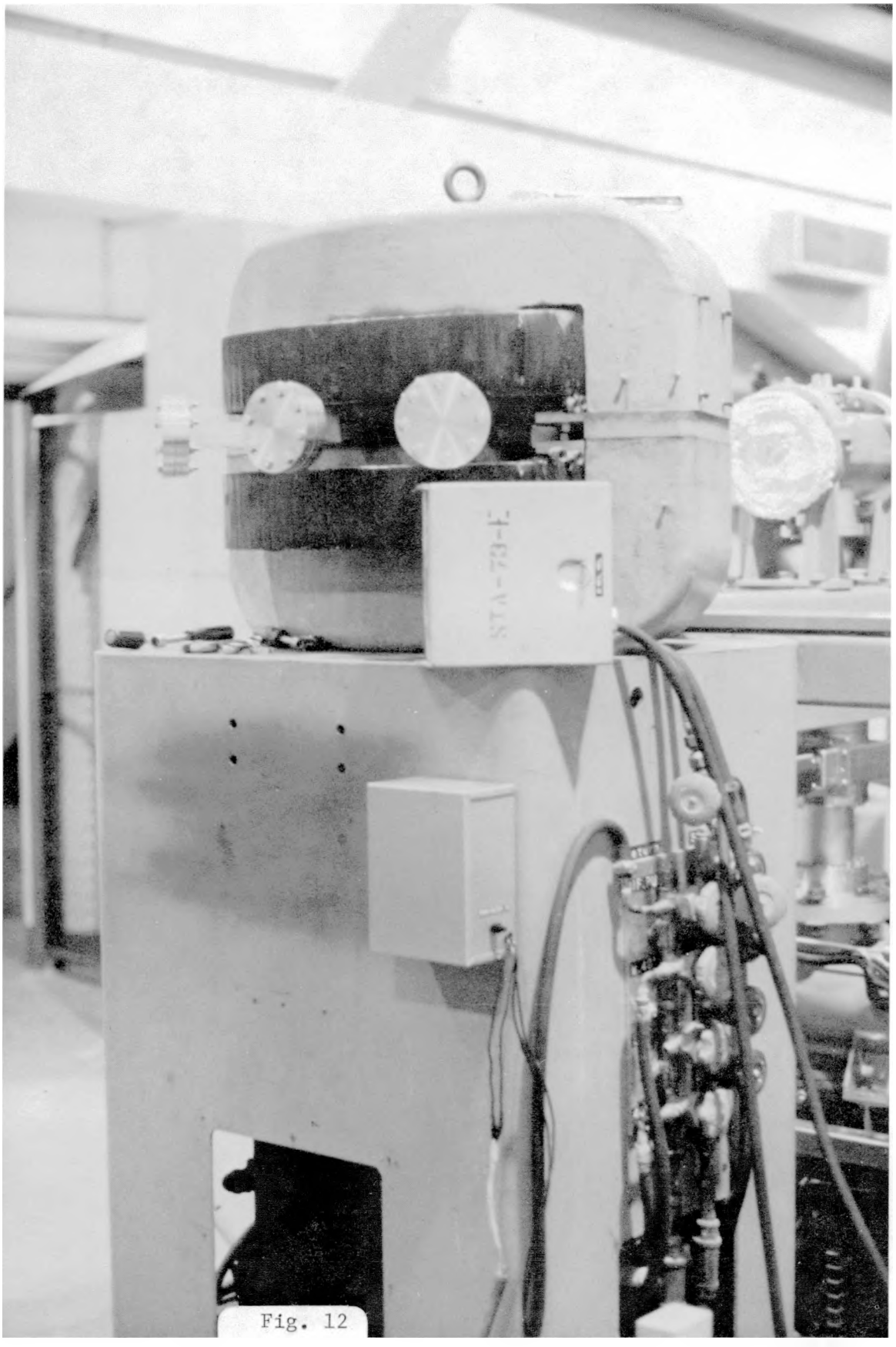

Fig. 12

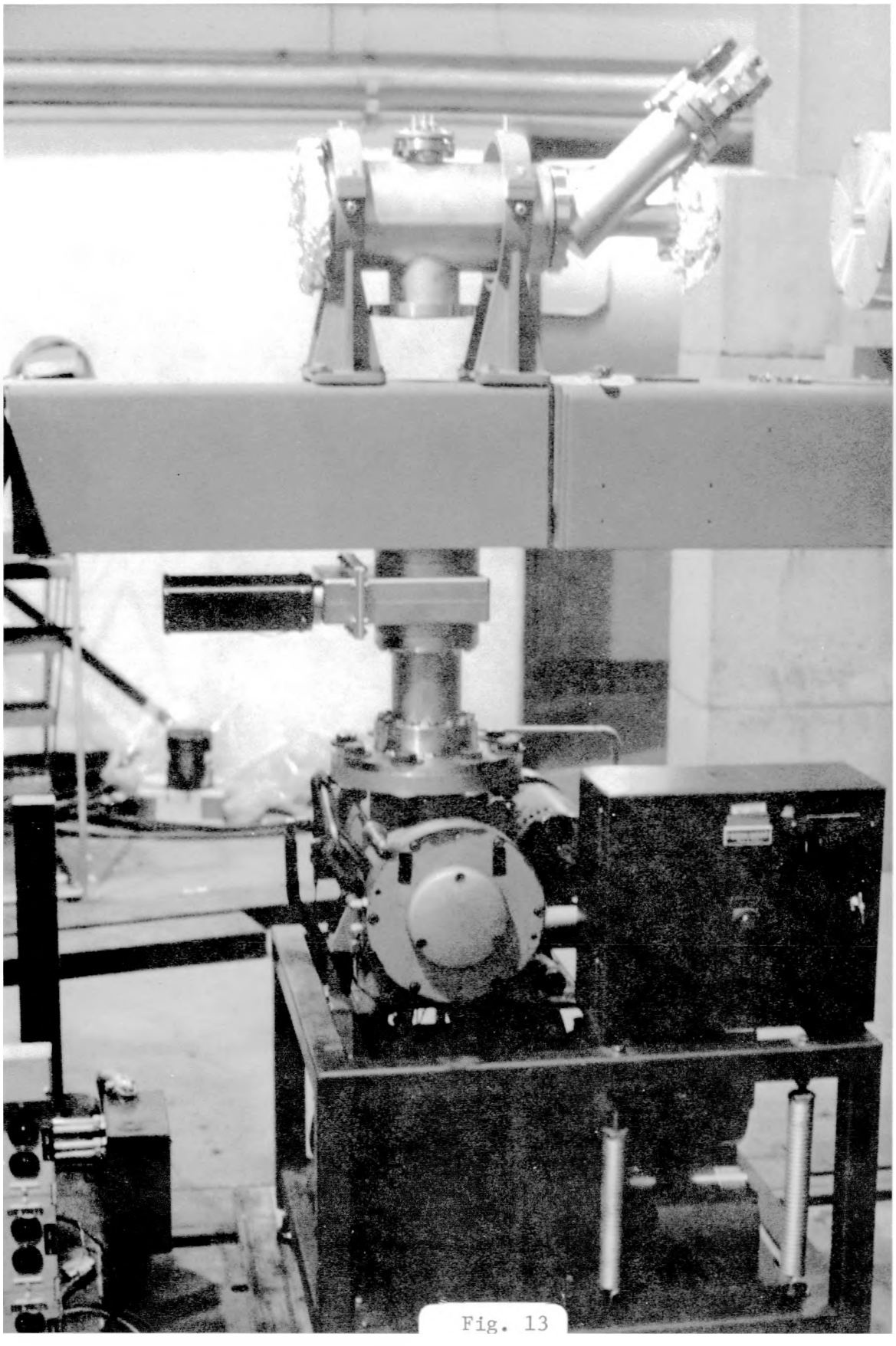

Fig. 13

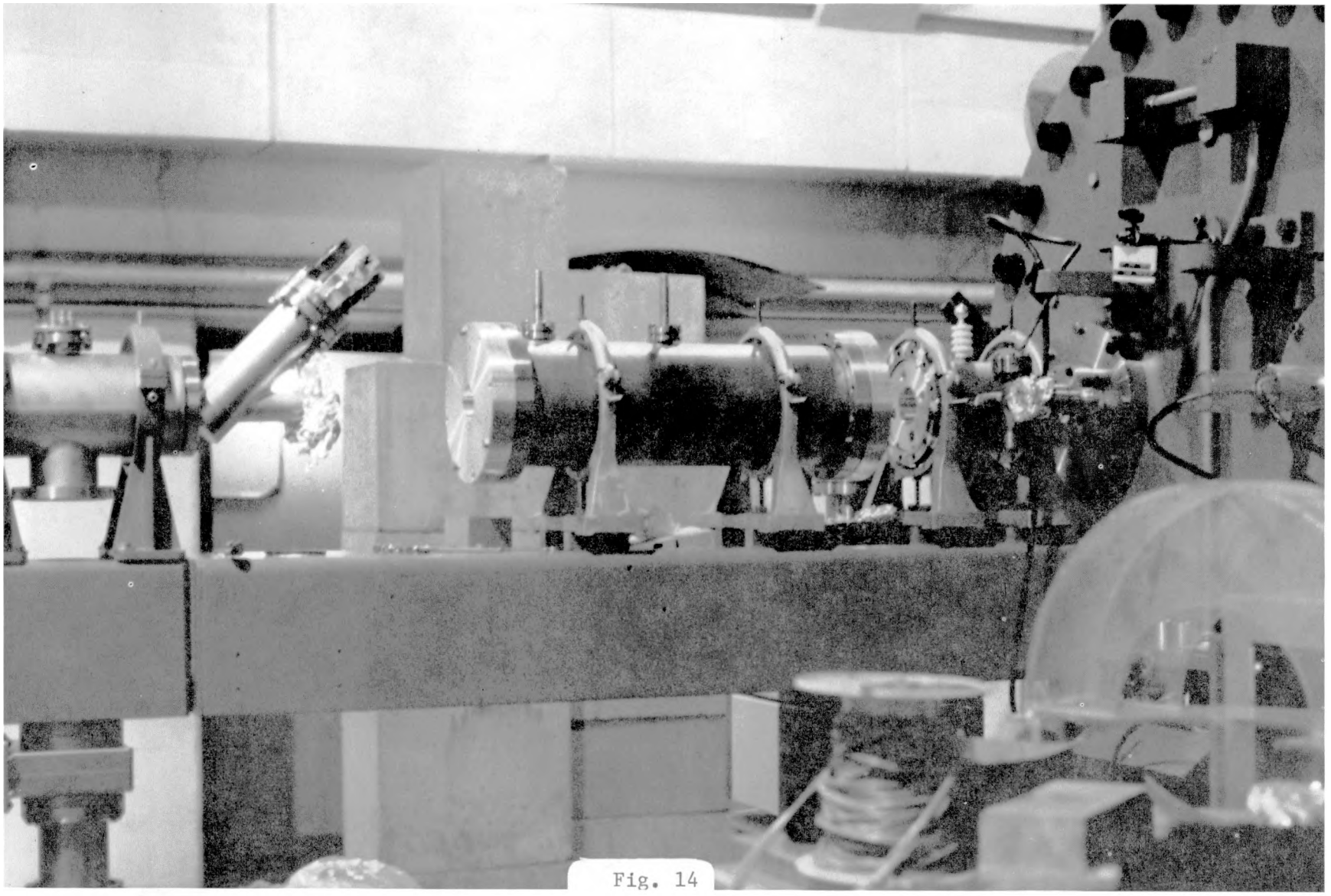

Fig. 14

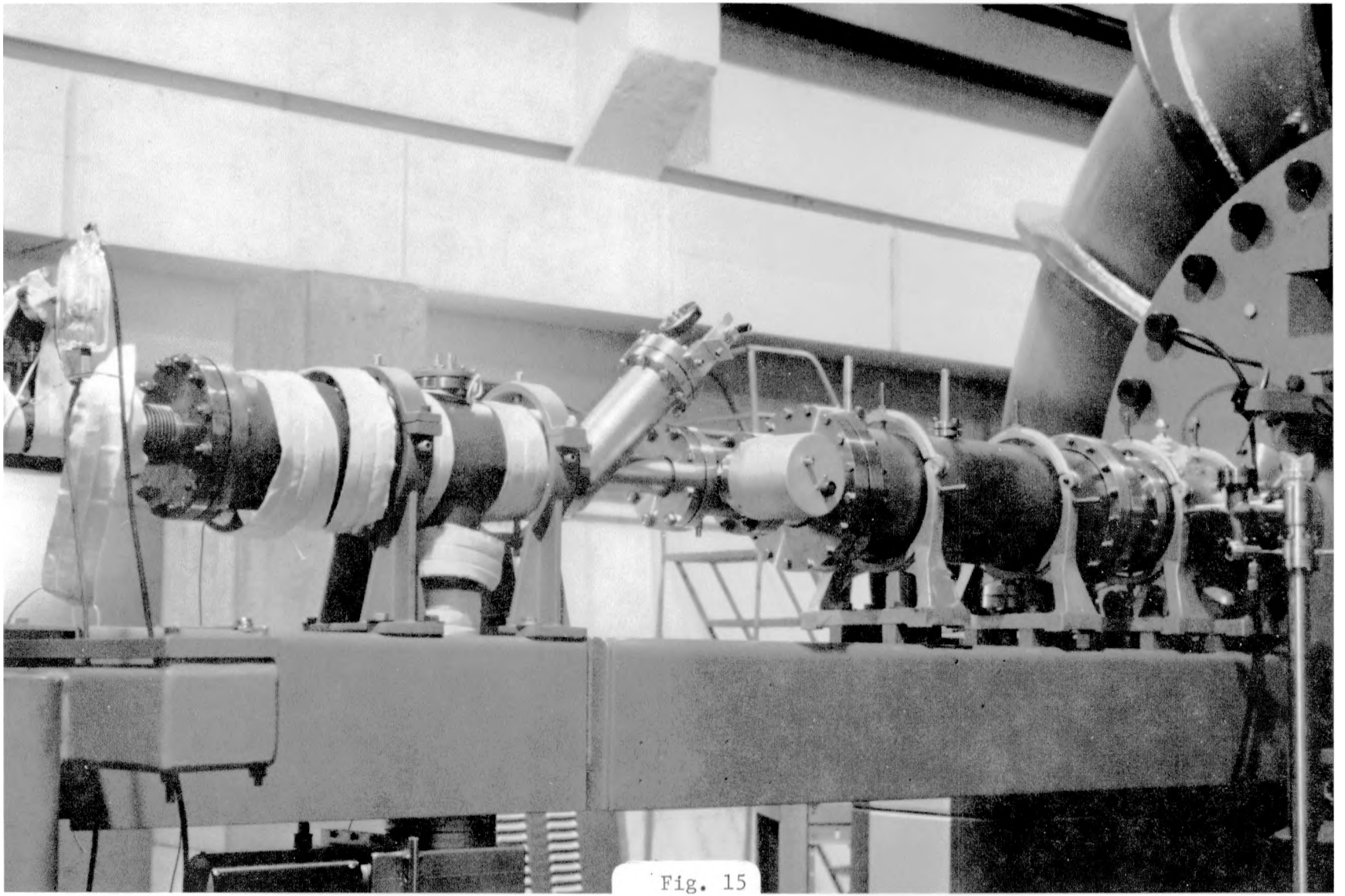

Fig. 15

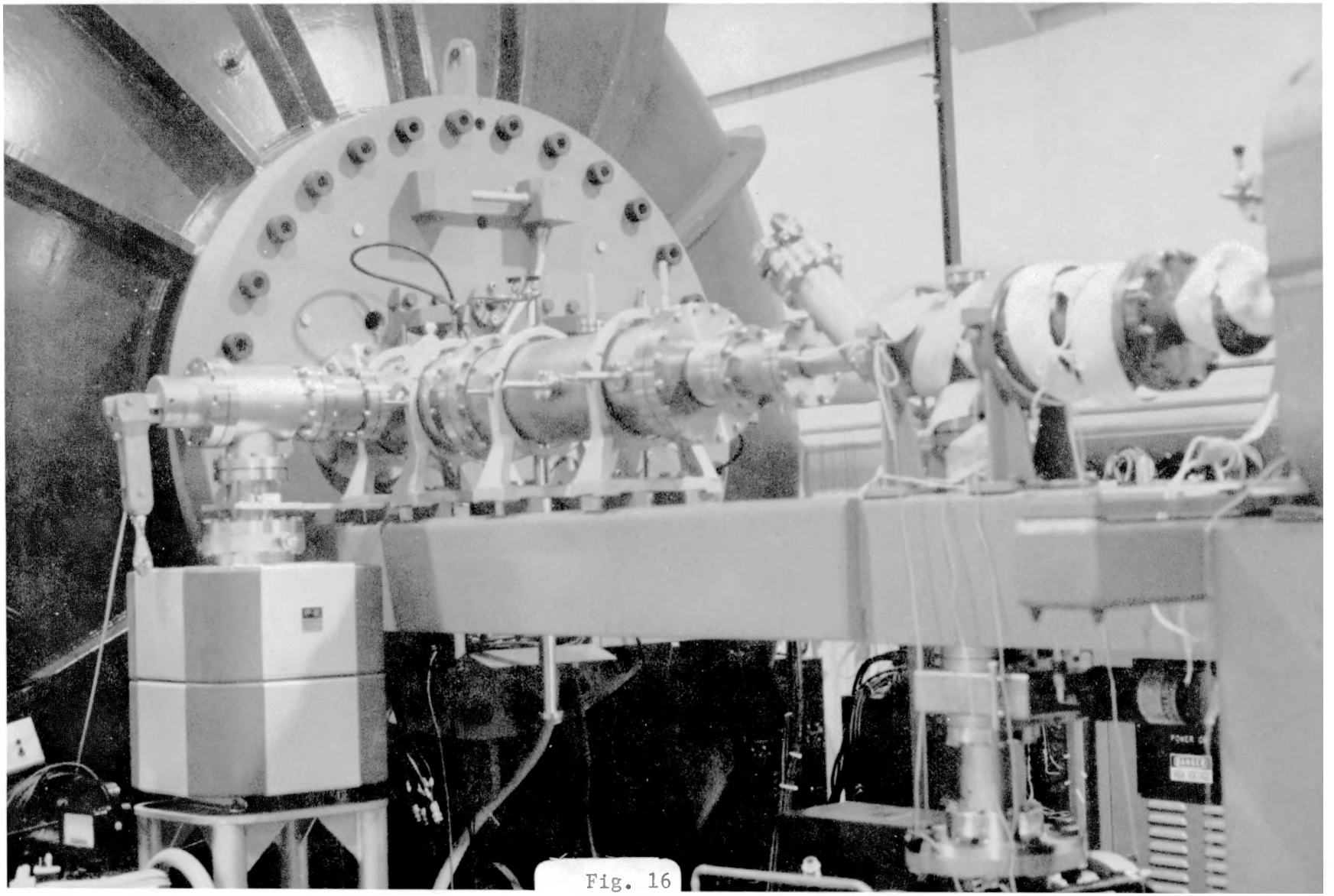

Fig. 16

Fig. 17

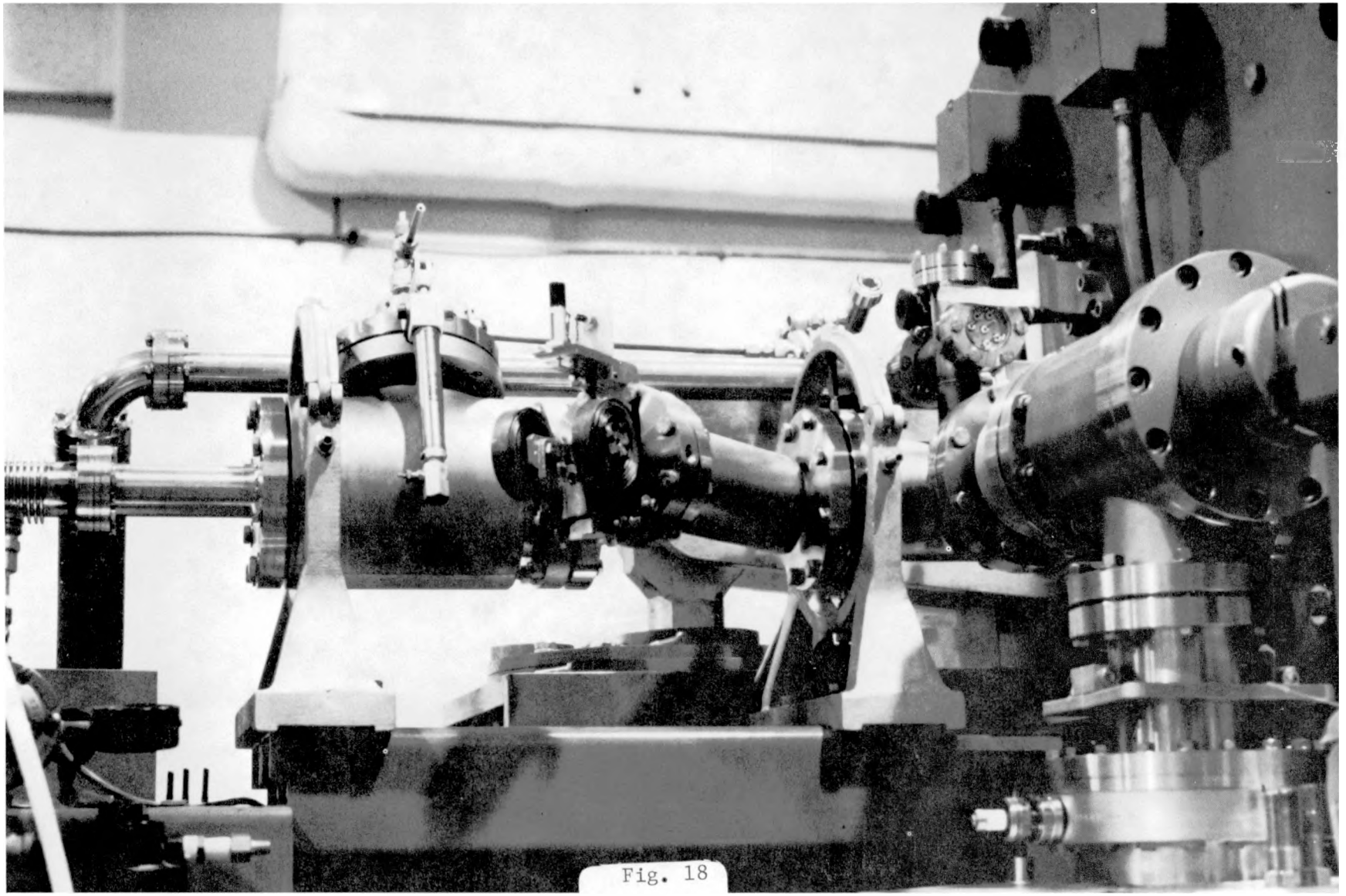

Fig. 18

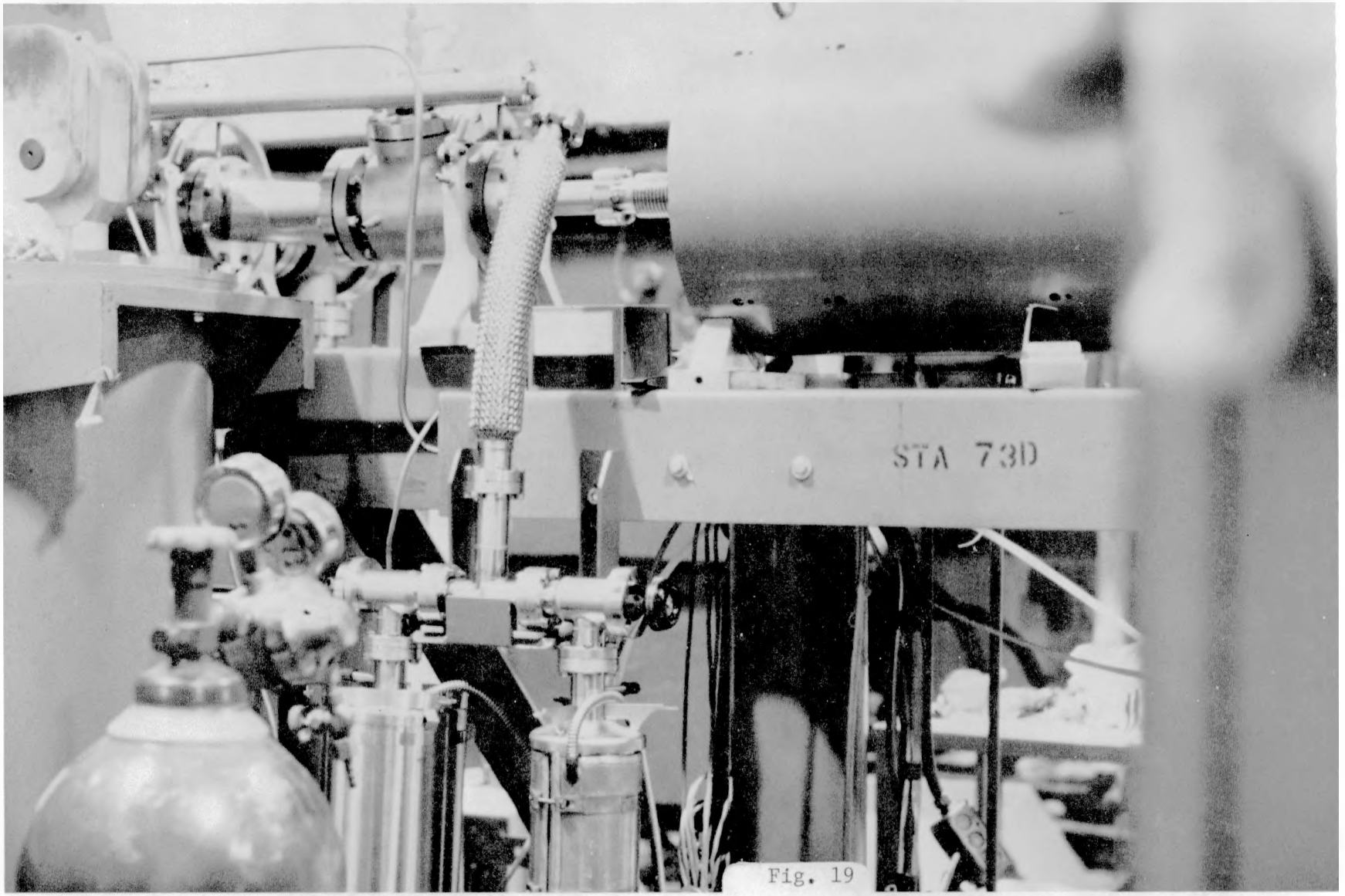

Fig. 19

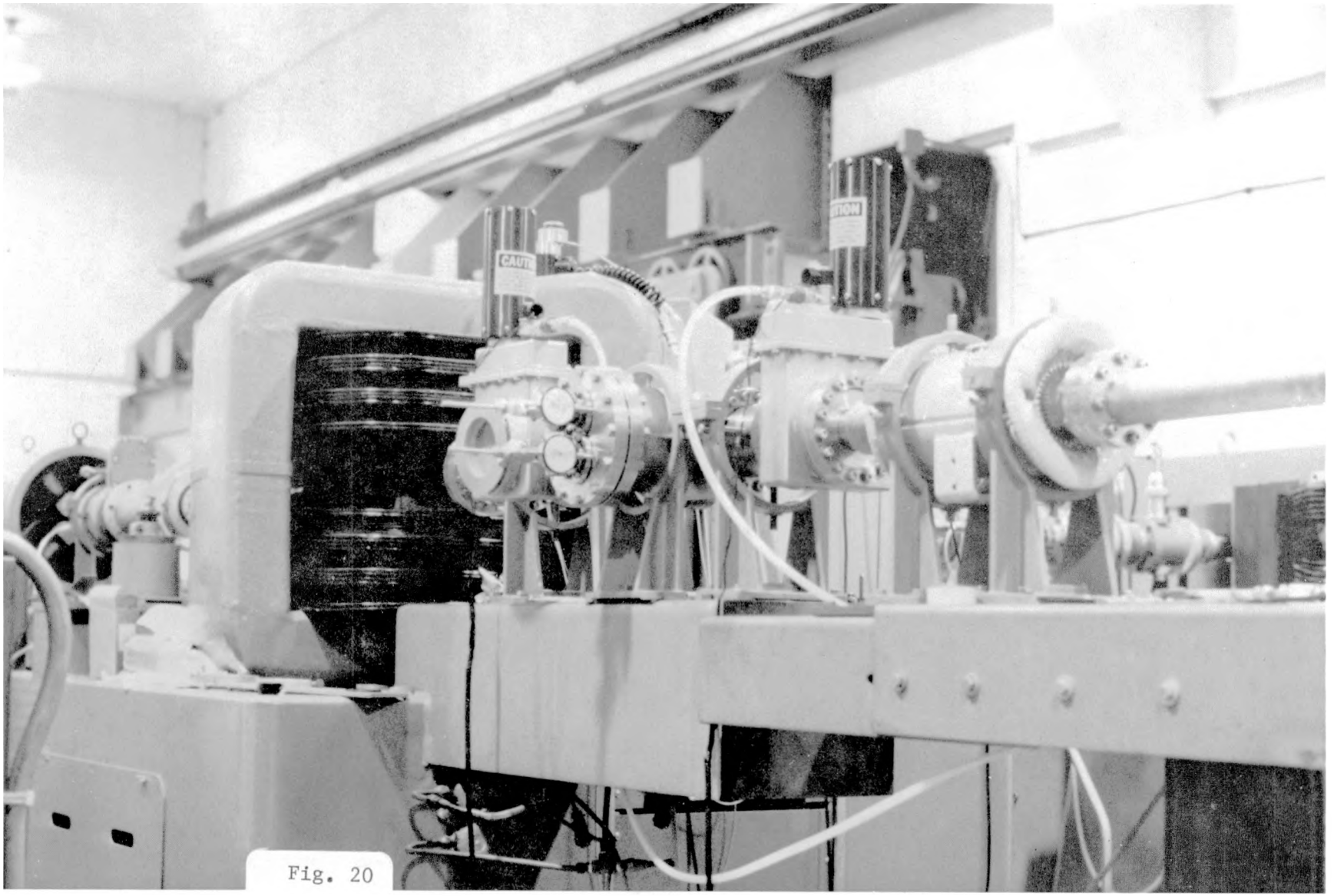

Fig. 20

PELLETRON INSTALLATION AT BROOKHAVEN

Bob Lindgren
Brookhaven National Laboratory

After the trouble we had with charging belts, we were very pleased that we had a Pelletron practically in house. On April 26, we decided to shut down and install the Pelletron. It took us about three days to have the machine completely stripped of belt system parts. We cleaned the tubes in the column as best we could with the dry method. We did not use the water spray in MP-7. We had tried that one time in MP-6 and it worked out very nicely. While the Pelletron installation was being carried out, we had to replace a lot of the connections between the column members, since the dust collectors in the high-energy end were an electrical connection in an MP. We replaced those with a piece of 1/8-inch steel rod with banana plugs in each end. We found that gives us a very low inductive electrical connection. We are using this technique now in the low-energy end, both top and bottom of the column, to give us the electrical connection between the column members. We also installed additional magnets on the accelerating tubes for a little more electron suppression. We had tried this out at one time on one of the tubes of the machine, tube four, and we felt that we could do with more electron suppression. We have added these additional accelerator tube magnets to all the tubes, and the results that we have seen indicate that it has certainly paid off. The results that we see are tied in with results we see from additional spark gaps that we installed on the acceleration tubes at the same time. I am not sure whether it's the additional electron suppression or the additional spark gaps. The stainless-steel tubes, as they come from High Voltage Engineering, come with about 0.200 in. gap which says that it shouldn't fire in the SF_6 mix that we use until the voltage gets somewhere up to about 200 kilovolts, so we narrowed that gap by adding additional gaps on each electrode. Our gaps are a 0.110in., normally, and they may vary from 0.090in. to 0.130in.. Also along with the Pelletron, we got two 3-kilowatt alternators that are mounted in the terminal. We plan to have a switch that will allow us to select the ac output from either of these alternators to power the circuitry in the terminal so that we could run with one chain only, if necessary. I have a few slides and most of you have seen pictures of Pelletrons in MP's and I don't want to repeat all of that. Figure 1 shows the tube resistors mounted on a section of old aluminum tube that we had. We, at one time, had a tube drop out of a machine and that's a piece of the aluminum tube. We have mounted the additional spark gaps right along side of the built-in spark gaps. We have clamped our spark gaps on with clamps that are very similar to the type that are on the Rochester titanium tubes. They can be put on or taken off at will. We have six on each electrode. We have decoupled all of the tubes of the machine now, so that we are using 800-megohm resistors on the tubes, and we have built in an additional spark gap on that resistor that goes straight across. We got that idea from Florida State. I think that they were the first to try additional single spark gaps. Since putting additional spark gaps on all of our resistors, we have lost very very few. Figure 2 is a view, from the outside of the machine when it is closed, of the high-energy drive shives. Using the old belt installation port on an MP, we mounted a port in the middle of that large port with a glass window so that we could see what's happening to the high-energy end. It has proved very useful. Just at the beginning of last week we had a peculiar phenomenon on the high-energy charging chain, and just looking in

through the window we could see why. Some of the bolts holding the inductor were completely out of place and leaning right against the chain. A few bolts that should have been installed with Loc-Tite weren't. We didn't have to run very long before we could look through this port and see this trouble. Figure 3 shows the large motor installation port with the new port we installed on it. I think it is a very useful addition to an MP. As far as troubles go with the Pelletron, we are very pleased with the operation of it. We can close the machine up and then eight hours later be at 12 million volts and from 12 to 14 may take 2 to 3 hours more, perhaps. We feel we never would have gotten to 14 if we still had a belt to dry out or a belt to spark down. The Pelletron has given us very few sparks down the length of the chain. One can count the sparks that one gets down the chain by looking very closely at the mating ends of the pellets to see how many pits there are on them. In 2000 hours of operation there may be about 20 pits. One very large problem we have had since our installation is, with only 2000 hours on the chains, we have used up 57 pulleys out of 144. There are 144 4-inch idler pulleys in the machine. There are also six-inch idlers that carry the weight of the chain in the dead sections. We have not had any of the six-inch pulleys go bad. We feel the grease used by NEC in these bearings on the 4in. pulleys was the wrong choice, although it's the same grease that Heidelberg and Munich use in the same bearings and they have not noticed any trouble. They report failure rates of about 18 bearings per year. In a matter of three months we have had 57 failures. Beginning October 3, NEC is cooperating with us to change all of the bearings on these pulleys and we expect our bearing problems will be gone. We don't have belt dust anymore. We have urethane dust and chips in the machine. For the first 1000 hours of operation, we did not have urethane dust, we had nylon dust off the charging shoes and that was as bad as bad belt dust. It caused sparking. But that white powder disappeared after the first 1000 hours. I understand that most Pelletrons, when they are made, are run in for about 1000 hours at the factory. Ours wasn't so we got that white dust when it was installed in the machine. Another problem that we were plagued with, and I understand that Pelletron installations all over are plagued with, is trouble with the cables leading from the power supply outside the tank, in through the tank insulators to the inductors. These are always 50-kilovolt power supplies and RG8 cable is used to run this voltage up to the tank feed-through. Then, from the tank feed-through, one can run bare copper cables. Chalk River uses a wound plastic hose with a metal spiral in it to carry the voltage up. We tried just the inner conductor of RG8 with the shield off of it and ran it from the inside tank feed through up to the inductor. We had so many sparks that it blew out the cable external to the machine. We tried spark gaps and inductances on the outside of the tank. Those were the only measures we could take while the tank was closed. I believe we have that problem solved completely now. Michael McKeown will give a talk on the spark suppression box that he has put inside of the machine that has completely eliminated that problem of break-down even at operating voltages to 14 million volts. When we get a 14-million volt spark it's loud and it's powerful!

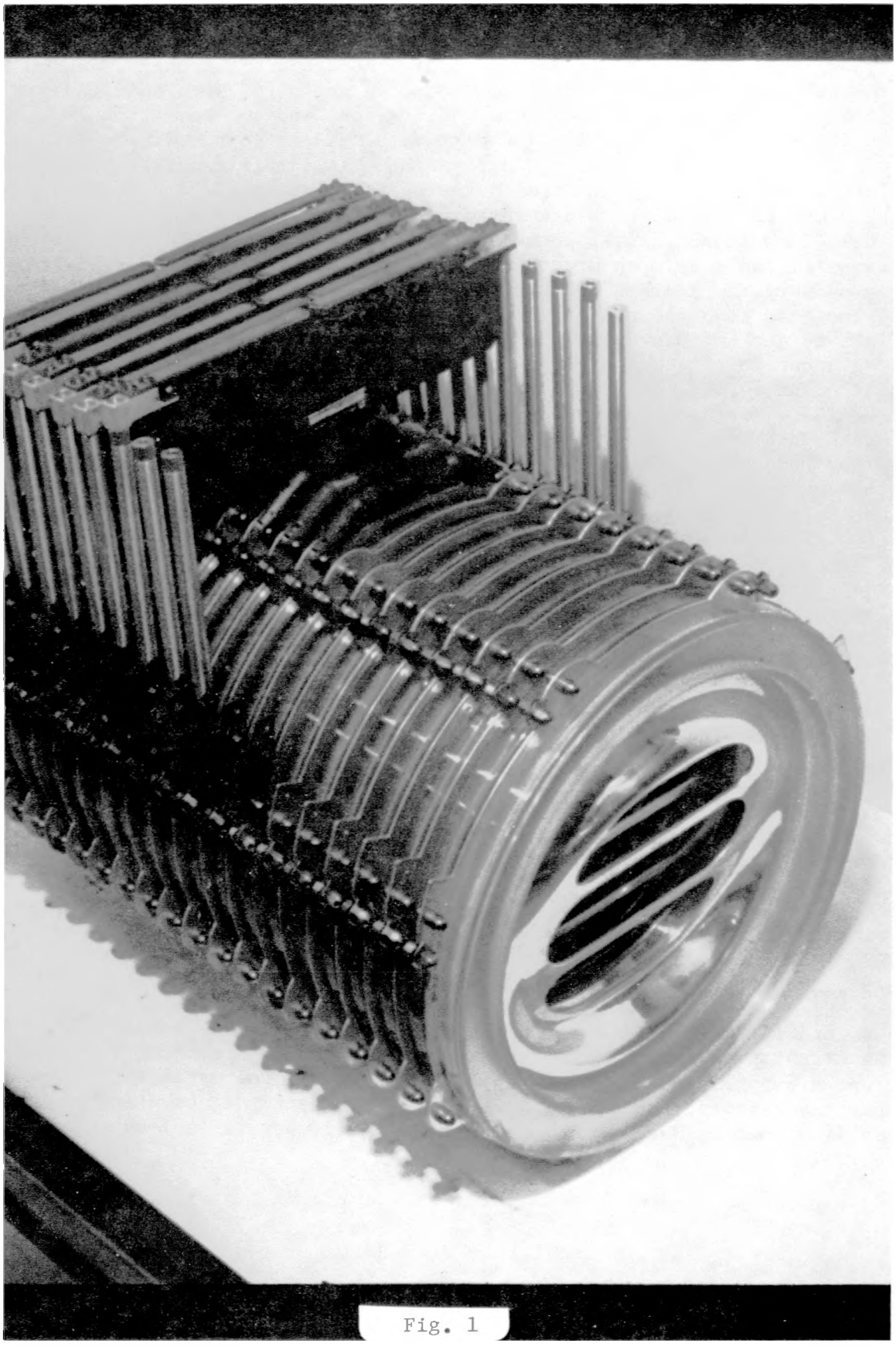

Fig. 1

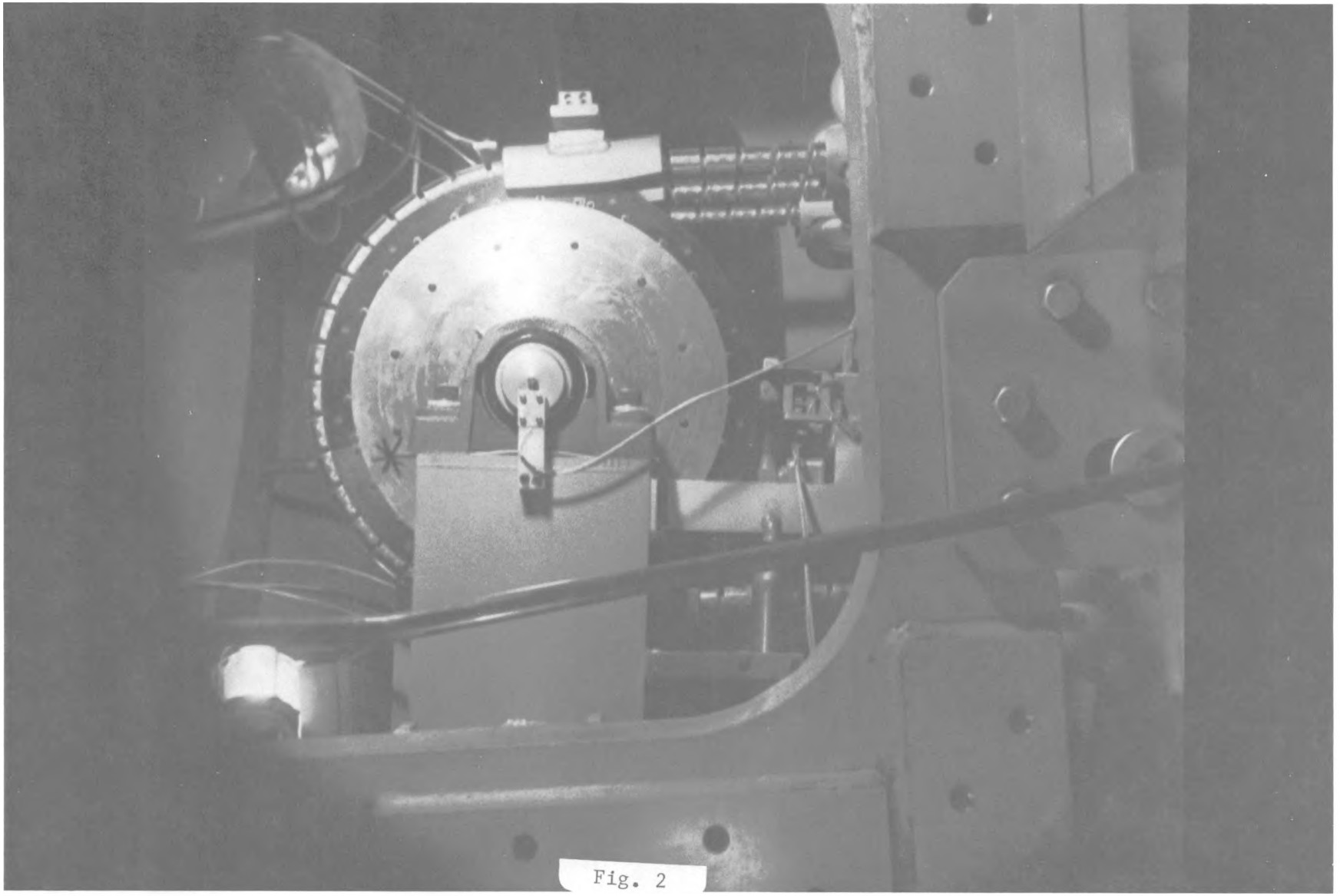

Fig. 2

Fig. 3

Session IV, Chairman, J. Janzen

BELT CHARGING SYSTEM, CHARACTERISTICS, PROBLEMS, STRUCTURES

M. Letournel
Centre de Recherches Nucleaires - Universite Louis Pasteur

In Strasbourg, as in other laboratories, we have belt problems with our MP machine. I spoke last year about some changes, mainly the half-open structure with supporting rollers in some dead sections. Since then, I have become convinced that in an MP, under certain conditions, the belt suffers severe electrical damage, not from sparks, but permanent electrical damage due to improper design in the MP belt guide structure itself. In May, at Strasbourg, at the accelerator conference, I put forth a theory to explain it. Since then I have made practical measurements which confirm my theory. So, from the practical point of view of the users, I will try to give you the philosophy of my argument. Of course, my experiments were made with the MP machine using SF_6 gas, but I would like to point out that it is valuable for any machine and with few changes it allows me to calculate any charging system, including Pelletrons and Laddertrons.

From experiments carried out in a pressure vessel using normal MP spacers and a metallic plate, Figure 1 shows curves of breakdown voltage versus SF_6 pressure for different distances between the spacers and the plate. As you can see we don't get a major improvement by going to a higher pressure.

Results are reported in Figure 2 which indicate a slope of 80 KV/cm for the curve of breakdown voltage versus distance for the standard MP spacers. The same curve, carried out with rods of 18mm diameter, shows a slope of 115 KV/cm. The top curve is a very conservative curve for a homogeneous field which shows a slope of 230 KV/cm.

In Figure 3 is reported the curve for MP spacers with the 80 KV/cm slope corresponding to an electric field comparable to a charge density of 7 nC/cm^2.

By the way, in our belt machines a uniform charge density of 1 nC/cm^2 corresponds roughly to a belt charge current of 100 μA. On the other hand, if I spray 9 nC/cm^2 on the plane electrode with the metallic or insulator MP spacers being at ground, and if I increase the distance between the electrode and spacers, the voltage developed between the electrode and the spacers goes up along a curve as $V = \frac{Q}{C}$ which, at 9 mm, cuts the preceding curve and a discharge appears. But if the electrode remains within 9mm, nothing happens. If I now spray on 6 nC/cm^2 instead of 9 nC/cm^2 and increase the separation, there is no limitation for the separation distance, the voltage curve is always below the Paschen curve.

If we bring another spacer on the other side of the plane electrode (Figure 4) the field looks towards both sides according to the written equations and the

voltage developed is less than in the previous case. Shown in Figure 5 is the situation of a charged belt during a short circuit terminal experiment where we must correct the electric field on the spacers by the factor $\frac{25,4}{18}$ to first order as shown if we apply Gauss's law. I will come back to that later.

Let us come around the drive motor where we charge an MP with 400 μA, that means 4 nC/cm^2. I would like to add some considerations. Due to the grooves in the pulley and the corona discharge process there are some reasons why you get charge inhomogeneity. We must be aware that the belt thickness of 4 mm is a distance of importance for the calculations and the charges are located only on the top surface of the belt, which is less than 40.10^{-8} cm in depth. Because of the nature of an insulator, there is no motion of any kind from the belt in the charging or discharging process. It is the same in the case of an uncontrolled discharge from the spacers due to too high a voltage difference between the spacers and the charged belt. Ions, which come from positively or negatively ionized SF_6 molecules near the spacers or in SF_6 space, are projected onto the charged belt, in order to neutralize the belt charge. Under this bombardment, which is a uniform flow at the beginning and becomes very harmful later with higher voltage, the belt will deteriorate.

When the charged belt leaves the drive motor, the belt voltage goes up as in a homogeneous field to the first order. When entering the column structure, the charged belt first encounters a really rough structure which determines an inhomogenous field and, second, it also encounters the longitudinal electric field between the spacers due to the terminal voltage. The belt location between the spacers, and subject to both belt-charge field and the longitudinal field, determines in the spacer geometry, an electrical situation made of equipotential surfaces and lines of force shown in Lehmann's diagrams. These were quickly made for this conference and consequently they have to be taken as a first approximation. Figure 6 shows the situation of an uncharged belt between live spacers which is the case of the down-belt run. Figure 5 shows the situation of a charged belt right in the middle of dead spacers which is the case of a short circuit terminal electrode experiment.

Figures 7 and 8 show the combination of both fields in some particular belt locations, which can be the case for some part of an MP belt run. On the diagrams we can see that there is a larger electrical strain situation between one side of a spacer and some parts of the charged belt, whereas in some locations there is almost no field. Let us examine the position of the belt point A. Following the line of force up to the spacer to integrate its voltage compared to the spacers, we see a dependance on the belt charge, on the spacer voltage, on the belt spacer gap, and on the general geometry. In fact, it is more complicated and the curves shown at the beginning are not the only concern, but calculations can be easily done. They show that under normal conditions this structure is critical at the limit of a distance of 5 - 10 mm for 13 MV and 400 μA. No wonder difficulties arise if we exceed 4 nC/cm^2 somewhere, because of charge inhomogeneity, or if we push the belt charge for conditioning, or if we really need more than 400 μA. This situation is shown in Figure 9a. The situation is the same for the case of a resistor which exceeds the normal 400 mΩ, which is not an uncommon occurrence. This is shown in Figure 9b. Figure 10 shows the belt voltage situation (Figure 10a) and belt field situation (Figure 10b) for a particular belt point, and second, the spacer field situation (Figure 10c) at A. Both lead into two areas on Figure 11 and two ways of running. In

the first one only the voltage developed on the belt is taken into account and has to be below the Paschen curve. The belt spacer capacity plays the major role. The distance between belt spacers is critical and must be as small as possible. You must have spacers on both sides, with a separation of not more than than 2 cm, for capacity reasons. We must understand why, in a restricted gap, the voltage must be our major concern. In fact, for the same voltage shown in Figure 11, it is very easy to run a belt safely along line I where the electric field is very high, but with a lot of current you can rapidly ruin a belt. Running along line II with less than half of the belt charge for the MP machine, I think that a gap of 1 cm will be safe enough and will allow the machine to run. A major problem would be the friction which consumes power in proportion to the square of the charge and for 500 μA it becomes quite significant. The electrostatic forces are 4 times the belt weight. To get the same current, but half of the friction, a good solution is to balance the charge at 250 μA on each run and, therefore, it is not critical anymore.

A very closed structure which increases the capacity is extremely convenient to carry a lot of current. I wish to point out that running right in the middle, between the spacers, is an unstable position, and with more than 260 μA, the electric field on the belt will push the belt to one side of the spacers. The best belt positioning for an MP is for the belt to enter the column smoothly at the spacer gap base and to remain everywhere closer to the lower spacers. Because of the strong electrical strain, a much better design for a spacer would be unsymmetric and of the shape shown in Figure 12. Possibly alumina rods could be placed somewhere between some of the metal spacers to take care of eventual gliding.

Another choice is to run a belt in an open structure under the field limit determined by the shape of the spacers (Figure 11, area II). In Strasbourg we have chosen to run our machine with rods of 18 mm in diameter (Figure 2) as gradient rods and spacers, along with six rollers running at floating potential as shown in Figure 13. The reason for the 2-cm gap between belt and rods is for safety; we don't know exactly the curve out at 2 cm but calculations show that we can very safely carry 350 μA on each run at 13 MV. In this case, the sum of the longitudinal field and the belt charge is always under 115 KV/cm.

I would like to take the opportunity at this SNEAP meeting to present a new belt charging system design, which I call "the decoupled-belt structure."

Figure 14 represents a belt running at 5 cm between belt and spacers. As you can see, outside the belt, the field is quite inhomogeneous and very homogeneous inside the belt. In Figure 15, the belt is concentrated inside the machine with the two belt runs equally charged of opposite polarity. Inside the belt there is a very homogeneous field which can be very high, and outside there is a very low field. If you apply Gauss's law you can see that, depending on the geometry, there is almost no electrical strain outside the belt which is almost completely decoupled from the column and does not interfere with it. This design can allow one to carry a lot of current and it acts as if the charging system itself and the accelerator column run independently, without interference or restriction. Special attention must be given to avoid failure of any power supply either for up charge or for down charge because there will immediately be a discharge onto the belt.

Discussion:

Hurley: How are your belts holding up now in terms of hours with your present modifications?

Letournel: We have holes here, 18 mm in diameter, and we put rollers in the dead section, one below the middle, here in the drive motor. Here in the alternator, we have another roller here, and another here in the terminal, and another roller here, and another, and another here. And that is the way we are building our charging system in Strasbourg. This time between the gap, between the control rods and the belt, is something like 2 cm or 1 inch.

Wegner: Are the rollers insulated or grounded?

Letournel: They are metal but insulated and running at a floating potential. These rollers have run for 1 year and we are completely satisfied. But they have not cured the problem, which is the deterioration of the belt which comes from physical processes, and it is not at all unlikely that we will have another failure.

Berners: In the last picture that you showed, was the belt carrying charge on both runs? I did not understand the reason for the reduction in the field from 105 kilovolts per cm to 5 kilovolts per cm.

Letournel: It is because you have charge on both runs.

Larson: I think, in an earlier SNEAP meeting, I mentioned some experiences that Clarence Turner had with a small single-ended machine, which I believe are related to what's just been described here. It was a very open structure so that the belt runs were relatively close to each other compared to their distance to the rest of the structure of the machine. In this accelerator, Clarence Turner had put two generating voltmeters facing the insides of the belt, and an up-and-down charge was applied to the inside of the belt and it was found very essential to balance those GVM signals. In other words, he was monitoring the voltages on the inside of the belt runs and that had to be balanced to avoid what he described rather graphically as fireworks. I think that's exactly what you are saying, if you lost one of the charging supplies or imbalanced them rather badly you would get breakdown.

Wegner: You said you had some belt damage problems with the open configuration and rollers; is the damage characterized as different from the damage you experience with the closed configuration, or is it more or less the same kind of damage?

Letournel: It is exactly the same kind of damage only we did not see the pin holes. The pin holes were coming in a half-open structure and it is the wrong way to go because the capacity goes down, so running in a closed structure with very high currents is better. If you calculate the capacity of 1 cm^2 of belt compared to the metallic environment, you see that in this case 1 cm^2 is already critical. So when you are in the half-open structure where the belt is like 1 inch from the spacer and control rods, you are already in the open structure so you have to take care of the limit. In this case it is the electric field which determines the limit. When we were in this case, which is a closed structure, we could get discharge from a spacer above and also from the spacer below and that

would go through the belt and probably is the origin of the pin holes. When we were running in the half-open structure we did not see anything else, because there was nothing coming from below.

Wegner: Do you believe that your new rods in your present configuration will now allow operation at 13 megavolts without belt damage?

Letournel: Yes, I believe so. From my calculation I can explain the theory of the fields. It is exactly the same field that is seen by the Pelletron or the Laddertron. If you take a point here, and you calculate the voltage of this point determining by Gauss's law what is the field at each point, you come to some figures and it is a very easy calculation. We do the same with odd gradient holes and here you see that it is very concentrated. We can do exactly the same thing with each configuration, and you determine exactly what is the electrical strain on each spacer, the MP spacer or this round spacer, and you can determine exactly what your limitation is. The limitation comes from a combination of both the electric field coming from the column and, in addition, the belt field. For the normal MP structure it is much too critical to run with a normal structure 13 MeV and 400-450 μamps. We ran at that and higher with our first belt. The explanation for that is when we first ran our machine, is the belt as it left the drive motor entered the column tangentially to the lower spacer which was there. The belt always lies on the space here, so the capacity of the belt was very, very high, so the voltage was very low. We were able to run at more than 13 MeV this time because we went up to 17 MeV without damage and 350 to 400 μa for awhile, but the belt was always at the base of the spacer. For some reason, I don't know exactly why, we never damaged our belt. The alternator and the drive motor were lifted so that the belt was obliged to run from the drive motor into the middle of the gap and touched the base at section 6 or 7. It would have to go through the middle and at this time it was very, very critical and it got discharged. That was probably why we destroyed our first belt. If we would reduce the gap in an MP to 1 cm, I think it would be safe. At this time there is a friction problem from the 250-μa electrical forces which are of the order of the weight of the belt itself. These forces go as the square of the electrical field and it becomes very critical to carry. It would be better for one to carry 500 μa with 250 μa on the up run and 250 μa on the down run. At this time you have only half of the friction. I would like to add something else. There is another reason why the belt has deteriorated on the MP, I think. In the drive motor there were grooves, and when you put the charge on at the roller, there is no reason for the charge to be homogeneous. The charge, then, sitting on the belt, is not very regular, mainly because of the grooves. There is a high-charge density at this location and that was also one of the reasons why we had trouble. We think it would be better to charge in front of the roller.

Haberl: There doesn't seem to be a lot of interest in the single-ended machines. I have wondered if anyone is using a single-ended to do heavy ion work on mass 50 or above? I wasn't aware that we would have problems with poor vacuum at the upper end to be honest about it. I am wondering how close the tandem experience relates to our machine concerning the need for a good vacuum in heavy ion work.

Bair: Our old CN is now being used to accelerate nickel ions into stainless-steel samples to simulate radiation neutron damage. I believe this work is going on other places too, certainly our old machine is still alive with nickel beams.

Haberl: Are they doing anything special about the vacuum or is it just a non-problem?

Bair: They are, as you can guess, continuely upgrading the vacuum.

Middleton: Hasn't an NEC tube gone into the CN in Berlin?

Norton: The NEC tube is presently in use in the CN at the Hahn-Meitner Institute in Berlin, and they are accelerating primarily argon. They have a turbopump in the terminal and a forepump which discharges into a gas bottle and in this way they keep the pressure very low. I don't know the numbers. This is part of the Vicksi program where the CN is injecting into a cyclotron and 6 MeV is the normal injection energy. They have been running smoothly without the tube problems of the kind we have seen at Munich. They have had some small problems with the in pressurization. They are running very high pressures by our standards and we have supplied them with spacer rings which go in the flange which support the weld of the titanium cup to the flange. These were shipped tc them just recently, and this is to eliminate leaks which have developed at this weld. Other than that I know of no problems with that system.

Laval University: We currently are running at 7.5 MV and our research program mainly calls for the production of 40% heavy ions. The heavy ions used are carbon, boron, and fluorine. The beam current is about 2-5 μa. I would like to hear of anyone's experience in obtaining metallic ions with the spot ring of the exit canal of the source.

Janzen: This is an RF source that you have?

Laval: Yes, in fact we succeeded in obtaining about 5 microamps of ^{27}Al using argon as the sputtering agent. I wonder what will be the life of this source in that mode, and if it will be tough on the tube.

Janzen: No other comments on that? We have tried our machine to run carbon and oxygen by just feeding carbon dioxide into the source, but I can't recall how much current we could get, but we did get ion species of carbon and oxygen. I think that is fairly common. We haven't tried sputtering the canal. I presume that you can replace your canal.

Laval: Yes, in fact, we have built our own source.

Hurley: We have 2.5-MV NEC machine at Chalk River and my understanding is that they accelerate everything up to and including iodine. It is used for ion implantation, but it's used in Chemistry and is not normally associated with nuclear physics.

Janzen: Do they use a RF source?

Hurley: They use whatever source NEC had installed in the terminal. It is a Danphysik ion source.

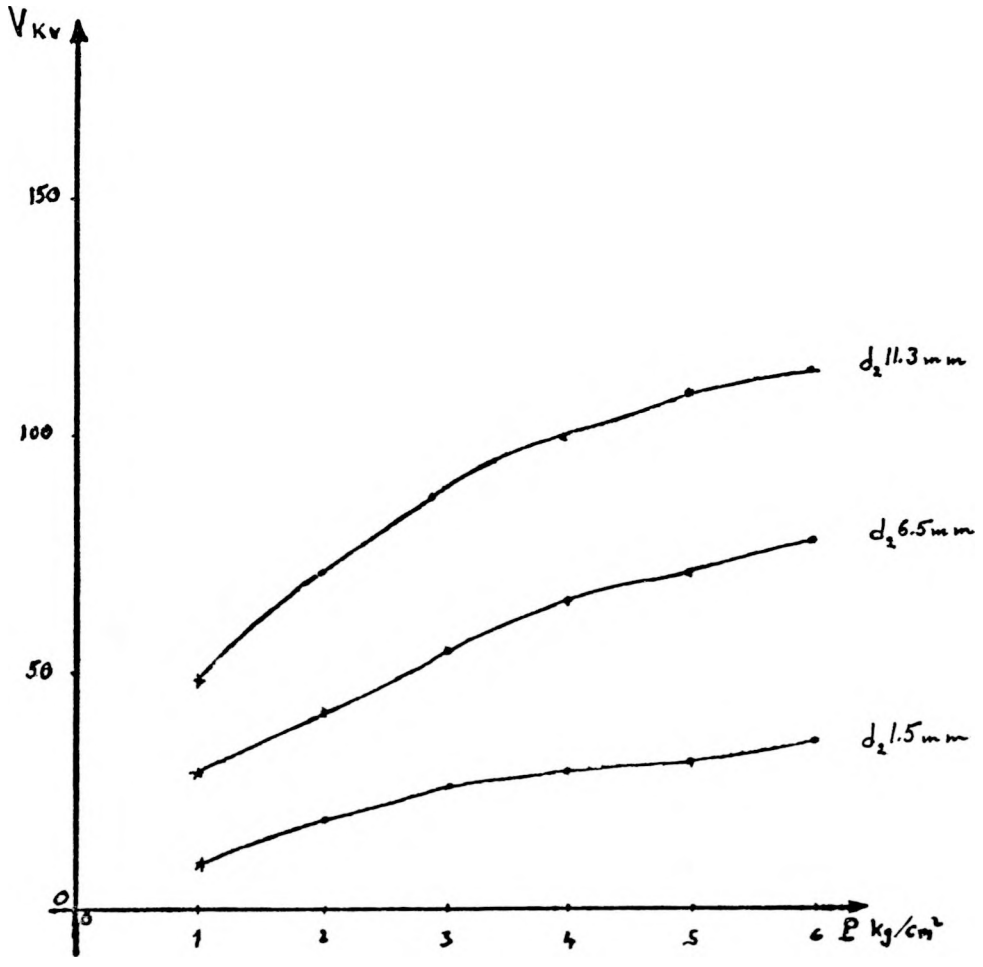

Fig. 1

Structure fermée HVEC.

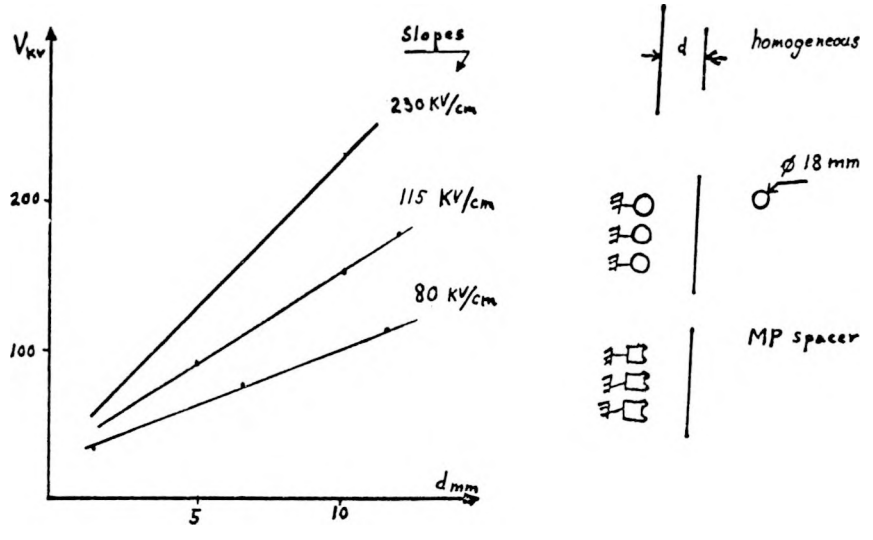

Fig. 2

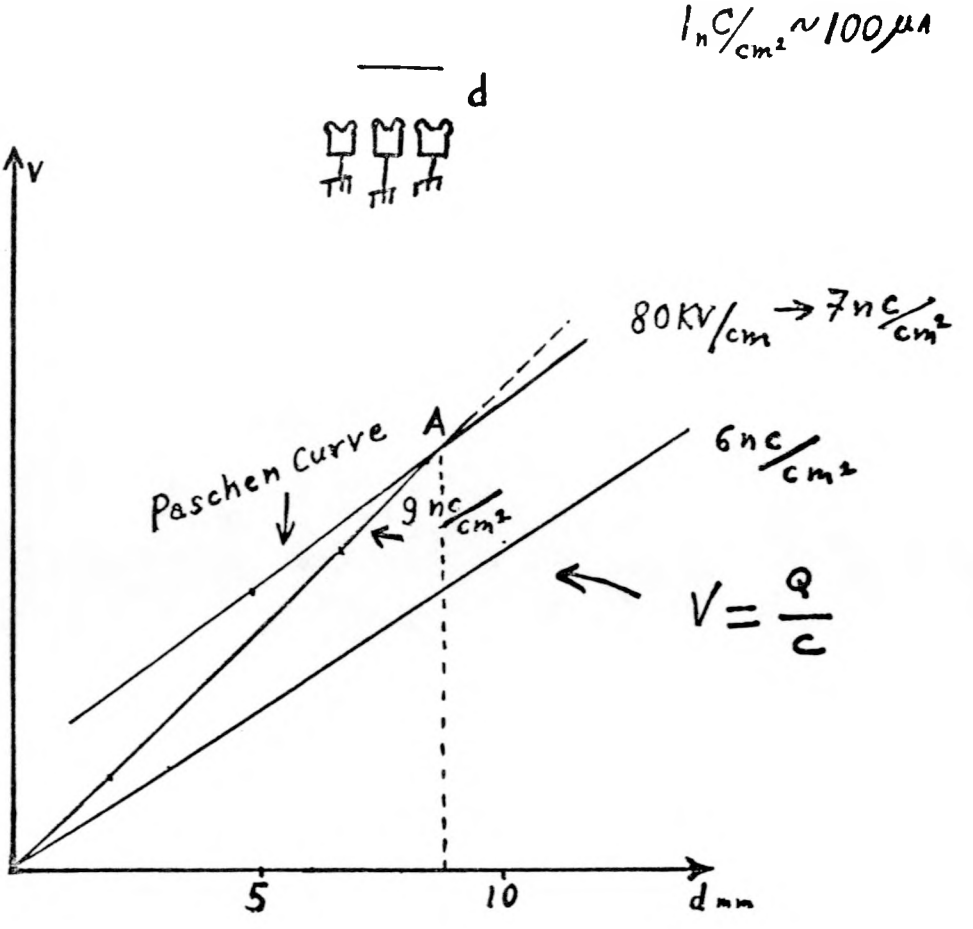

Fig. 3

$$V = \frac{\sigma}{\varepsilon_0}\,\frac{d_1 d_2}{d}$$

$$E_1 = \frac{\sigma}{\varepsilon_0}\,\frac{d_2}{d}$$

$$E_2 = \frac{\sigma}{\varepsilon_0}\,\frac{d_1}{d}$$

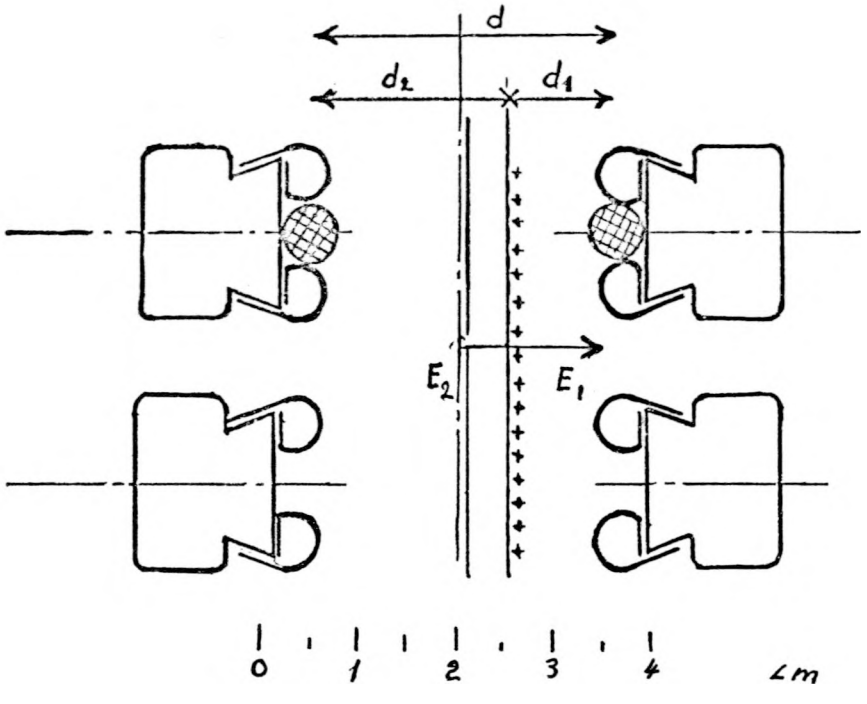

Fig. 4

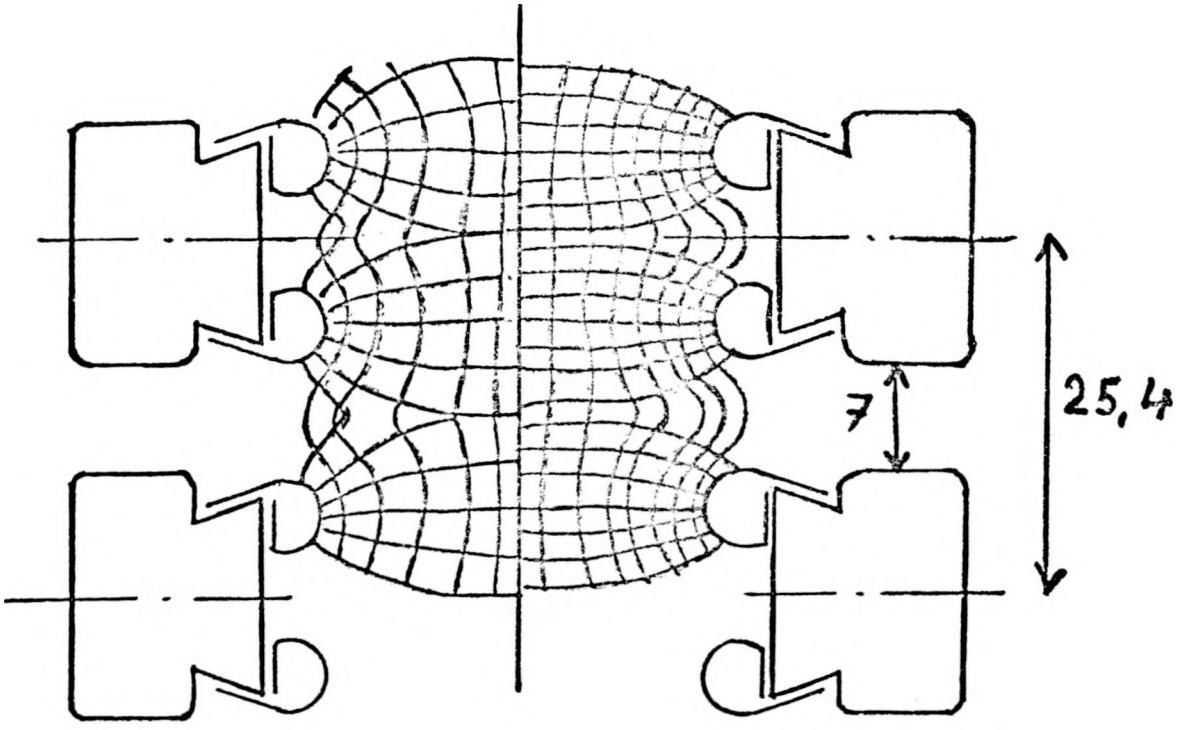

Fig. 5 $E_{sp} \sim E_{belt} \times \frac{25}{18}$

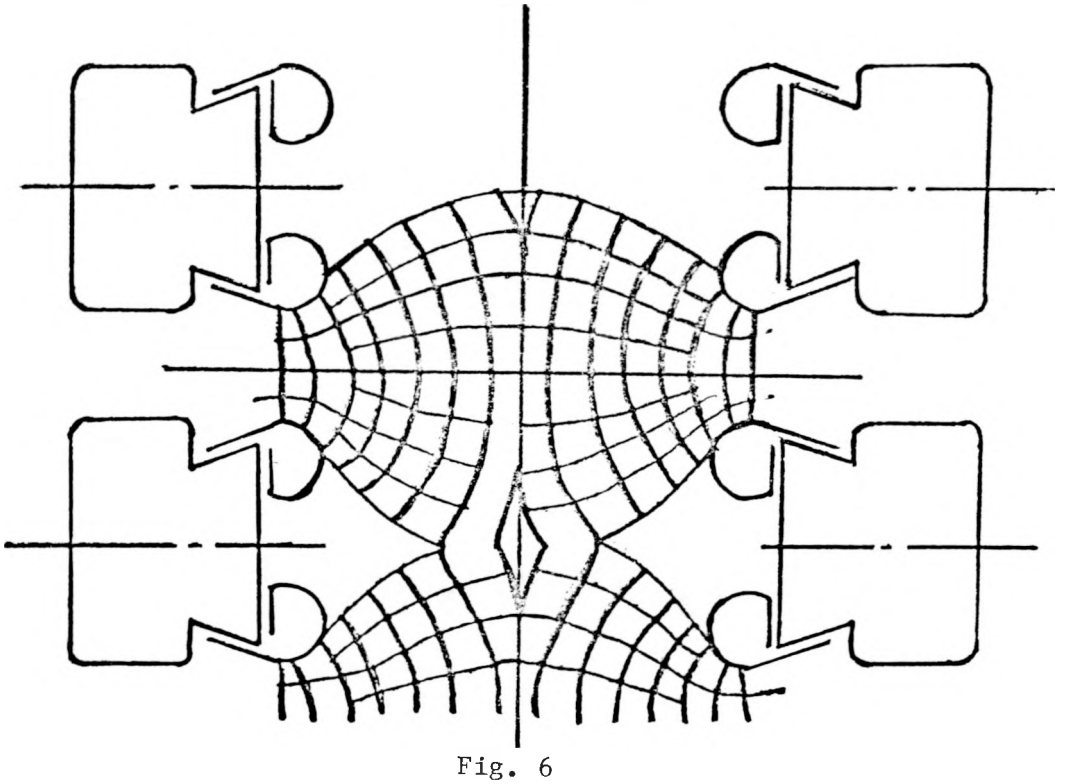

Fig. 6

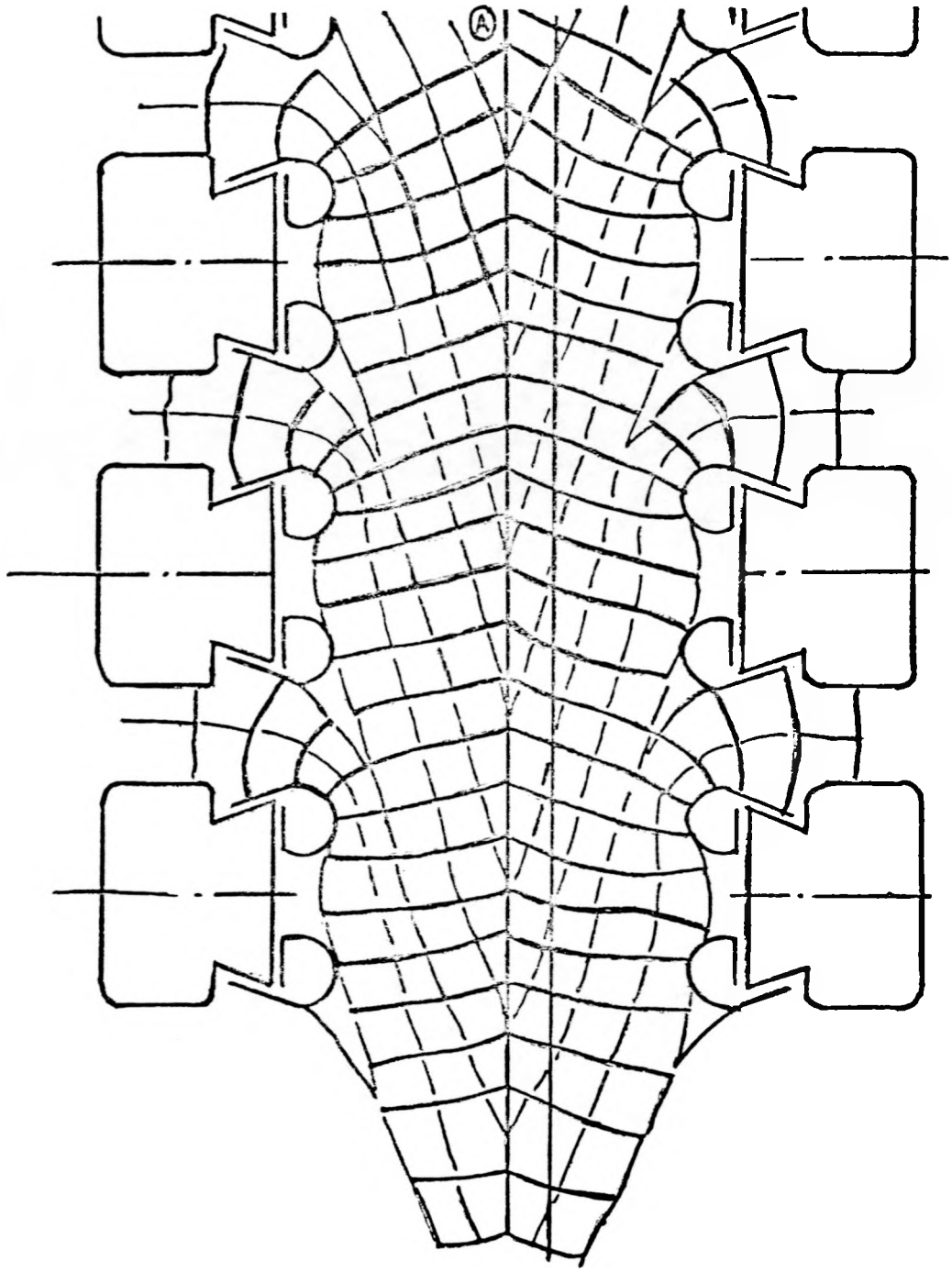

Fig. 7

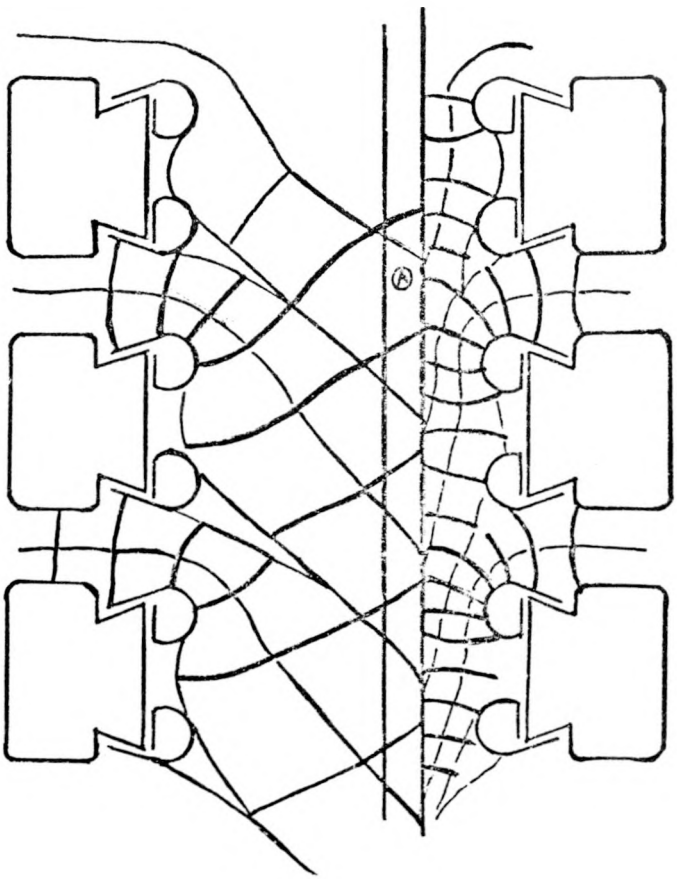

Fig. 8

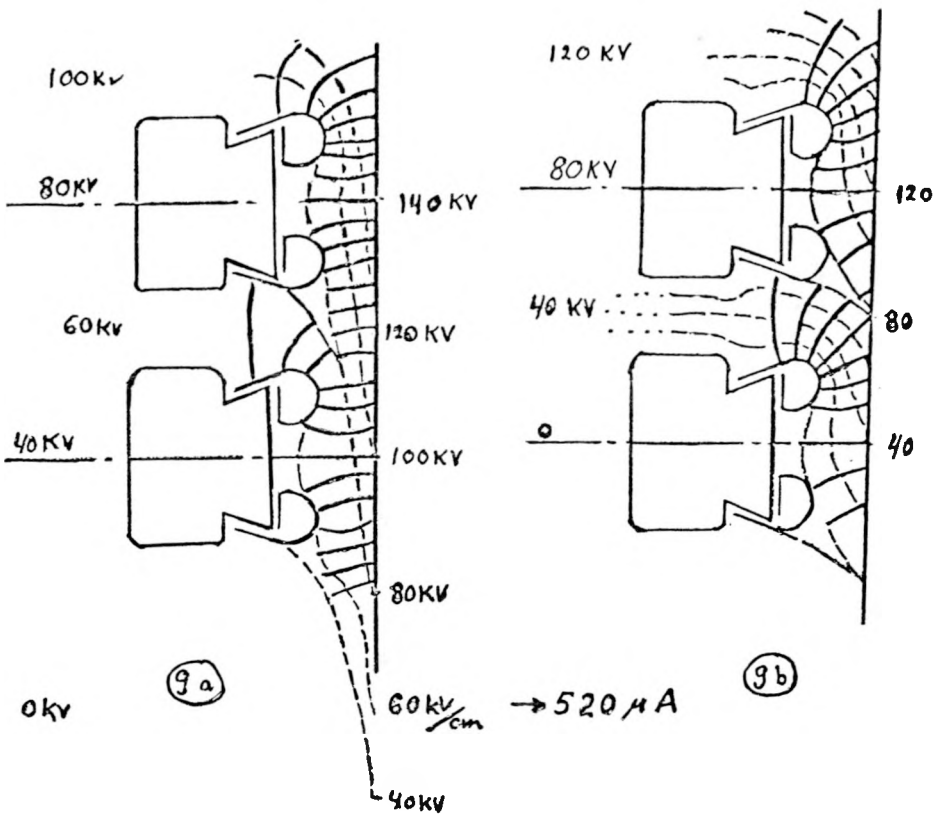
100KV
80KV
140 KV
60KV
120 KV
40KV
100KV
80KV
0KV
9a
60KV/cm
→520μA
40KV
120 KV
80KV
120
40 KV
80
0
40
9b

Fig. 9

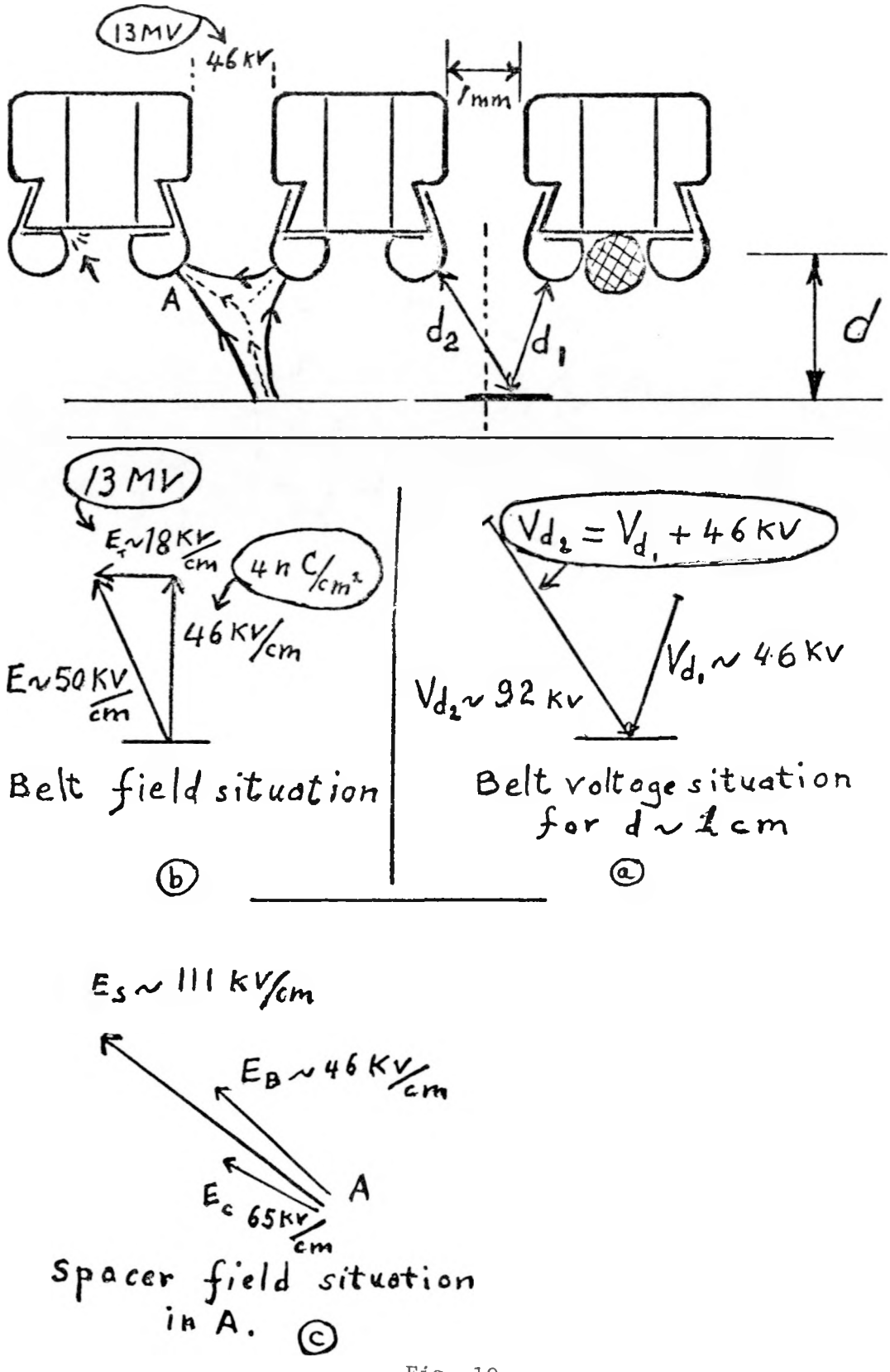
13 MV
46 KV
1 mm
A
d_2
d_1
d
13 MV
$E_r \sim 18$ KV/cm
4 n C/cm²
46 KV/cm
E ~ 50 KV/cm
Belt field situation
b
$V_{d_2} = V_{d_1} + 46$ KV
$V_{d_1} \sim 46$ KV
$V_{d_2} \sim 92$ KV
Belt voltage situation for d ~ 1 cm
a
$E_s \sim 111$ KV/cm
$E_B \sim 46$ KV/cm
A
E_c 65 KV/cm
Spacer field situation in A.
c

Fig. 10

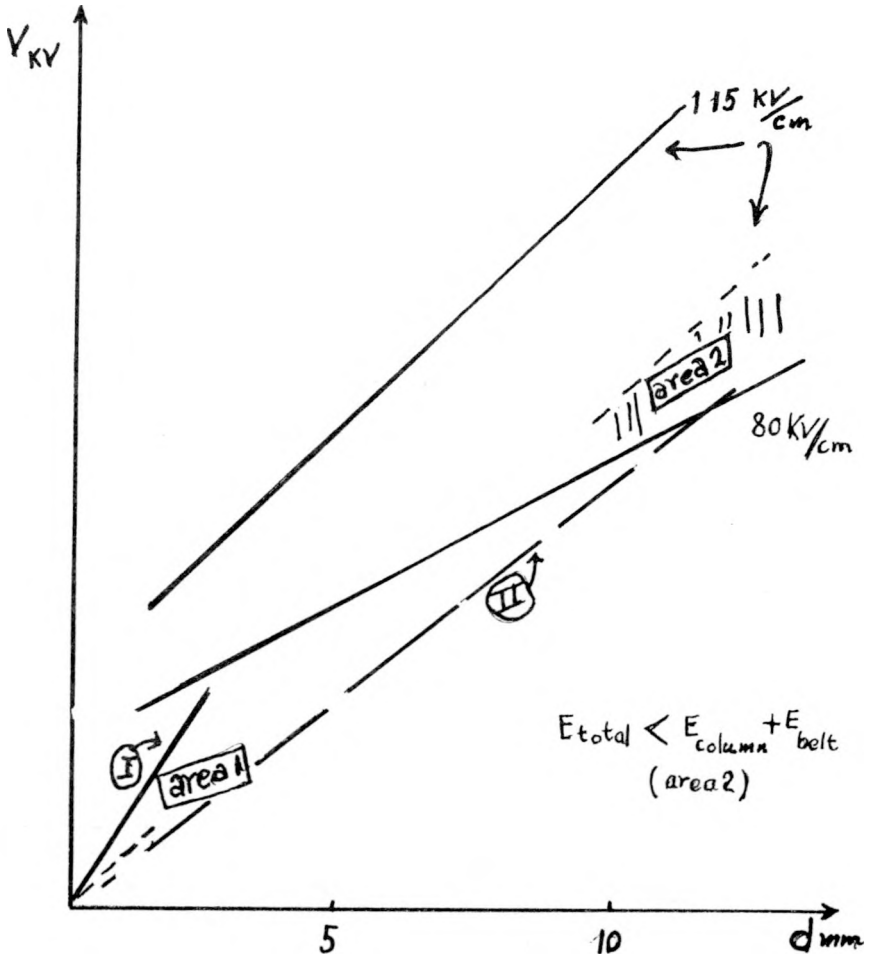
V KV
115 KV/cm
area 2
80 KV/cm
II
I
area 1
E total < E column + E belt
(area 2)
5
10
d mm

Fig. 11

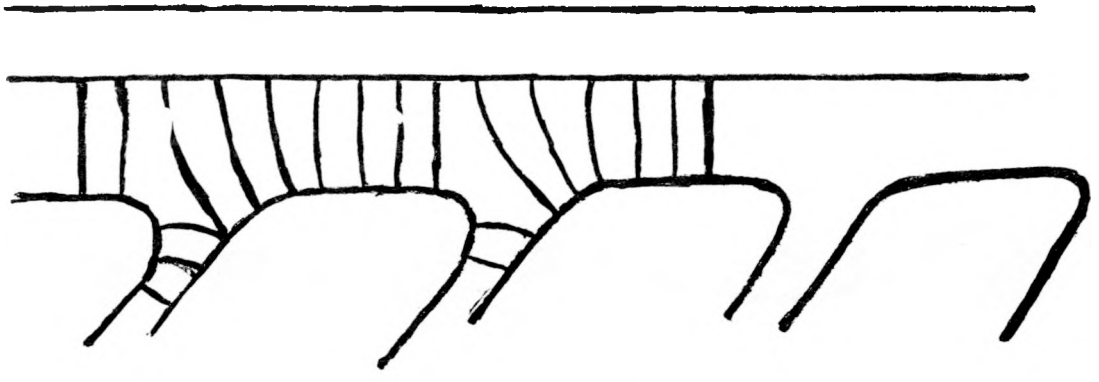

Fig. 12

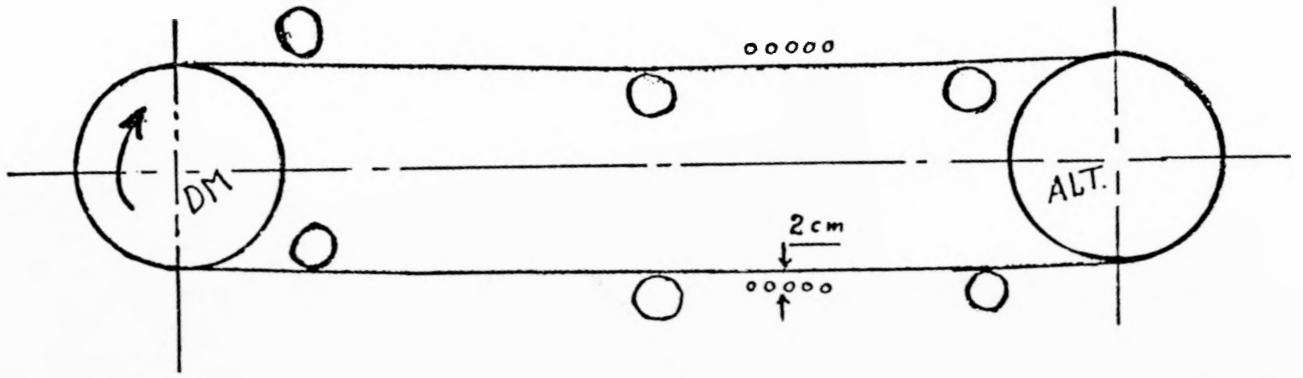

Fig. 13a

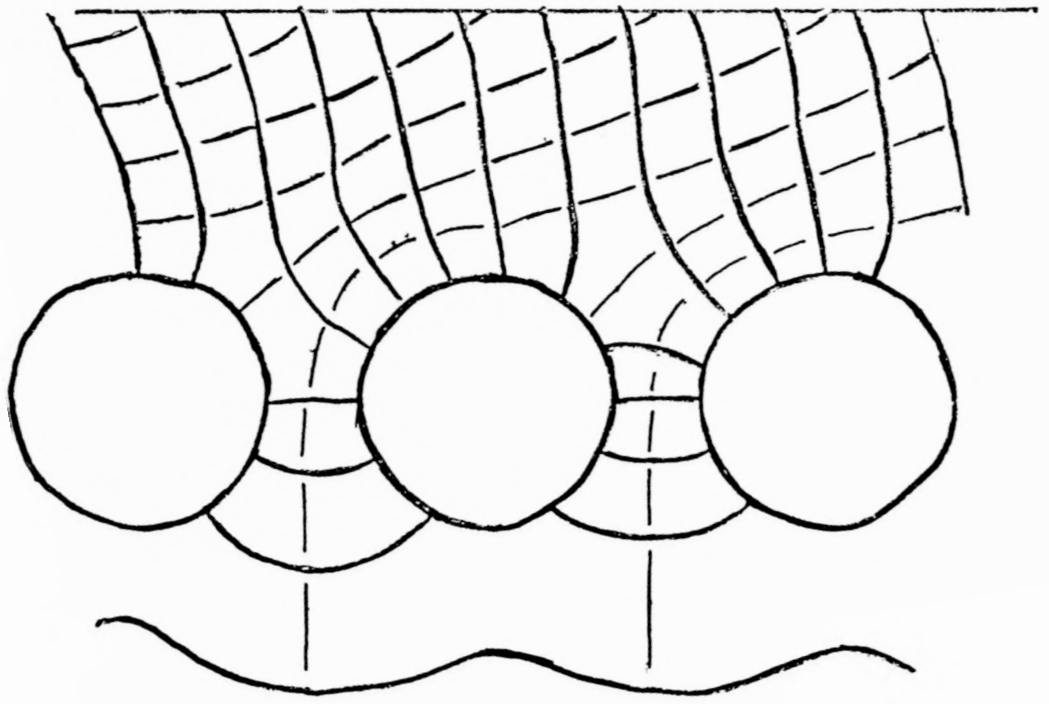

Fig. 13b

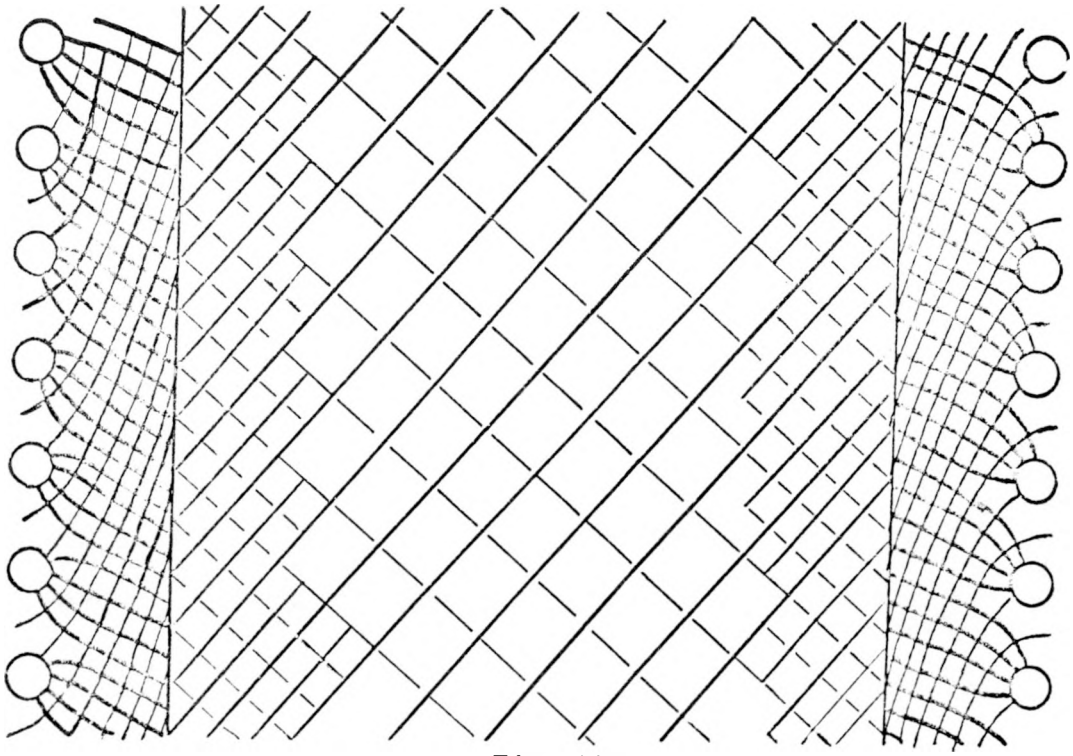

Fig. 14

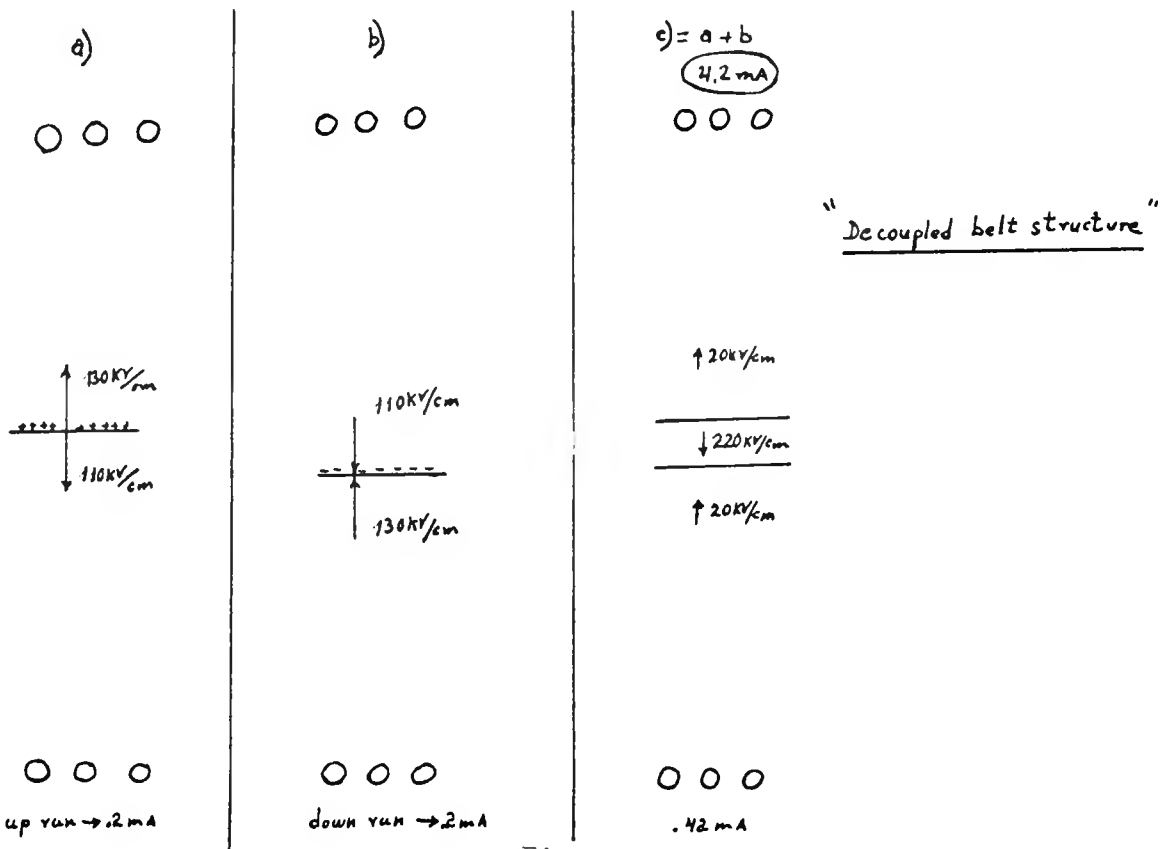
a)
b)
c) = a + b
4.2 mA
"Decoupled belt structure"
130 KV/cm
110 KV/cm
110 KV/cm
130 KV/cm
↑ 20 KV/cm
↓ 220 KV/cm
↑ 20 KV/cm
up run → .2 mA
down run → .2 mA
.42 mA

Fig. 15

PROBLEMS WITH OUR UPGRADED 3-MV MACHINE

Henry Janzen
Queens University

Well, if there are no further discussions, I might burden you a bit with some of the troubles we have had at Queens with our upgraded three. This past summer certainly has been a bad summer for us with all sorts of troubles developing in our machine. I am not sure if we understand why this machine, which for many years had been relatively well behaved, has suddenly become very badly behaved. I might describe some of the things we have found. The machine is now running again and I have my fingers and legs crossed hoping it will continue. What started things off was that we changed the pole pieces on our analyzing magnet. This was simply to get a somewhat better NMR signal, but this meant that we had to align the analyzing chamber and everything again. In order to do that, I opened the tube to atmosphere so that we could sight back through the magnet to the ion source and tube-or at least to the focus electrode in the tube. The tube was open for this for about a total of three days. Whether this is significant or not. during that time, which was in March, we had a terrific rain storm in the Kingston area. It was so bad that we had water seeping in to our machine room. When we came in one morning we found water lying on the floor while the tube was still open, and clearly, the humidity in the room must have been very high. Whether this is significant or not remains to be seen. After the alignment, we closed the tube and pumped it out. We got down to our basic vacuum, which in this machine is about 1.6×10^{-6} torr, and proceeded to run the machine to test and condition. We found at about 2.5 MV on the terminal we got a terrific amount of radiation from this tube, and this was without beam. The tube was clearly emitting copious electrons which were going up to the terminal, and they must have been coming from the electrodes in the tube because there was nothing down in the beam line that we could identify as being a source of electrons. The tube simply lost its condition, and we couldn't get it over 2.5 MV because the radiation was horrendous. We tried to recover the condition of the tube by running continuously with beam and without beam for a total of 72 hours. We did get up to 3.5 MV at one stage, but at no time did we ever really see any improvement in the radiation. When we went back down to some pre-selected voltage and measured the radiation level at that voltage, it always was about the same. So clearly the tube did not clean up. One other factor which may have a bearing on this, which we didn't discover until later, was that when a piece of equipment had been installed in one of the beam lines a year before, an insulating coupling made out of nylon had been designed by the person installing the equipment. When he put it in, he had not shielded this piece of nylon from possible impingement by the beam, and this was in the analyzed line downstream of the analyzing magnet. People had complained to me several times that when they analyzed the beam they got a burst of gas showing up in the vacuum system. We didn't quite understand why this should be, but when we dismantled this line for other reasons we found out why the beam had been hitting the nylon, and it was completely burned up wherever the beam had swung across it in the analyzing process. This nylon vaporized inside the tube, although it was some distance away from the machine. I presume that it could have had vapor from the nylon (when the beam struck it) migrating back into

the tube and possibly depositing on the electrodes. We opened the tube again, having decided pretty well that we had to do something drastic, such as get it rebuilt. After opening, before we did anything else, we examined it very carefully and there were no tracks on the glass that we could see. The electrodes of the tube when you looked in the end of it, did have interference colors and really was quite pretty. But we had seen these before, when I had the tube open years before this, and it really didn't look any different on this occasion than it did then. That was before the episode with the nylon insulator, so maybe the colors themselves are not significant, but that the film on the electrodes is important. This came possibly from the nylon, which then got activated in some way by the high humidity that also happened, fortuitously perhaps, while the tube was open, and these combined to somehow produce on the electrodes a really good electron emitter which was sufficient to completely decondition the tube. To make a long story short, we did have the tube rebuilt by High Voltage. They reported that they found very little glass damage, and replaced something like three of the glass insulators. They repolished all the electrodes, which are stainless steel in the upgraded tube for this machine. We got it back on April 13, put it back in the machine, and ran the machine on tests after this and the tube certainly looked all right. We had no problem. We didn't have the massive radiation. We did see some kicks on the capacity pick up trace on the oscilliscope which we called conditioning kicks. But, in general, the tube was good. At no time during this conditioning process could we get the tube to emit x rays or to radiate. Now when we got the tube initially after the upgrading, during the conditioning process we got quite a bit of x-ray radiation from the tube while it was conditioning with no beam. This time we just didn't get any x rays from it at all. In other words, we could go right up to four million volts on the terminal and saw no appreciable radiation. Occasionally there would be a short burst of it, sufficient to trip a monitor, but never a steady x-ray output at all. Also we had tank sparks, as usual, but what we also began to hear was more of a ring. It sounded like somebody was on a picket fence with a stick going along the potential rings. These sparks persisted and we really didn't know what they were, but they got gradually better and we continued to run the machine. However, this was only a temporary situation. When we got these funny sounding sparks we also could see arcing or sparks occurring in the base plate of the machine. These must have been due to very fast transients. I traced down with the lights out one rather surprising spark seemed to happen very consistently when we got the odd type of ring spark. It was coming from the end of a shielded wire, from the shield itself, to ground. The shield was itself grounded about six inches away. This wire was possibly picking up as an antenna. There was sufficient energy to give a noticeable spark from the shield to ground, and I would assume that this could also spark from the shield through the insulation to the wire inside of it. We had found, particularly in the terminal of the machine in the past, this very sort of thing happening. I think this only proves that we had very fast transients on sparks, particularly on these peculiar sparks which were not the standard tank spark at all. One of the things that we thought might have caused the problem with this sparking was that when the tube had been rebuilt, and actually when we had originally obtained the stainless tube, we found that the electrodes of the tube itself did not register very well with the potential rings on the column. In other words, the original aluminum, electrode tubes were thicker than the stainless steel and I assume that High Voltage, when they made the stainless steel tubes, used the same glass insulator so that you got a cumulative dimensional effect and the tube was a little bit shorter. This became larger and larger as you went up the column, so near the terminal the

tube electrodes were oftentimes closer to the potential plane, one down from the one to which they were connected. We thought that this reduced gap might be causing some of our problems. We decided a way to fix this was simply to move the tube out a little bit further. This might produce somewhat poor registration at the bottom end of the tube but at least would fix it up at the top end where things seem to be much more critical. We did that and the machine ran all right but we got very unstable beam. We finally traced this down to the fact that some of the lugs on the focus electrode, which are larger than the standard lugs, have holes in them through which pins go to protect the second electrode down. The focus electrode has its own spark gaps. The lugs on this, because we had moved the tube out a little bit, came close to the terminal spinning and we were getting some corona discharge there. To cure this we simply cut those lugs off, since there were lots of others left and it doesn't seem to hurt the tube. We continued to have these odd ringing type sparks which moving the tube out did not cure. These got progressively worse after a month or so of running. The next thing I thought I might try to improve the situation was to put more SF_6 into the machine. I put in a whole bottle which I calculated to put our SF_6 percentage from about 16% up to 20% in the tank, but this did not improve the situation and, in fact, oddly enough, it made it worse. Previously we could at least run the machine with occasional sparks of this type up to 4 MV. After I had put in a new charge of SF_6 we could get up to only about 3.6 MV and then this was sort of a threshold. These ringing sparks would set in and it would go into an almost complete discharge of this type. These ringing sparks would simply persist one after the other and there was nothing you could do except turn the terminal voltage down until they stopped and it would stop when you got down to a terminal voltage of about 2.4 MV. This deterioration set in at some sort of a trigger level and then it was very bad. The discharge persisted until you took the voltage right down to 2.4 and then you could not go up from that either. If you tried to go up from that you went into this discharge state. If one shut the machine down and waited for some hours and then turned it back on, you did not recover the 3.5 MV; you were limited to the 2.4 MV. No amount of conditioning seemed to improve this. I tried to find out where this discharge was occurring. We felt that it was somewhere in the column. We have a shorting rod in our machine and I tried pushing the shorting rod into various places on the column to see if I could isolate where this might be but I didn't succeed with that. Nothing consistent came out of it; we got that type of spark almost anywhere with the shorting rod. Finally, we decided that we would have to open up the machine. Oh yes, the other thing that I thought I would try, since I couldn't get up to high gradients, was running it at lower gas pressure. Our normal tank pressure is somewhere around 260 psi with a mixture of nitrogen and 20% SF_6. I took the pressure in the tank down to about 160 psi. This did not cure this type of spark but on the other hand we could now run to four million volts and not get this drop-off of voltage which we had noticed before. So at the lower tank gas pressure the performance was really better than the higher pressure but the sparks were still there. The machine was really not usable in that condition at all. So eventually we faced the problem and decided that we had to dismantle the machine completely to try and find out what this was. I might mention one thing that happened while we were at the low gas pressure. I was running this one Saturday morning when one spark really threw the monkey wrench in the whole works and tripped out a circuit breaker. The whole machine shut down from this spark and I could see, out of the corner of my eye a flash of an arc somewhere in the control panel. Needless to say, I jumped about two feet off the chair when this happened. The machine was completely shut down. The power in our

area was down and I had to trace back to find where the trip out had occurred, and it had actually tripped the breaker just downstream of our transformer in the substation. This was a real discharge. This puzzled me as to how that could happen but in retrospect, the spark must have jumped to one of the drive-motor lead-in wires and instigated an arc somewhere between conductors in the 3-phase power line, which then shorted one phase of the line and the whole thing dropped out. It was only when we got the machine back in operation and we tried to energize our quadrupole circuits that we found an interlock relay completely burned up in the control panel where I had seen the arc. So we decided that we would recover the machine and we would have to do something drastic so we stripped it down completely. We found a strip of belt of about 1/8-inch wide, maybe 18 inches long, which had been torn off and it looked as though the belt had scraped on something. Yet when we looked for something, anything that the belt might have scraped on there just wasn't anything. So again I must conclude that this was somehow electrical damage but it certainly looked as though it had been stripped off with a sharp knife. We found nothing very significant on the belt or anywhere else. The next thing was to examine the column glasses. There was one spot on the tube glass where there appeared to be an arc of some sort and it looked rather peculiar. It looked as though a spark had hit the glass of the tube. Now how this could occur I am not sure, but that's what it looked like. This was the only thing that we could see on the tube that looked at all suspicious. It certainly had not been there on the previous opening of the machine. I might mention that we were opening the machine every three or four days during this time trying to find what had been going on. We had not seen this previously so it had not been there all the time. It was not the initial cause of the problem. On the K3 machines there are two sets of glasses, an upper set, and a lower set, and we found about five glasses on the upper set that showed very definite erosion tracks in the glass, some on the sides, some on the top, and some on the bottom. In the bottom glasses we found about four more with this type of track. To repair these, the only thing we could think of doing was to erode them out somehow and we did get a commercial air abrasive unit which is a development from the dental air abrasive unit used for drilling teeth and it simply shoots a fine stream of abrasive particles which are at high speed. It can be used to put holes in glass or frost glass or whatever we like. The nozzle that came with the machine wouldn't fit between the belt guides on the column, but by placing a small piece of plastic tube between the handle and the nozzle itself, we could sneak this in. By manipulating this around we were able to sandblast out these tracks. I looked very carefully at all the glasses to make sure that we got them all, and I guess we did, because after this process we put the machine together and it is now running again reasonably well. I didn't mention it but we went through all sorts of tests to try to determine what was wrong. We thought wet gas was a problem, we changed desiccant in the dryers and everything you could think of, but none of this ever did any good. The problem, it now appears, was really the fact that the glasses in the column had tracked. What I would like to know is why? I don't know why these glasses break down since they are protected by spark gaps which are the same as they had been for years and we had been running with a perfectly well-behaved machine for years and all of a sudden this happens.

Discussion:

Woods: This is what I consider to be the third definite piece of information along this line. Heidelberg and Los Alamos have both suffered damage to columns and we have both traced them to one thing--that is a mixture of nitrogen and SF_6.

Neither one of us has any explanation of this but we both have traced the time during which the column damage occurred with a time when we had a gas mixture. We went to pure SF_6 and neither one of us has ever suffered anymore or any change in the damage to the glass. This is the third very definite piece of information along this line. I have no explanation.

Goldie: Dick may be right about that. One other thing that has happened to columns in the situation such as you described is that a machine running for a long time accumulates a certain amount of belt dust on the insulators and then is exposed to a very humid atmosphere. We know that belt dust is hydroscopic in some way and that you can't get rid of the problems it produces unless you clean it off by using strips of cloth and toweling action. I would guess that there is a possibility that this is what caused that problem. Can I ask a question? Did you put the same belt back in?

Janzen: No, we didn't change the belt at the time that we had the onset of the trouble. I should mention that we had also cleaned the column when we had the tube out. Yes, I took the belt out at the time when the tube was out and we cleaned the column.

Goldie: Did you clean it by this toweling action?

Janzen: We brushed it, yes.

Goldie: It's rather hard to get a brush in all the surfaces, particularly on the inside.

Janzen: Yes, I agree. We had a whole selection of brushes and strips, too. Someone suggested that a Scotch Brite could be used, but this is difficult to manipulate in there.

Goldie: I guess I would also say, in connection with what Dick brought up that your machine had been running in a mixture of SF_6 quite successfully for a long time before that and voltage probably even in access of 3.75 MV.

Janzen: Yes, that's quite right.

Goldie: One more thing, certainly when you put that machine back together, you did change the screens. Did you change the screens to try to stop this sparking, which I agree sounds just like column sparking from what you say?

Janzen: We had tried everything we could think of. We changed everything.

Goldie: The reason that I bring that up is that we have found that charging in SF_6 is less likely to be uniform. It is more demanding of the charging screens than in nitrogen and CO_2. We have had problems in the past of belt sparking with charge very poorly distributed across the belt width.

Janzen: Certainly one of the things we did was to again change the belt, thinking that this may have been the problem and it didn't make any difference.

Goldie: Those marks on the insulators, were they actually eroded?

Janzen: Yes, if you took a sharp point you could feel the depression made by these arcs and they weren't easy to see just by looking. The only way I could find them was by actually rigging up a light bulb small enough to go in between the column planes on a rod and I was able to push this inside the column and look at the glasses from top and bottom with a light behind them. This is the way I picked them up. I might mention that during this time I did have a discussion with various people at High Voltage about possible reasons for what was going on. At one stage, after a particularly bad session of this spark, I guess when I had run it at reduced pressure in the tank, when we opened the machine we found that there seemed to be a deposit all over the machine. It was sort of a whitish-looking deposit and we never determined what that was. I might mention also that the other things we saw when we took the tank off was debris in the tank which was this gray epoxy glue used to glue the column together. On several occasions when we opened the tank we found little bits of this lying in the tank and this we had never seen before. We had seen all kinds of other debris in the tank, such as springs, but never this stuff. I am sure it was column glue. Once I found these discharge paths then you could see what happened during the arcing along these tracks. The glue bead, where these tracks ended, had simply blasted off and fallen down into the tank.

Chapman: I would like to make just one comment on Dick's remark concerning the N_2-SF_6 mixture. I think there is fairly definite evidence now from a number of sources backed up by some direct experimental evidence, that in a discharge in pure sulfurhex, much more of the discharge energy goes into the gas and much less into the hardware. When you first start to run a mixture, or if you run a mixture for some time, there is the possibility that you can run higher voltage than you could with pure nitrogen-CO_2. But if you have discharges then you very likely can do damage to your column structure. We did quite a bit of damage to our column structure in the earlier days, which appears to have been caused by running at marginal gas pressure. In this instance, you very frequently will run a column spark rather than a tank spark. With this mixture you will do considerable damage to the glass structure itself. The other comment I would make is that nylon is a very poor vacuum material. It has a very high affinity for water. If, in fact, it did spread by some mechanism over at least part of your machine, it would be very difficult to get rid of if it had the opportunity to take up this quantity of water.

Janzen: Incidently, this nylon that I talked about was in the vacuum system, not in the tank.

Chapman: I believe on your system you use turbo-pumps, do you not? I am far from convinced that turbo-pumps are clean and I would suggest that there is a possibility that you have oil-vapor problems as well.

Janzen: We have run our turbo-pumps on this tube for five years and I have never seen this sort of thing before.

Moak: I think that we have to look here for something that changed at some point in the history of your machine. In Oak Ridge, when we first started our EN machine, it was nitrogen and CO_2. We very quickly began to add sulfurhex to the mix, but nobody even thought that the spark gaps on these machines need to be changed when sulfurhex is added to the mix. Spark gaps are critical things. If a spark gap is basically designed so that it won't let go before the glass lets go, then it's a sort of sometime thing. Sometimes the glass will let

go, sometimes the spark gap will let go, and you never know where you stand. On the other hand, if the spark gap is set too close, the Daresbury experiment clearly demonstrates that the probability of breakdown just goes up, you can't work, but on the other hand a spark gap that's too wide won't work. The way to demonstrate this is to increase the sulfurhex, and if your problem gets worse, you know your spark gaps are not protecting something that really needs protection now. This is maybe a new thing which has suddenly started needing protection and it hasn't got it. Now you increase the sulfurhex pressure and the spark gap is even less of a protection and you get in more trouble rather than less. I suspect that something in the column or in the tube got a bit weaker and then the spark gap was no longer of any service to protect it.

Sato: Dick mentioned that Heidelburg and Los Alamos have column-damage experience, and we have the problem, too, and we are also using a mixture sometimes. In Session VI, I will present a talk in which I will try to give you the inside look at what is physically happening. I am simply saying that in your case, you are using a nitrogen-SF_6 mixture and floating out of the SF_6 finally occurs in a critical amount. One type of strange discharge could happen when you are pushing up to extremes. At one spot we could make it up very accurately to within 50 kilovolts before we were limited by this. You cannot recover to any voltage in our case if that happens over 12.3MV. A second voltage we recover to after that phenomenon is 8 or 9 MV. We never ever exceed this even though there is a 30-minute curve of quiet down. When we opened the tank in many, many cases we found nothing except tiny washers or screws on the tank floor. Such small items never have been connected with the continuous spark around 7 or 8 MV. This has forced us to study the phenomenon going on in this kind of mixture. Finally, we reached a conclusion and pinpointed it on the amount of fluorine. In our case, if the gas contained H_2O in a more-than-normal quantity, it very quickly made an etching agent. HF is etching the glass and weakening it so a discharge can cause damage.

Janzen: Do you know what the percentage of fluorine was that you had?

Sato: Tomorrow I will give you that information.

Janzen: The reason I ask is that one of the things that we tried to get done was an analysis of the gas to see if we could see any products in the gas that we suspected could have caused this damage. But certainly within the limits of that instrument we didn't see anything.

Wegner: The Munich machine ran on a 30% mix until they switched to pure and suffered no column damage of any kind on their machine. The Brookhaven machines have always been running on one kind of mix or another and are at 50%, roughly, now, and have never suffered any column damage with the mixture. I believe that the Rochester machine does not have any column damage and is working on a mixture, so I don't think the correlation is all that perfect because of our exceptions to the rule. I might add that in terms of the sequence of events you describe, it might be possible that when you were working in the discharge mode, where you had the high radiation intensity in the vicinity of the terminal, the gas could have gotten ionized enough near the terminal area to allow breakdowns to occur along the column just because of the high ionization level in that area. You could have initiated the damage at that time, and you wouldn't have known it because you couldn't hold voltage properly and you really discovered it later when you got up in voltage after you got the tube fixed.

Janzen: We certainly had massive radiation.

Wegner: That softens up the insulation to where you can actually discharge in it.

McKeown: Coming back to your nylon problem. We had a similar problem with the cesium sputter source. We were doing some focusing on the return of the cesium beam and insulated the source from the source box. The beam hit the nylon insulator, and I must have evaporated something around, because the source wouldn't hold the extraction voltage anymore. We took it apart and we kept blaming each other because one wouldn't clean it properly, or things like that. Finally, we just ran it and ran it and that seemed to clean it up. It is sort of similar to your ion problem.

Janzen: We ran for many hours to get some improvement. If it had even improved a little bit we would have been encouraged to go on. It didn't improve at all in 72 hours of straight running. Initially, when we got the tube, we conditioned it in something of the order of 100 hours of running. We could see improvement occurring and this time nothing changed.

Levesque: Until the time that you said something about finding tracks on a glass, your problems paralleled our installation shake-down problems with our 3.75-MV machine. I might suggest that you might have had a collection of problems. We found the same kind of strange noise in the tank sparks, some of these sparks going all the way back to the console. We solved the problem by resealing the feed throughs on the base plate. The over all problem of not being able to hold voltage, and conditioning the tube was eventually solved. We suspected the gas and we had the gas analyzed by an independent firm. We were running 100% of brand new SF_6 gas. It exceeded the specifications of the lab that was doing the analysis. The problem just went away with time. Really, I think the problems went away because we opened and closed the machine and changed so many things in a short period of time. Eventually, we stepped on some of the smaller problems and things just went away.

Larson: I would like to make a comment about your tubes, but first a couple of questions. If I understand correctly these are straight tubes, is that correct? (Yes) Did you see any vacuum phenomenon going on?

Janzen: No, we never saw any vacuum tube sparks that we could identify as such, no great vacuum surges or anything. The vacuum was quite steady.

Larson: I will anticipate, a little bit, things that I want to talk about on Wednesday. I think that for any acceleration tube of any design we are somewhat at the mercy of the cleanliness of the tube. That is, what's in there besides what we think are the basic materials of construction. On a straight tube you are most sensitive to this. I believe that on the various suppressed tubes, either the inclined-type tube or the NEC-type of axially modulated field, there is some suppression of the effects that go on, but in all cases, we are at the mercy of the cleanliness of the tube. If the tube gets contaminated with some unusual material, and I am quite sure that is what happened in Munich for example, very mysterious things will happen and they will be very hard to correlate with what you have done with the tube, be it new tubes, old tubes, or other things, because you may not know what this sort of microscopic contamination is.

Middleton: Can I make a guess what it might be, what about sodium chloride? What time of year was it when you had your flood?

Janzen: It was in March.

Middleton: Couldn't that water have brought in a lot of salt from the road? Did you have a lot of salt in the basement?

Janzen: I don't think the water that got into the basement came from the road, but I don't know where it came from. It certainly came through the wall.

Billquist: Just one more statement about the exception proving the rule--our FN has always run with 100% SF_6 and I dare say we have as much column damage as anybody around.

Session V, Chairman, T. Lund

PRINCIPLES OF PULSING FOR TANDEM ACCELERATORS

R. Liebert
General Ionex

What I am going to do today is survey the traditional form of pulsing systems for tandem accelerators. These have become traditional, now, but are not that old. Then I will try to give some indication of how the principles involved in designing these pulsing systems have to be looked at, in light of the needs for heavy ion acceleration, which many of us are headed for in the future. I am going to, mainly, concern myself with klystron bunching today.

In the traditional system, one has a method of producing beam bursts which have lengths that can be finally bunched to time intervals which are usable by the experimenters, namely, to a nanosecond or less. Experimenters would like to have time intervals between pulses of the order of 400 ns or more. For some applications, people like to have very long time intervals, but usually they still like to have very narrow pulse widths available. The problem in chopping is that you have two pulses which go through an aperture in one cycle of a chopper. The traditional system would eliminate the second pulse because of the inevitable asymmetries which occur in the system because the pulses will have different shapes and they will not go through the buncher the same way. Finally, the system, which attempts to get the state of the art, would follow it with a post-acceleration sweeping after bunching. Bunching would take the rather wide time pulse that you get through an aperture after the beam is swept over the aperture and velocity modulate the beam to a pulse width of the order of one nanosecond. Usually, in the process of doing that, you are left with a lot of material which comes through in the tails, because material, coming through the buncher in the wrong phase, is not getting bunched properly, but will still squeeze through. You get beam, which we call dark beam or dark current which you would like to eliminate, and you would also like to clean up the tails of the pulsed beam. One thing you don't want the pulse acceleration sweep to do is to throw away useful beam. Many people have, in fact, used them in this way by literally chopping the bunched beam and cutting out the better section of it in the middle, but they throw away intensity in the process.

Let's just take a look at what one of these systems looks like. Figure 1 is the system which we are delivering to the NEC accelerator in Japan for the Gerry installation. The chopper you see is located in front of a pair of defining slits. In the traditional system, shown in Figure 2, which is designed for light ions, it would be followed by a pair of plates, usually orthogonal, which would sweep the beam in a Lissajous pattern of some sort, or an ellipse, and only accept the beam one time per cycle through the aperture. The second pair of plates would be dc, biased continuously, so no beam would get through until a square wave was applied to them. They would be tied to the plate of a tube such that, when the voltage collapsed on the tube, you would suddenly have zero field

on the plates, and the chopped beam would just go right through the aperture. This way you avoid adding extra energy spread into the beam. Other techniques that have been used applied the dc bias directly to the chopper plates themselves. The buncher system (Figs. 1 and 2) is made up of a series of tubes whose length is matched so that the gaps on each end tend to bunch the beam. If you look at the leading edge of the pulse the buncher gaps tend to slow down the leading ions and they tend to speed up the ions in the trailing edge. The buncher produces a bunched beam at some designed-for distance, far away from the buncher. The post-acceleration chopper, shown in Fig. 2, is usually pretty far down the beam line, fairly close to where the target is located.

We are going to look at some of the detailed constraints for a pair of chopper plates. You have a variety of variables that you are considering, first of which is the frequency that you have to run them at, and second is the wave form that you put on them. Traditionally, you wanted to get beam bursts of a known length through that aperture, so one would want frequencies which were on the order of 2-1/2-mHz or higher. The buncher frequencies, which would allow you to phase properly, would usually be multiples of the choppers. Now, when you are looking at a given ion, you have a particular problem which comes into play for heavy ions which wouldn't be there for light ions. In the light-ion chopper, the speed of the light ions is so high that you can very often count on getting through the chopper plates before any substantial phase change takes place in the applied voltage, assuming the driving wave form is sinusoidal, so that one does not tend to get any strong rise time effects. Now, one can see in the limit, where one was staying in the plates too long, the potential would switch polarity and one would literally get the beam swept back and forth. There is a further problem which is associated with energy spread, which I will get to in a moment. I would like to point out here that, when you are getting to heavier masses, the flight time is going up very dramatically. I haven't extrapolated the curves down to injection energies, but I think I have some numbers that I can give you for the case of mass-60 down around 200-kilovolt injection energy you are talking about 1250 ns per meter. The flight time is getting quite long. The inevitable conclusion is that, if you want to avoid these problems in the limit where the flight time becomes very long, you have to end up increasing the amplitude on the chopper plates to keep the beam going through the aperture as it is getting swept back and forth and you will hit the plates. Somewhere along the line you have to change the length of the plates, or change the voltage. This affects the energy spread. The energy spreads you will see in a few moments are integrally tied up with the amount of bunching that you could effectively put in the beam.

If you are designing a light-ion system, one takes a look at the phase change in the plates, and there are articles by Neiler and Good which contain Fig. 3. This relates the entering phase of the beam to the phase that the beam occupies while it is traveling through the plates. If one takes a given δ, in this horizontal coordinate in this drawing, which is the transit phase or the phase which is occupied in the plates, and ω_o, which is the entering phase or the phase with respect to the sinusoidal voltage where the beam enters, and calculates, one finds that there is a unique ωt_o at $\pm \pi$ for which the beam will actually go through the axis point at a given distance away. ωt_o is a function of the ratio of the distance to the plates to the actual length of the plates, and the curves for this are shown in Fig. 4. One can either calculate or read off the curves, the appropriate phase angles. Then, after one goes through the calculation, one calculates the writing speed. The writing speed gives you, for

a given aperture size, the actual size of the beam burst to, at least, first order. If one knows the dc size of the beam, then the writing speed tells you how long it takes to sweep that size of beam through a finite aperture. If you assume that the writing speed is the writing speed on the axis, then it is approximately equal to the time interval it takes to sweep the aperture size plus the beam diameter. That would give you the base of a trapezoidal beam pulse which would come through. It's, initially, putting a fraction of the beam through, and, as the beam is swept through the hole, all the beam will go through the hole, then, finally, you will go past the hole and you will be getting the trail end of that trapezoid. That's, again, an idealized shape and I am sure that you will all tell me that what you get, in real life, is a shape that is much funnier looking than that.
What one does then is, take this retrace elimination scheme, which is a pair of deflector plates, and pulse it on with a rectangular wave, namely, when one takes a pair of plates, in the traditional system again, what one would do is ground one plate and apply a voltage to the other plate, thereby deflecting the beam away from that aperture. If one pulses that plate, by clamping a tube effectively to zero, then one would provide a field-free region for the beam to pass through, and it would only pass through according to the logic which you have prescribed with additional circuitry. So one sets up gate circuitry which will turn this tube on or off. One would only get pulses at the intervals when one wanted to. One could, effectively, count down the frequency by factor of two or by any factors that you want, using modern logic circuits to get the appropriate beam intervals coming through the final aperture. One point about energy spreads--in a sinusoidal system, particularly one where the phase is changing considerably when the beam is in the plates, one can see that the beam is going to enter on an equal potential which is determined by the field distribution in the plates, and since it is being deflected in the plates, it is going to leave on a different equal potential. Typically, the magnitude of the energy spread that has been introduced at this point, is a function of the beam diameter W, and the ratio of the transient times to the chopper period, and it is also a function of the amplitude on the plates. There is a limit to how much amplitude you want to put on the plates, because it is increasing the energy spread you are putting into the beam. You are not totally free to play with the parameters, you have to be careful.

When you are looking at heavy ions you have a problem of the transit time that I just mentioned, and they have this problem of energy spread. What one would like to do is to take a pulse and put all of the pulse, which is going to be bunched, or the useful time interval in that pulse, and put it in with a minimum of energy spread. The way to do this is to make sure that, when the beam arrives, it sees zero field on the axis, and also, if you can do it, keep that field constant over the period of time that the beam is in the plates. What we have gone to is an attempt to produce an essentially rectangular pulse in such a way that there is a symmetry plane in the center. We have done this by taking a 4-mHz sinusoidal signal and an 8-mHz sinusoidal signal and we have varied the phase between the two, and the amplitude ratio of the two, and you can set up conditions such that they add to an approximately rectangular shape. The widths of the pulses are within the time acceptance of the buncher. Typical time acceptances of the buncher are 20 to 50 nanoseconds, and you want to bunch down to about one nanosecond. If we take a look at how we arrange this, take another look at Fig. 1. In the case of the light ions, we put two plates in series, and if we add these two signals properly, the result is that the beam sees, essentially, a rectangular pulse all the way through that chopper. In the

case for heavy ions, this still won't work. One needs to have a much longer series of plates. If one looks at the transit times involved, each individual plate has to be quite short. We have designed a traveling wave deflector which produces a zero field on the axis whenever the beam is ready to enter it. So the only energy spread that you get is on the very entering rise time, and on the exit. Each of those 12 plates has 2 tubes, which makes 24 tubes altogether. The plates are pulsed in tandem so that the symmetry plane is, in fact, on the axis. When the voltage is collapsed on those tubes the beam again sees zero field and, therefore, it cannot get any energy spread. The use of this dc offset allows us to use it for count-down. We use this second set of plates to countdown for the first set and we also use it for both count down and chopping for heavy ions. In the light-ion application, the heavy-ion deflection system would be used for countdown and, in the heavy-ion case, the light-ion set would be off and the heavy-ion set would be used for both countdown and chopping.

The changeover between light and heavy ions takes place at mass number 60, but I think people will be using it a little earlier. The time interval we are talking about is variable from 10 nanoseconds to 100 nanoseconds per plate, due to the variable frequency arrangement on it. It has a variable time interval of the beam in the plate system, so that you have 12 x 10 to 100 x 12 for the total number of nanoseconds that the beam can spend there, and those are approximate numbers. You can make the transition between the two systems as you are looking at beam quality. I think it will be at the order of 60, because that is the designed-for number. That's why there are two sets of plates on the light-ion chopper which are about 4 inches long each. The traveling wave chopper keeps a much longer rectangular pulse on in the kilovolt region effectively for the beam. Each plate normally has voltage on it which prevents anything from going through the aperture. So when one pulses the first tube, one will get a zero deflection for the first plate; when one goes through the second plate one will get zero field, and so on, so the beam is going to go through the final aperture at the final end. In other words, if you collapse the next plate in order before the beam gets there, it doesn't know that there has ever been a field on that plate, if the beam was far enough away. It enters into a zero-field region and it will go through on axis and then one actually is pulsing the entire system and one runs a train of rectangular pulses along this set of plates. If you are talking of different velocities of ions involved, you need, effectively, a different length of plate.

We now come to the more critical application of bunching. "What is a buncher?" is a good question. If one has a gap between two cylinders at different potentials, one will accelerate or decelerate the beam, depending on the potential change. If one is modulating that field, then the amount of acceleration that's obtained by the beam will be related to the rate of change of that modulation voltage on the velocity of the beam entering. What one does is, one takes a beam of finite pulse width and attempts to slow down the leading edge of that beam and speed up the trailing edge this way, such that, at a given designed distance away, the beam reaches a minimum pulse width. This is analogous to a lens action in special coordinates, whereas this system works in time coordinates. Fig. 5 shows a beam with a finite time width plotted vertically and distance horizontally and one can see that you have problems similar to light optics in this time-optics region. If one focuses the beam down short, one introduces more angle into the beam just by geometrical optics considerations. The analog-to-angle in this system is, in fact, the energy spread. If this is the bunch point that we are talking about over here, the

final time spread at that bunch point is, in fact, going to be proportional to the bunching length and it is going to be proportional to initial energy spread which is in the beam. The final energy spread in the beam is going to be proportional to what the initial time spread was and it's going to be inversely proportional to the length. We have the usual considerations of optics involved in a time-lens system. The top equation in Fig. 6, is the time that it will take a given velocity ion to travel this distance, L, for a given mass and injection energy. If one introduces a time or an energy spread in the system, there are two ways of looking at the way the time and energy spreads are coupled. For example, the second equation in Fig. 6 shows that the time energy spread at the final bunch point, corresponds to a given amount of energy spread that originally came from the source. In order to make Δt_2 small, one needs, for large masses, to make L quite small. What does L mean? One might say that L is the distance to the experimenter's target because he is not interested in the bunched spread somewhere in the accelerator. However, if one looks again in a time space, the amount of time spent at high energy is really very, very small. Almost all the time spent is at the injection energy so that the lengths are only a little longer than the length from the buncher tube to the first acceleration section. If one calculates the effective length of the acceleration section, then all the drift sections at higher velocity, one sees that they contribute a very small amount for this total length. So, effectively, we are talking about the distance between the buncher and the first acceleration tube or first accelerating plane. This says for large masses one wants to make that length very small and one wants to make the injection energy large. If one has a time spread entering, such as Δt_1 in the lower equation of Fig. 6, that means what one needs to put in is an energy modulation ΔE_2, which will correspond to that chopped spread. There is a kind of emittance conservation involved. If one starts out with a given time spread and one wants to bunch it down to a much smaller time spread, one always pays the price in increased energy spread. That's really the moral of the whole story. We, in fact, have to provide a very large energy modulation if we make L small, E_o large, and M large. If in the case where M is getting larger, we compensate with a small L and a large E_o, we have to correspondingly keep the energy we are putting into the beam quite large.

What are, in fact, the parameters? You can see that the peak voltage one has to supply and the energy spread one is putting in, as well, is related to the frequency involved. Since this is usually the consideration, one makes L short and one tries to match the frequency to give the appropriate acceptance of that buncher and the appropriate bunching factors. The ideal wave forms have been calculated and, in fact, I have transformed them in a funny sort of way as shown in Fig. 7. It is a linear term plus second-order terms. In order to apply all that energy modulation, the only way to do it is with tuned circuits, so we have to use sine waves. That means you only get matching over a small phase angle. The ΔT at the bottom of Fig. 7, is what is plotted on Fig. 8. It is the difference between the linear parts of the sine wave and the ideal wave form. One can play a few tricks by moving the phase slightly so that you are exactly phased correctly. One can also increase the amplitude slightly over the calculated amplitude needed, in order to give a much longer period where your error on this graph is small, plotting the error, vertically, vs the time, horizontally. If you take a look at the difference between the sign wave form and the ideal wave form, as shown on Fig. 8 you want the time difference to correspond to something of the order of the time spreads you are seeking in the final pulse. It pretty much prescribes the type of acceptance phases that you

are going to have in the buncher. Any other time it enters it's going to be wasted and you are not going to get efficient bunching.

Looking again at Fig. 1, in the Gerry system we have provided for two bunchers. The light-ion buncher is farther away and allows you to use reasonable voltages to match for light ions. There is a heavy-ion buncher which is inside the tank, that has to be remotely controlled. You can't just put in one accelerating gap, when normally it has a tube which is raised to a high potential and one has an entering gap and an exit gap. The exit gap has to be phased properly so that you are still getting bunching on the way out. That means that there is a series of lengths which can be used for bunching for a given injection energy. Looking at Fig. 9, one can see how the length of the buncher tube goes as a function of injection energy and mass. One can see how to use different values of this n which corresponds to the bunching wave form going through several cycles. In order to cover the periodic table, one starts with N=1 for the light masses in a system where we had only one buncher and then we go all the way up to the other end, to presumably mass-240. One needs to have fairly high injection energies in order to do this. What we do is we allow some variability in the injection energy, but we still want to keep it high for the heavy ions. We also provide for different lengths of tubes in this system. In the light-ion buncher we take a series of tubes in series and interconnect them so that we get different effective lengths. The buncher that we are putting inside the tank is actually a turret arrangement as shown in Fig. 10. One rotates in different length tubes in different combinations to allow one to bunch efficiently ions of different masses. The old technique used sliding contacts, which is a very poor way to make good connections in a vacuum system. We are capacitively coupling the system by rotating in a set of meshed plates for each buncher tube that we get into position. This allows for efficient coupling and we are locating the system with a Geneva mechanism which is run by a ratchet in the vacuum. The Geneva mechanism is a very precise locating system.

There are some considerations about the maximum energy spreads one puts in these systems. You can see, if you put too much energy spread in, you are not going to get around the transport system too efficiently. This energy spread limit is shown in Fig. 11 for a typical stripper canal for an MP, and one can see the percentage of modulation that can actually be put in before one starts to overfill that stripper. They correspond to this sort of energy spreads here. These are, in fact, the maximum time spreads that can be bunched. As you can see, you can get numbers consistent with the acceptance of typical bunchers, if one is careful with it. Finally, there are time spreads that are induced in the beam transport system. One of the places that you get enormous time spreads, particularly with heavy ions where you have different velocities for the trajectories, is in the normal analyzing magnet setup with crossovers at the object and image slits, which lead to different length trajectories, as shown in Fig. 12. One can run it in an isochronous mode, as shown in Fig. 13, by opening the slits and forcing a crossover in the center. I am sure I will get many complaints from audience people because they don't like to tune systems this way. There is an alternate way to do it, and that is to use a focus at the image and object slits, but use another magnetic element to compensate for the path difference, and usually a switching magnet can be used for that purpose. These are two considerations people use in designing the beam transport system. And lastly, I promised you a picture of the post-deflection system, because at much higher energies one needs to have a really mammoth system in terms of the power and size. One normally makes the post-deflection optics part of the tuned

circuits with very long plates and extremely high power input. Referring to Fig. 14, there is tank coil for the circuit right there. There are several of these units out in the field and they are a bit of a nuisance to pump but they work quite well. Remember we had a pulse which was gaining energy spread only on entering and leaving the heavy-ion chopper and that produces an effective time spread further on, because that part of the beam is really out of the central region of the acceptance of the buncher wave form. One chops to take away those tails so one might meet the specification at FWHM without a post-acceleration chopper and you might meet it full width, 0.1 maximum, when you put one of these things in, and you also will get rid of the dark current between the pulses much more efficiently.

Discussion:

Wegner: What kind of minimum time lengths do you see on target, say with a nickel beam?

Liebert: I think we guarantee numbers which are larger than what we see. We expect to see a nanosecond or less, but we guarantee numbers in the order of 2 or 3 ns.

Wegner: Is the system operational now?

Liebert: No, the system should be shipped fairly soon to NEC but it's not ready at this moment.

Norton: One question I have is, what is the i.d. of the post- acceleration chopper? We are involved with the optics and need to know exactly where to place this.

Liebert: I don't have the number at the moment, but I can get it for you. In fact Terry Lund probably has that number somewhere. It will have to be larger for yours. Terry can tell you what it is in his system.

Lund: Yes, off the top of my head, on the Rochester system it is about 2 inches between the plates.

Schultz: What are the lengths of the new 12-plate traveling wave deflector and what is the pulse amplitude?

Liebert: The pulse amplitude is under a kilovolt per plate and I think it is substantially under a kilovolt but the plate length is in the order of 2 inches. I don't know the gap off hand.

References

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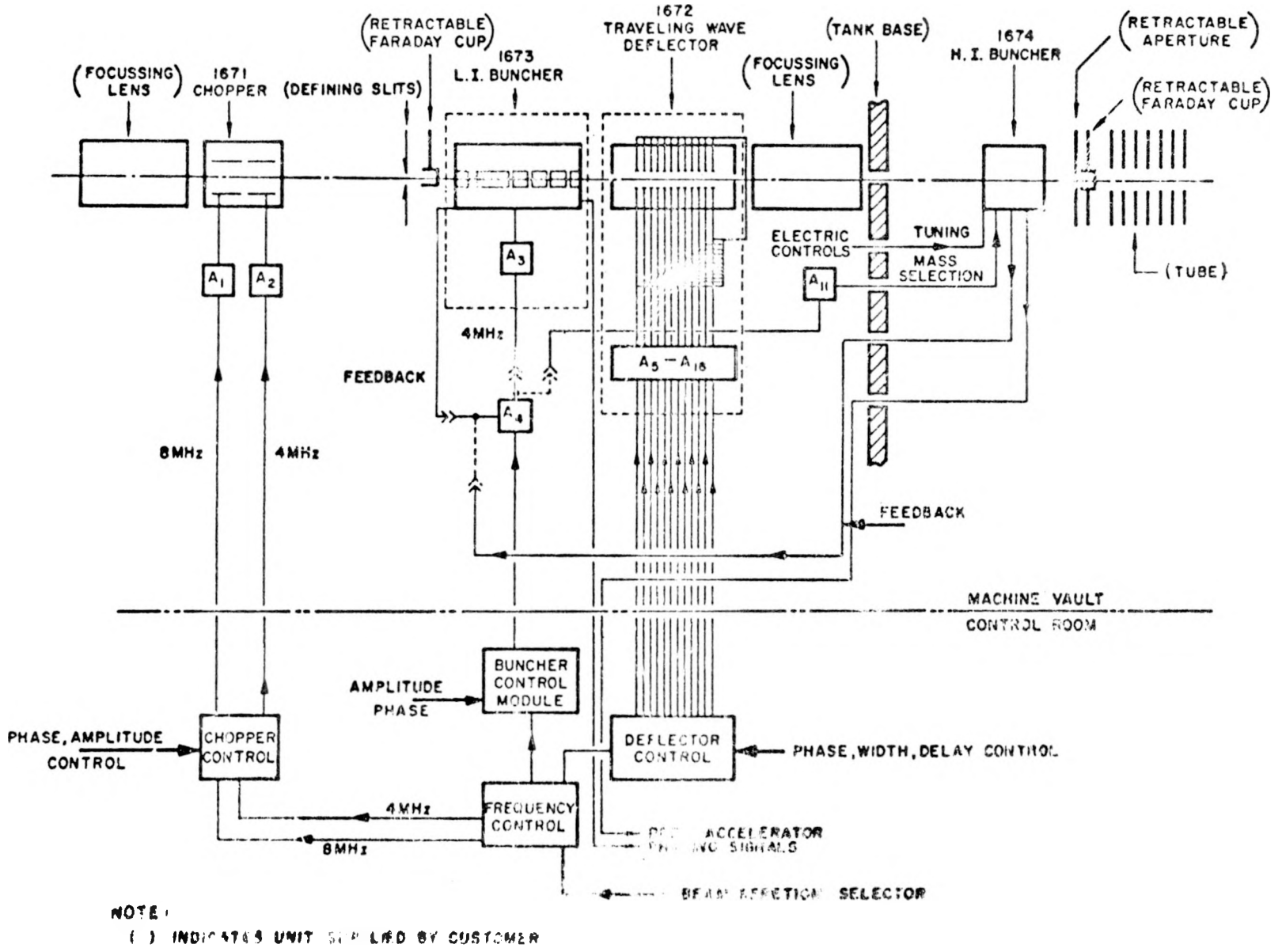

(FOCUSSING LENS)
1671 CHOPPER
(DEFINING SLITS)
(RETRACTABLE FARADAY CUP)
1673 L.I. BUNCHER
1672 TRAVELING WAVE DEFLECTOR
(FOCUSSING LENS)
(TANK BASE)
1674 H.I. BUNCHER
(RETRACTABLE APERTURE)
(RETRACTABLE FARADAY CUP)
(TUBE)
ELECTRIC CONTROLS
TUNING
MASS SELECTION
A1
A2
A3
A4
A5 — A16
A11
4MHz
FEEDBACK
8MHz
4MHz
FEEDBACK
MACHINE VAULT
CONTROL ROOM
AMPLITUDE
PHASE
BUNCHER CONTROL MODULE
PHASE, AMPLITUDE CONTROL
CHOPPER CONTROL
DEFLECTOR CONTROL
PHASE, WIDTH, DELAY CONTROL
4MHz
FREQUENCY CONTROL
8MHz
ACCELERATOR
BEAM DEFLECTION SELECTOR
NOTE:
( ) INDICATES UNIT SUPPLIED BY CUSTOMER

Fig. 1

$$t_o = L\sqrt{\frac{m}{2E_o}}$$

I. Δt_2 (Target) $= L\sqrt{\frac{m}{8E_o^3}}\ \Delta E_1$ (Source)

m large → need $\begin{cases} L \text{ small} \\ E_o \text{ large} \end{cases}$

II. Δt_1 (Chopped Spread) $= L\sqrt{\frac{m}{8E_o^3}}\ \Delta E_2$ (Required Energy Modulation)

$\left.\begin{matrix} L \text{ small} \\ E_o \text{ large} \end{matrix}\right\} \rightarrow \Delta E_2$ large

Figure 6. Buncher Operation

rf Voltage: $V_p \cong \frac{1}{2\ \omega L}\ \sqrt{\frac{8eV_o^3}{m}}$

Ideal Waveform: $V(t) = Kt(1 + \&)$

$$\& = \frac{3}{2}\frac{t}{t_o} + \frac{4}{2}\left(\frac{t}{t_o}\right)^3 + \text{---}$$

Sine Wave: $V(t) = Kt(1 - \&')$

$$\&' = \frac{(\text{Sin } \omega t - \omega t)}{\omega t}$$

$$K = \frac{2\ V_o}{t_o}$$

Δt Due to Nonideal Wave Form $\cong \frac{\Delta V}{K} \cong \& - \&'$

Figure 7. Buncher Parameters

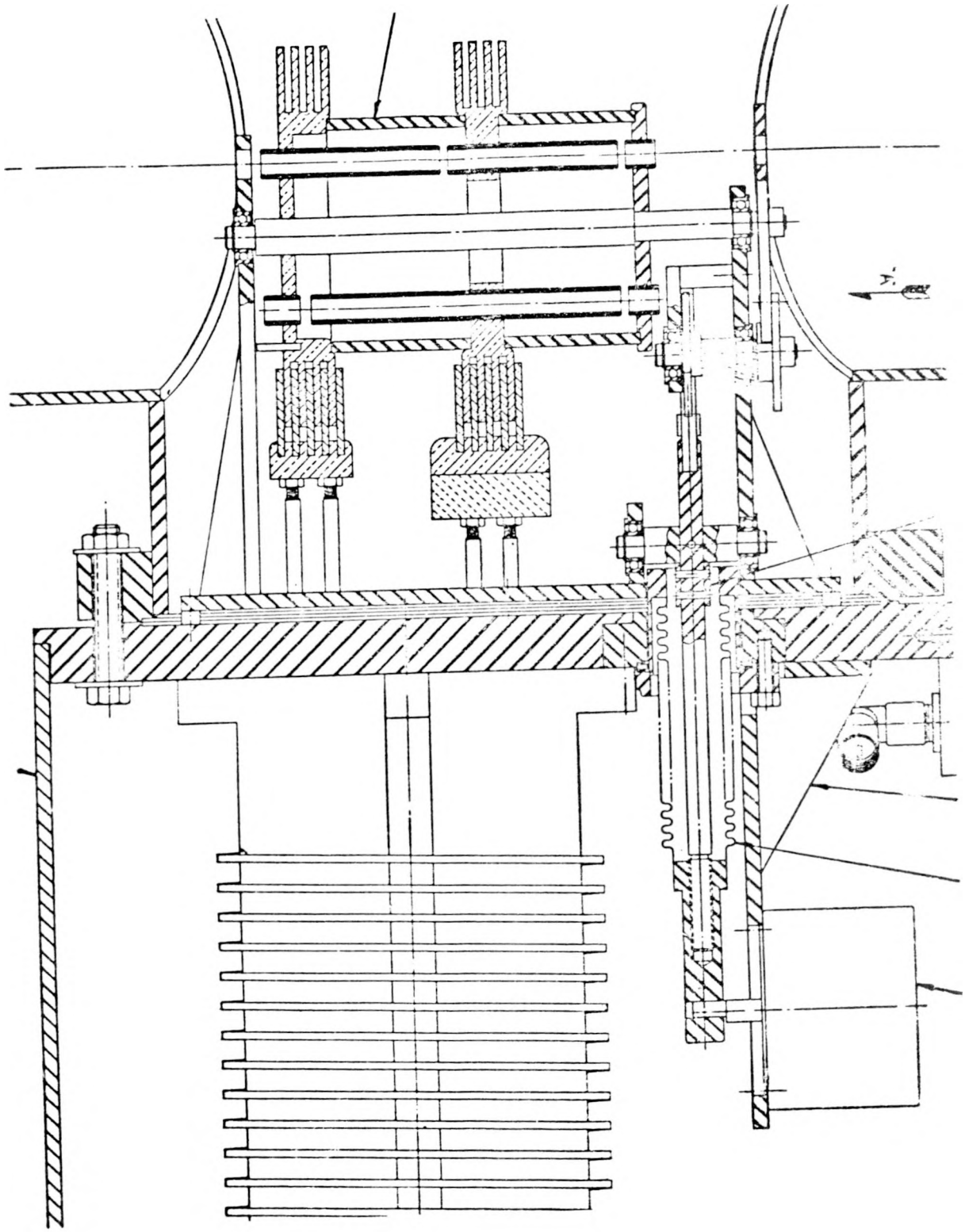


Fig. 10

BUNCHER DEVELOPMENT FOR ORIC INJECTION

R. Milner, N. F. Ziegler
Oak Ridge National Laboratory

The Oak Ridge Isochronous Cyclotron, ORIC, operates over a radio frequency range of about 7 to 21 mHz and an orbital frequency range of about 4-1/2 to 14-1/2 mHz. This is for ions in a mass range of 12 to 240 amu. For ions in a mass range of, say, 12 to around 140 amu, the rf frequency is three times the orbit frequency. Of course, in all cases the acceptance is defined by the radio frequency at which the cyclotron operates. For a tenth percent energy resolution, which means single turn extraction, this acceptance is about ± 3° of the Dee frequency. In units of nanoseconds, just to get us oriented, this window varies from about 1.1 nsec for carbon to about 2.2 nsec for iodine-127. Those are ions which are accelerated on the fundamental whose orbit and radio frequencies are the same. For ions above mass-140, the window varies from about 0.8 nsec up to about 1.2 nsec for ^{238}U. Our task then is to try to inject as much beam as possible from the 25-MeV tandem into this ± 3° window.

In selecting a system, we have made the assumption that the ion source will often be a limiting factor, in other words, it will limit the amount of dc current that can be produced, and there will be many times when the acceptance of the accelerator is not the limiting factor. For this reason, we have decided we need to have a bunching system which has a rather high efficiency. We looked at the maximum efficiency, this is without considering chopping, just the maximum efficiency of bunching systems. We found that roughly, for a single harmonic buncher of the conventional klystron type, about 30% of the beam can be bunched into 3 degrees, for a two-harmonic klystron-type buncher with one accelerating element. We will have something over 50%, and for a two-harmonic double-drift buncher, we should have an efficiency of slightly over 70% for bunching at 3 degrees. A multiharmonic or sawtooth drive voltage could probably approach 90% with a single acceleration gap.

We believe that for our purpose the double-drift system is probably the best choice. It has a relatively high efficiency, is fairly simple, and it can be easily operated over the required frequency range. Before I get into too much discussion on our own application, I would like to show a few slides which I hope will illustrate how the double-drift buncher works and how it differs from the two-harmonic buncher. Figure 1 is just a computer demonstration, if you like. What I will do is just show some simple calculations for a specific case. This is buncher one, which is located 37 meters from buncher two, and is located 100 cm away from the target. This is all in free space and is just for demonstration. The case shown in Fig. 2 is an ion of mass-16, charge state one, with an injection energy of 82.88 kilovolts to make all the other numbers come out even. Frequency one is 10 mHz, and frequency two of the second harmonic buncher is 20 mHz, and the amplitudes on 1 and 2 are about 1.7 and 6 kilovolts, respectively. The 100 nanoseconds is the time between pulses and the distance traveled per pulse is 10 cm. Buncher length 1 is 5 cm, and buncher length 2 is 0.5 cm. We will start out in Fig. 3 with just a uniform dc beam whose profile is shown below. The dotted line shows how much energy modulation would have to

be given to each part of the beam. The trailing part of the beam would have to be energy modulated by around 4 or 5 kilovolts to be increased in energy, and the leading edge would have to be decreased by the same amount and is almost a straight line. The sinusoidal curve is the amount of energy that we actually apply to each part of the beam with the first-harmonic buncher shown above. Notice that we over-bunch in a region which covers somewhat more than 180 degrees. Here again in Fig. 4, the dotted line shows the amount of energy modulation needed for perfect bunching. You can see now the profiles start to change, and particles are moving in from this region towards the center. Here in Fig. 5, 200 nanoseconds later, we still show the energy required for perfect bunching and the bunch starts to form. Now, at the location of the second buncher, operating at twice the frequency as shown in Fig. 6, we see that, in a region of approximately 180 degrees of the total bunch, or the total sausage, that the energy requirements can be rather well matched with a sine wave of twice the frequency of the first buncher. This is just matched up manually by hand, plus no great effort is made to match it perfectly. At this point, quite a bit of the beam has been moved into the region over which this can operate effectively as a corrector. As shown in Figs. 7 and 8, from then on the thing just bunches. Figure 9 was not really very well tuned. These wings should have been actually bunched a little better than that, but it is easier to tune it with a knob than it is with a computer.

We decided sometime ago to implement a double-drift buncher of this type on the EN tandem to test out certain things about it and to develop a control system for it, and to generally be ready to operate the thing when the new tandem is finished. The system that we have installed on the EN tandem is shown in Fig. 10. We have an oscillator whose frequency is variable and can cover the range that we would like to cover. We, essentially, have the first and second harmonic bunchers. The first buncher is driven through an amplitude control on the broad-band rf amplifier and the second harmonic buncher is driven through a frequency doubler, a phase shifter, and an amplitude control on the same sort of power amplifier. There is actually a resonant circuit which is not shown in this schematic. The Q of the resonant circuits are fairly high, like around 400, so that not much power is required. In the EN tandem case, the first buncher is about 1.4 meters from the entrance to the accelerator and the two are spaced only 26 cm apart. The beam goes through these two bunchers, through the accelerator, through a pair of 90-degree bends made up of the energy analyzing magnet, and a 90-degree bending magnet which operates in a fairly isochronous mode. The beam is then detected by a capacity pickup unit which is a very conventional design and also a fast Faraday cup which already existed in our lab and was not designed especially for this beam. The signal from the capacity pickup unit and the Faraday cup is amplified by two broad-band preamplifiers made by Hewlett Packard. Model 8447F is what we used. The output from the fast Faraday cup is observed on a sampling oscilloscope. The output from the capacity pickup unit is fed into the start pulse of a TAC and the stop pulse of the TAC is derived by looking at coulomb excitation gamma rays produced when the beam hits the stainless steel beam stop in the fast Faraday cup. So we start the TAC every time a pulse arrives and only very rarely do we stop to get a stop pulse, because the efficiency for this gamma-ray production is very low. In this way we are able to measure the absolute efficiency of the bunching system. We usually use the fast scope for tuning purposes; this other device is much too slow to use for tuning. Figure 11 shows the first and second harmonic bunchers that we actually have installed. These devices are in the same vacuum chamber on the EN tandem. They will later on be removed from this aluminum flange and

will be mounted on a Conflat flange and will be mounted in separate vacuum chambers about 1 meter apart on the 25-MV accelerator. Figure 12 shows this plate with the two bunchers being installed in the vacuum chamber. There is some other equipment in this chamber, such as a cup and so forth, which is used for regular tandem operations. These two boxes above contain the resonant circuits, the inductors, and the rf feedthroughs which supply the drive voltage and we are using sliding contacts. Right here you can see there is a flat plate which slides between two contacts which are connected to the high voltage. We don't really expect to move these contacts very frequently. So far they have not been a problem. The thing after it has been put together is shown in Fig. 13. The rf is fed in at the very top and is inductively coupled to the resonant circuit. Both these boxes are essentially identical except for the inductor and the capacitor, though this is the tuning capacitor here. Now this system will eventually be controlled by CAMAC and have analog stablization circuits for stablizing the rf amplitude, as well as the phase between buncher 1 and buncher 2. The amplitude stablization needs to be good to probably better than a per cent and the phase between the two bunchers should be at least $\pm$ 0.5^{o} or better. But that's the same sort of control that we need in phasing this thing to ORIC anyway so it's not a special problem.

Figure 14 shows an oxygen beam,which is the only beam that we could easily use to measure the absolute efficiency of this system because it was the heaviest beam that had enough energy to produce enough coulomb excitation of that stainless-steel beam stop. Here we see a TAC spectrum from 20-MeV oxygen with the first harmonic buncher at 8 mHz, the second at 16 mHz and about 59% of the beam fell within $\pm$ 3^{o}, and this is about 2.1 nanoseconds. Full width at half max of this is about 1.25 nanoseconds. There is probably a slight contribution to the width from the time resolution of the detection system, but probably not very much. We had rather limited dynamic range on the plastic scintillator side and probably very little time spread introduced by the capacity pickup unit and that time pick-off circuit. The buncher system that we will install on the 25-MV tandem will consist of two bunchers located about 110 cm apart, centered on a line with the ion source, 90 degree mass-analyzing magnet, and a set of slits where the mass analysis is done and the entrance to the accelerator. This set of slits is about five meters from the entrance to the accelerator. The beam will be converging into this, then diverging out and being refocused by a quadrupole lens here. The optimum choice, in many respects, for the location of these two bunchers is just ahead of and just following this cross-over in the beam. I did not mention that this particular set of data is taken with an ungridded buncher. We have done the experiment with conventional fine mesh grids, which ended up cutting out almost sixty percent of the beam because there were 8 grids. We have done an experiment with coarse grids which were 3-mil wire on 200-mil spacing that cut out 18% of the beam. It did produce some noticeable focusing and defocusing effect and we lost about 12 or so percent of the beam, probably because it wasn't focused perfectly on the stripper. We would like to start initially operating the bunchers on either side of this waist, where the beam is small, and either operate the system ungridded or with the coarse grid. If we operate ungridded then we have apertures here, which we could use to limit the acceptance of the accelerator, so we essentially throw away that beam which is being somewhat defocused on that entrance aperture. We really don't expect the focusing and defocusing effect will be very severe, even with the ungridded bunchers located here because the beam is fairly small. These are one meters apart and it is 50 cm from either side of the slits. The focal length of the lens is still fairly long. It is

probably on the order of 5 to 10 meters for the amplitude we will be running and the focal length of this element, for example will be one meter.

Discussion:

Schultz: Could I have a little more detail on your capacitive pickup along with what your average dc beam was when pulled?

Milner: This capacitive pickup unit was a very simple device. It was a 50-Ω feedthrough and some sort of ceramic insulator. It was a homemade device consisting of a little stainless steel pipe Dee which had two little plates on either side, with an aperture, so that the beam couldn't hit the stainless steel tee. It was not designed for this job, it was one that had been designed many years ago for protons and it just existed, and we put it in the beamline and used it. It was not too far from ideal as far as the length was concerned, and it was used in a place where the beam size was rather small, and the i.d. of it is probably 3/8 of an inch with a length of about 2 inches. We plan to buy one of the capacitive pickup units sold by NTG and try that out. It has Conflat flanges and it should be compatible with the vacuum system on our accelerator. The average dc beam varied between 150 to 250 electrical nanoamps. That was charge state four with oxygen. We also ran ^{63}Cu and ^{58}Ni, and there, the beam was quite a bit smaller. For ^{58}Ni we had about 7 electrical nanoamps and we were not able to see the pulse on either the fast Faraday cup or this capacitive pickup unit. With 150 nanoamps it was easy to see on either one of these devices, using this broad-band preamp, the Hewlett Packard 844F, which has 48 db gain and a pretty good noise figure. For the ^{58}Ni beam, we used a little homemade device which has a 50-Ω line which comes down the center part of this transmission line and has a carbon foil attached to it. The outer conductor was capacitively coupled at this point and came down and had a grid on either side of this carbon foil and, again, a capacitor here. We applied about 200-500 volts here and these capacitors by-passed the rf but isolated it from ground. The beam came through here and knocked off secondary electrons, which were then accelerated away from the carbon foil by this 200-500 volts. For the nickel beam we got an amplification factor of about 40 or more secondary electrons for each ion that went through. So we were able to easily tune a 7 electrical nanoamp nickel beam using this device. That will not be useful with a pulse detector for keeping a system phase-locked to ORIC. It was a simple device that worked surprisingly well.

Wegner: The 59% was percentage of the dc beam?

Milner: Yes.

Wegner: The classical problem with harmonic bunchers is that the amplitude and phase have to be really held cold or things drift around. What did your long-term stability look like?

Milner: We had no stablization circuits operating at the time we took this and it did drift some. It was actually possible to keep the thing under control manually. I would say you would have to tweak it every 15 minutes. This little experiment, that I showed here, was taken without any adjustments, but it was taken for a fairly short time, like 15 minutes, so it wouldn't drift out of control. But we do expect to have stablizing circuits for that, but inherently it is pretty stable.

Wegner: What did the full width of the nickel beam look like in time?

Milner: It was about 1 nanosecond. I must point out that the way we were using this we did not observe the time spread due to noise in the ion source. Our ion source has a fair amount of ripple on it. We used the capacitive pickup unit to trigger the sampling scope or, for some cases, the thing was strong enough that it was self-triggering and then we could use it self-triggering. We had about plus or minus 130 volts ripple on the ion source which would produce about a 2-nanosecond jitter and we did observe that in fact it was occurring. This is what the buncher is doing and not measuring anything about the ion source.

Schultz: Did you use the capacitive pick-off in conjunction with the foil system to measure the nickel beam?

Milner: The nickel beam was just measured from this device.

Schultz: You didn't compare it with your basic frequency or anything else, it was just a straight current measurement?

Milner: We just used the signal from this into a oscilloscope using the self-triggering mode. We didn't have enough signal from the fast Faraday cup or the capacitive pick-up unit to trigger it.

Schultz: Is there some reason that I am not thinking about right now, that you couldn't use the basic frequency.

Milner: You could use the basic frequency. In that case, the time jitter in going through the accelerator, which is mainly due to the ± 130 volts of noise in our ion source supply, would actually produce about 2 or 3 nanoseconds time jitter in the transit time, mainly, between the buncher and the accelerator. The one nanosecond is with that essentially removed.

Liebert: Is the beam profile anything like those first pictures you showed us with the tails?

Milner: Yes, very much. What we did was had the start pulse from the capacitive pickup unit and then we had the TAC set so that it would cover 2 or 3 pulses so that the dynamic range of the time-to-amplitude converter was, say, 400 nanoseconds. You could see a couple pulses spread way out in time and other faults. The beam through here was small and nearly constant in the dark current region and slightly tailed up here. You saw just about all there was to see except for just a long flat dark current between pulses.

Moak: You said that these pulses had everything to do with the ion source taken out, you didn't mean that the intrinsic energy spread of the ion source was taken out. That was included.

Milner: That's true.

Moak: This was just time jitter due to ripple on the power supply that was taken out. The other thing was, none of this was taken with any chopping whatsoever. The skirts would be at their maximal values because the choppers aren't even operating yet.

Milner: Yes.

Lund: You haven't mentioned rebunching at all. Do you plan on rebunching?

Milner: No. One problem is, that at the energy the beam will have when it comes out of the accelerator, it will take a rather sizable amount of energy to do almost anything as far as rebunching, or just leaning on the pulse to change its time of arrival. The amount of energy is something in the order of 250 kilovolts per nanosecond or more at that point. One problem is that between the foil stripper and the second turn in ORIC is very tight spacing. It will not stand very much energy spread in the beam, maybe 100 kilovolts, but actually, they feel comfortable if it's only like 50. One can't get too carried away about putting in rebunching and so forth.

Lund: So you are going to really push the time jitter through the machine to the limit, because you have 5 meters of drift.

Milner: We realize, particularly, for those ions above, where ORTIC is operating on the third harmonic, that it's cutting it a little close. We have provisions with NEC to provide some additional vacuum chambers so that we can move the bunchers closer to the accelerator than that. But from many standpoints this is the ideal location. We probably would not be able to operate them ungridded if we were at any other location. The other thing is that nobody has seemed to worry that much about beam between pulses. When it hits the foil stripper in ORIC, 10 or 15% is going to come out at the right charge state anyway, so the thing is going to be full of beam which has to get lost somehow. I can't see that we are really justified in going to a lot of trouble to just completely eliminate beam between pulses. This is a somewhat different criterion than one has for the normal time-of-flight experiment. We have given some thought to how we would use the system for that, too, but we would have to do different things.

Liebert: Did you make any attempt to estimate the source energy spreads, and, also, the chopper when you put it in, will it also introduce some energy spread?

Milner: I am not really convinced that we are going to put a chopper in there. We have made provisions for a chopper, and a chopper will be designed. Whether it will be operating, initially, or not, I don't know. But that's true, it would. The other thing that I did not probably stress is that we will be using a buncher of 6 cm for the first harmonic, and 3 cm for the second harmonic, for all ions. It just so happens that the way the ORIC frequency varies with the ion masses makes the buncher length being nearly constant within ± 15-20%, at the most. We do have some variation on the injector energy that we can use, and you know nothing really bad happens to you if the buncher length is off 20%, you just have to drive it a little harder. There was another part of that question, the five-meter location would work O.K. for energy spreads for ions below about 140 mass units for energy spreads approaching 200 electron volts. That would be cutting everything right, the model has to work exactly, but at 100 electronvolts things would be pretty comfortable. Above mass-140 we would probably not be able to get more than 40 to 50% of the beam into the 3 degrees. Below mass-140, or wherever that cutoff is, we would expect to get 55 to 65% of the beam.

Liebert: One other point, whenever you go to a very large mass there are space charge forces involved. Have you considered what you are going to do about that?

Milner: The buncher model calculations did not include space charges. I don't really think we are going to have a serious problem because I doubt that our dc beam currents will be that great. Somebody made the statement that I hope we do have a space charge problem, but the beam density following the buncher will be somewhere in the order of 5 to 15 times the charge density of the dc beam, but that still doesn't tell you very much. We are probably approaching a region where there will be some space charge effect, but I don't think it will be serious yet.

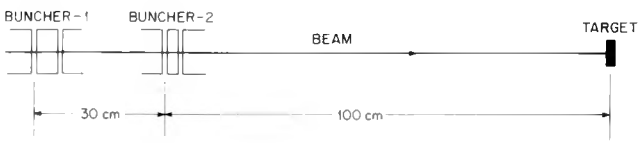


Fig. 1

ION MASS = 16 amu

CHARGE STATE = 1

E0 = 82.88 keV

f1 = 10 MHz

f2 = 20 MHz

A1 = 1.7 kV

A2 = 0.6 kV

ΔT = 100 ns

$\Delta \ell$ = 10 cm

BUNCHER-1 LENGTH = 5 cm

BUNCHER-2 LENGTH = 2.5 cm

BUNCHER SEPARATION = 30 cm

BUNCHER-1 to TARGET = 130 cm

Fig. 2

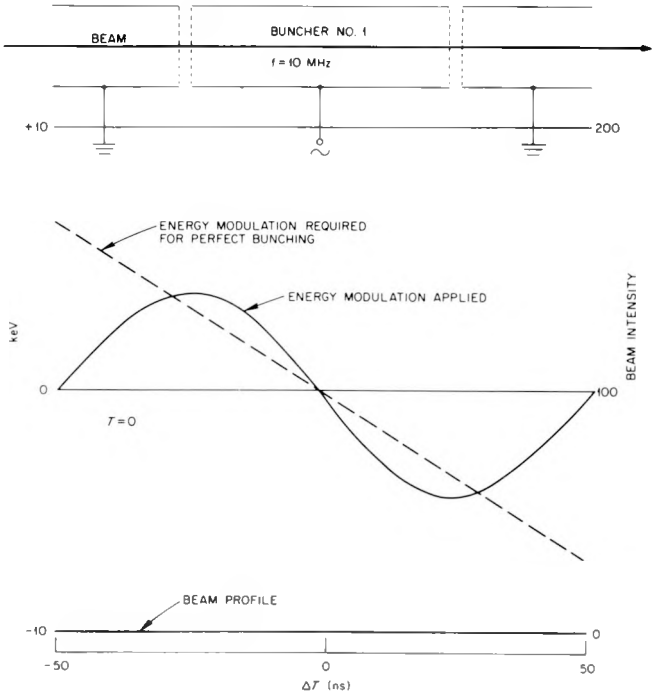


Fig. 3

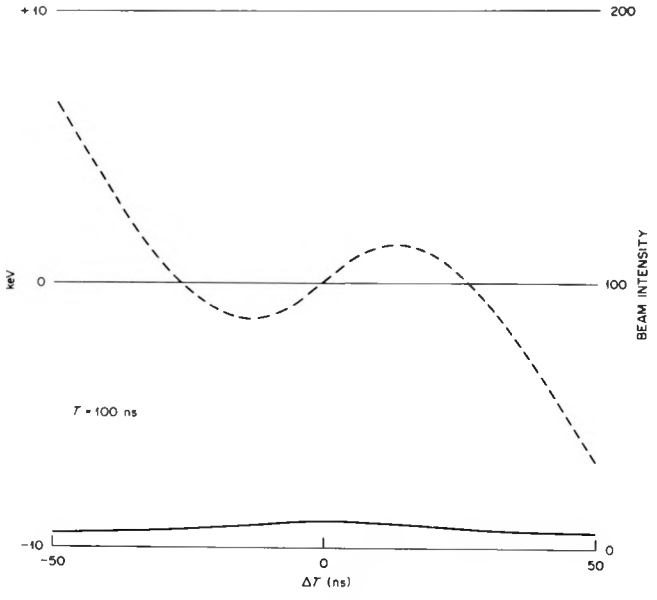


Fig. 4

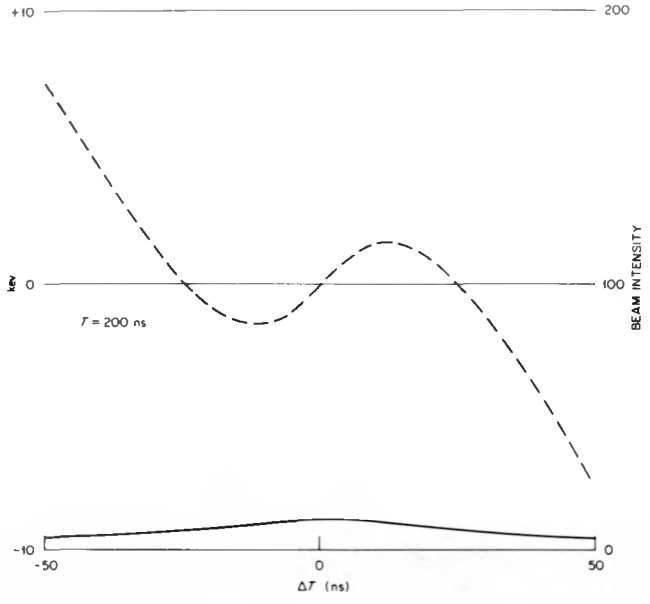


Fig. 5

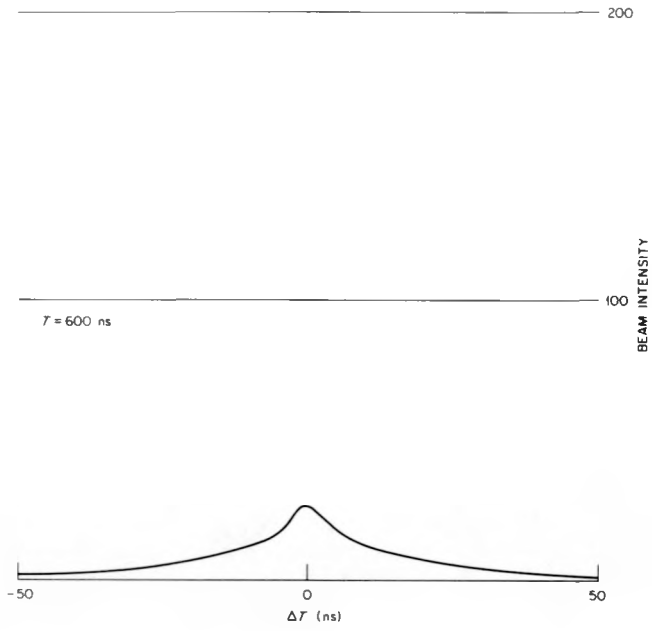


Fig. 7

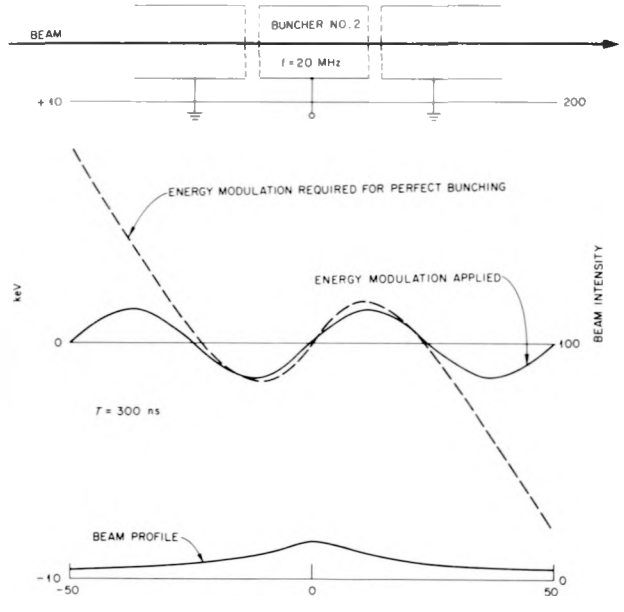


Fig. 6

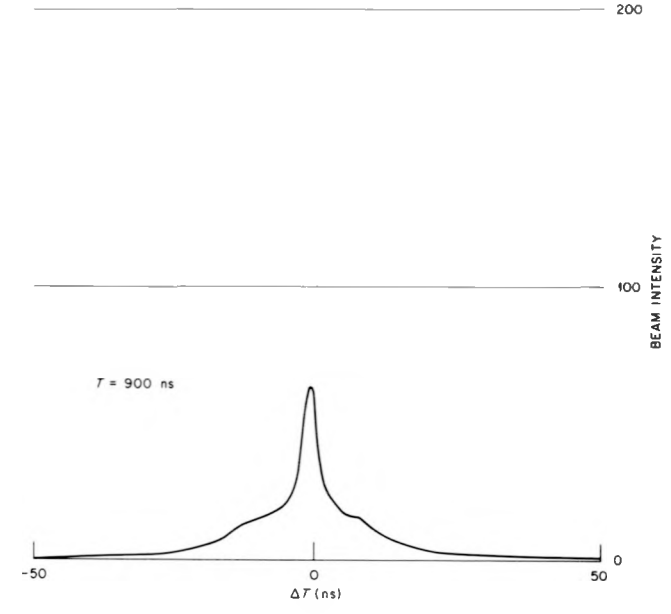


Fig. 8

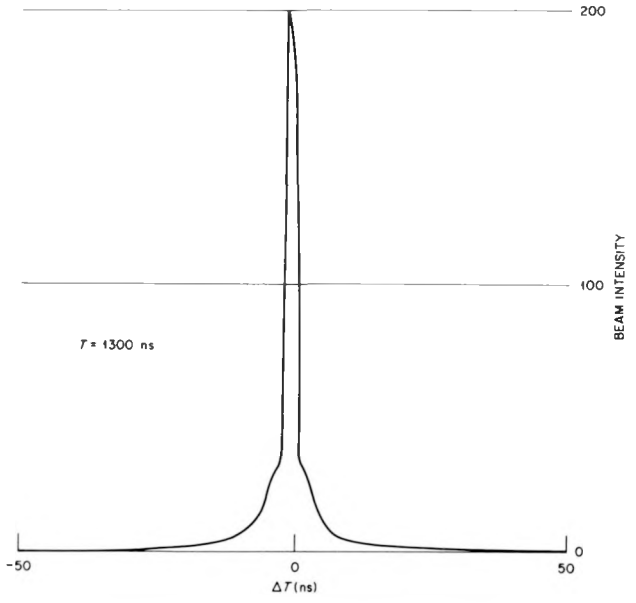
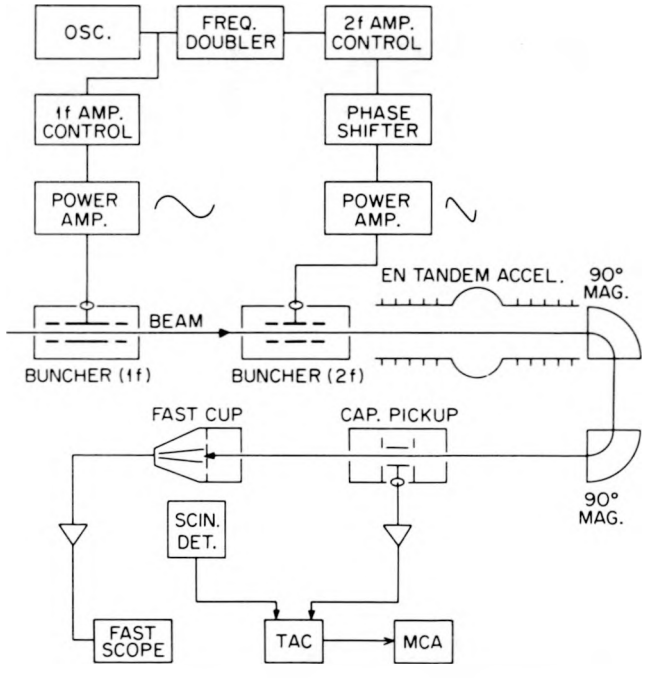


Fig. 9



Block Diagram of Buncher Test Facility.

Fig. 10

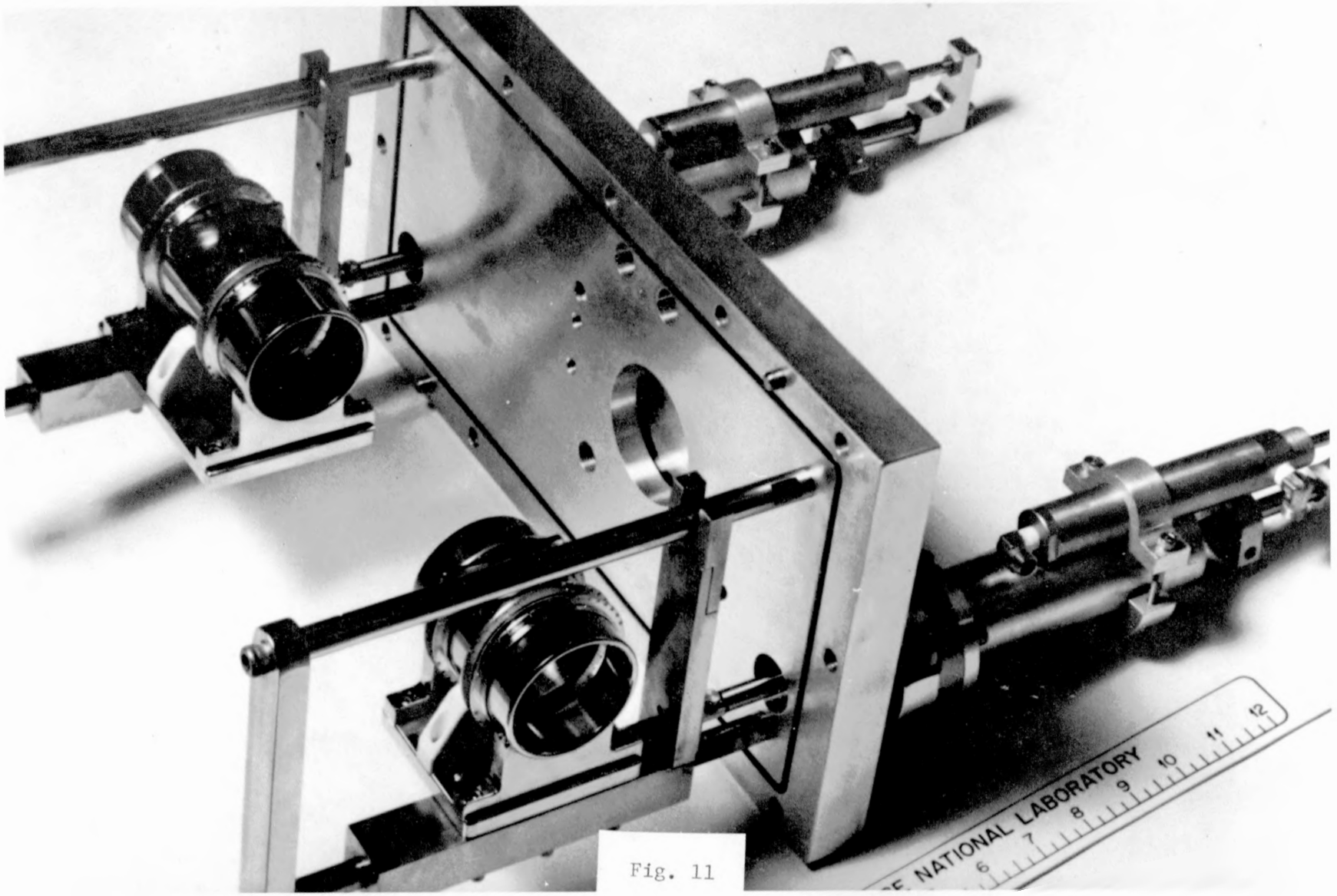


Fig. 11

Fig. 12

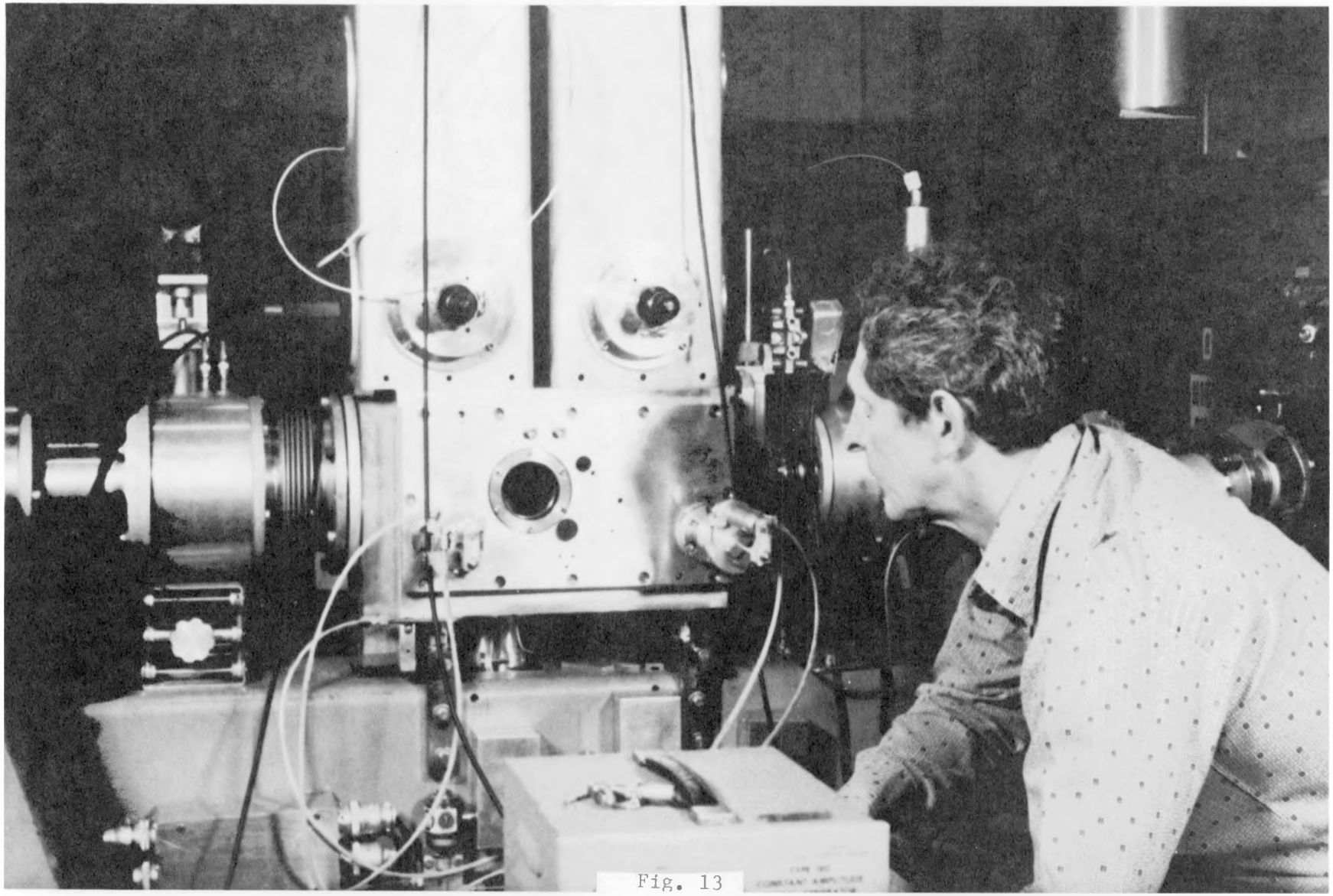
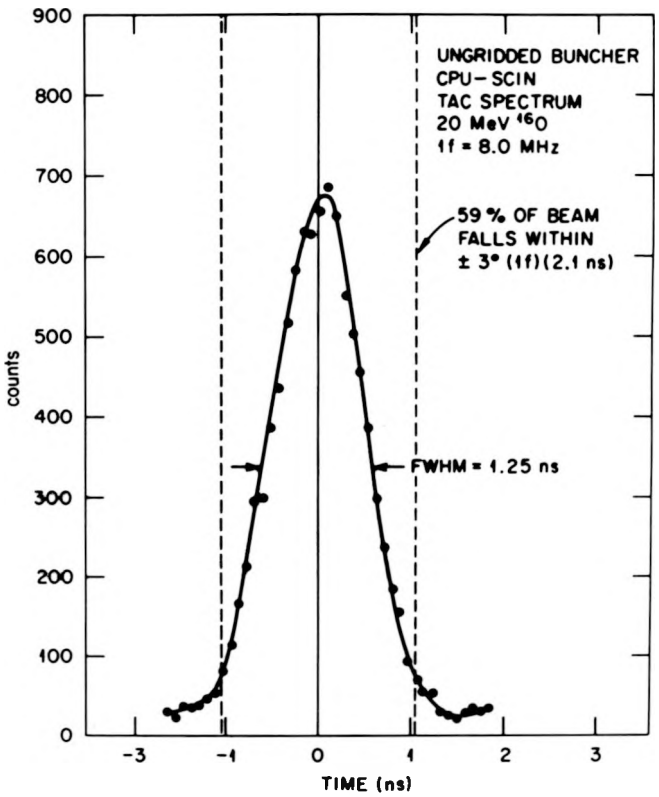


Fig. 13



Time Structure of Bunched Beam

Fig. 14

Session VI, Chairman, H. Haberl

MICROSCOPIC ACCELERATOR PHYSICS - I

Discussion of Important Physical Phenomena in the Tank Insulating Gas Mixture *

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I. INTRODUCTION. Puzzling mysteries of the V_TMax.MP (terminal voltage).

In 1965, after vigorous voltage conditioning efforts with a continuous discharge technique, the Yale MP-1 exceeded its guaranteed V_T(terminal voltage) = 10 MV and eventually reached V_T = 11.8 MV and successfully ran experiments at V_T = 11.5 MV. Now we know this voltage limit for the MP is imposed by the gas (N_2/CO_2 mixture) as I will explain in a later section. We initially reached that limit by using a few curies gamma-ray radiation source (one or two) and later on we finished the initial voltage conditioning without them. The typical gas mixture was Mixture H = 144 psia (80%) N_2 + 36 psia (20%)CO_2 = 180 psia, but we ran over V_T = 10 MV comfortably with much less pressure such as 125 psia.

At Chalk River, the MP-3 reached V_TMax. of 16 MV without a tube using 100% SF_6, however, $V_T$max. with tube reached only 10.5 MV with the original Aℓ electrode tubes. Later MP-9 (Orsay) and MP-10 (Strasbourg), both using 100% SF_6 as their insulating gas, barely managed to reach a V_T max. with a tube of 8.8 MV and 8 MV, respectively, with the Aℓ electrode tubes (Table 1).

In 1972, the Chalk River MP-3 was up-graded by installing the stainless steel electrode accelerating tubes newly developed by HVEC and easily reached a V_TMax. with a tube of 13.7 MV and later MP-9 (Orsay) and MP-10 (Strasbourg) also reached V_TMax. = 13 MV and 13.2 MV, respectively, by replacing their Aℓ electrode tubes with stainless steel electrode tubes (Table 1).

Even though the HVEC's data shows that a mixture of 30% SF_6 and 79% N_2/CO_2 can reach a comparable V_TMax. at an increased total pressure (Quotation #1), all MPs using a mixture have had some difficulties in reaching or maintaining V_TMax. with tubes of 13 MV (the guaranteed voltage with the stainless steel electrode tubes) after up-grading them. By the end of 1975 all up-graded MPs using an SF_6 mixture, were in deep confusion about the tank insulating gas as Quotations #2, 3, and 4 describe.

In 1976 we had reached a point where we gathered all the scattered information about 100% SF_6 and mixtures and correlated them.

1. (F/P Max. MP << (F/P) critical = 117.5 V/torr-cm (Table 2).
   V_T Max. MP = 13 MV - 14 MV, V_T critical = 60 MV.
   Table 2 shows (F/P)Max.MP = 19.5 - 33.5 V/torr-cm for 100% SF_6,
   = 39.1 - 82.2 V/torr-cm for Mixture.
2. ΔV_T = V_TMax. w/o tube - V_TMax. with s.s. tube = 2.3 MV - 3.8 MV 100% SF_6,
   (Tabel 1.) = 1.1MV - 2 MV Mixture
   = 0.2 - 0.5 MV N_2/CO_2.

* Work supported under USERDA Contract No. FY-76-C-02-3074

3. Effect of decomposition products of SF_6 and necessity of frequent gas recirculation for their removal.

Existence of the wild discharging condition, so-called "critical gradient discharge" at a well-defined V_T, and recoverable only through vigorous gas recirculation through the activated alumina.

4. Effect of easing the field F(V/c̄m) at the terminal surface by increasing the tank diameter from MP = 18' to XTU(STU) = 2'5' which was demonstrated by HVEC. (Table I, Fig. 13.)

V_TMax.MP No Tube = 17 MV → V_TMax. XTU No Tube = 21 MV,

V_TMax.MP Tube = 13.7 MV→ V_TMax. XTU Tube = 16.5 MV.

5. Unusually strong coupling between axial activities and radial activities which requires the following remedies (with 100% SF_6 mixture):

Better electrical contacts.

Additional spark gaps, readjustment of spark gaps.

Adjustment of equipotential ring gaps.

Decoupling between tubes and column structure; double resistor chains, double corona current system.

Electron traps (electrostatic, magnetic).

Inclined or spiraled field tubes and additional tube magnets.

Applications of UHV techniques to accelerating tubes, baking heaters, additional pumps in the dead sections.

Stainless steel electrode tubes, Ti electrode tubes instead of Aℓ electrode tubes.

Elimination of elastomer gasket using metal gasket exclusively.

Careful monitoring of voltage conditioning microdischarge; NaI x-ray monitoring, vacuum monitoring.

Cautious voltage conditioning techniques; much smaller ΔV_T step (10 KV or less).

Much longer cooling-off or waiting period after discharge.

Better line voltage regulator.

6. Surface area effect (Fig. 14)

Daresbury reported about the often-observed "Surface area effect" above 10 bars for N_2/CO_2 mixture, about 3 bars for pure SF_6.

7. As Quotations #2, 3, and 4 indicate many mixture users started avoiding CO_2 and using SF_6/N_2 mixtures in 1975 and observed a peculiar wild discharge phenomenon and damaged their column members. (Yale, Heidelberg, Munich, Los Alamos, Queens) and many mixture users are planning to convert to 100% SF_6.

We gradually started to realized that we should investigate the physical phenomena that are going on in the insulating gas in detail, understand them, and handle the gas problems better.

Studying the several excellent references (see List of References) of atomic physics and utilizing their data and information suddenly opened our eyes and helped us to understand what has been going on inside the tank insulating gas. Now we can understand and explain the phenomena that have been complete mysteries to us and get clear guidance of what to do.

To our surprise we found that the original HVEC suggested gas mixture was very close to the optimum gas mixture and its ingredients are really essential and none of them is indispensable. Even the old timer like 4-1 N_2/CO_2 mixture is a meaningful mixture and some of the mysterious phenomena we observed with N_2/CO_2 mixture are now explainable using the atomic physics data.

II. A. HYPOTHETICAL V_T LIMITING DISCHARGE MECHANISM

1. Table 3 V_TMax. 100% SF_6 vs V_TMax. Mixture; Critical Gradient Discharge

Table 3 shows that three different mixtures of N_2/SF_6 (A, B, C,) triggered the so-called "critical gradient discharge" at exactly the same V_T, and immediately after that, recovered up to three different voltages V_Ts-follow-up) and never were able to go over those voltages unless the gas was recirculated through the gas dryer system (activated alumina) for a few days to a few weeks, and eventually they were able to approach V_T critical.

Another peculiar fact is $V_T$critical which triggers that wild discharge is well defined within 50 KV, and after any discharge happens below $V_T$critical we normally are able to recover up to the same V_T without any difficulties. However, once we trigger a "critical gradient discharge" we are never able to recover and we have to settle down at another well-defined V_T which we call V_T - follow-up.

The fact that mixtures A, B, and C with the same SF_6 content ended up at the same V_T critical indicates the amount of SF_6 determines $V_T$critical in the SF_6/N_2 mixture, however, the $V_T$critical in a mixture is much higher than V_TMax. for 100% SF_6.

The effect of N_2 seems to boost up the V_TMax. to 70 psia and over 70 psia there is no noticeable contribution to V_TMax. Around 100 psia the effect of the N_2 is to ease the coupling between the axial activity and the radial activity, because only mixtures C and D, the cases with over 100 psia N_2, could we reach $V_T$critical without help from radiation sources. Also ending up with three different V_Ts follow-up indicates that V_T-follow-up is a function of N_2 pressure in the SF_6/N_2 mixtures. We can see the important role of CO_2 as is demonstrated in the large differences between V_TMax.-30% SF_6+N_2/CO_2 and V_TMax.-SF_6/N_2.

In summing up, Table 2 suggests that we need more SF_6 and CO_2 for a higher V_T and it may be necessary to maintain 100 psia N_2 for easing the coupling between the axial and radial activities.

2. Table 4. ε_k Electron Characteristic Energies in Gas Mixtures.

Electron Characteristic Energy $\varepsilon_k = D/\mu$(D= Electron Diffusion Coefficent)
(μ = Electron Mobility

Average Electron Energy $\bar{\varepsilon} = \frac{3}{2}\varepsilon_k = \frac{3}{2} D/\mu$(Maxwellian Distribution)

Table 4 shows the electron characteristic energies ε_k in N_2 gas from Fig. 27 corresponding to (F/p) values for $V_T$critical and V_TMax.-follow-up, and indicates $\Delta\varepsilon_k = \varepsilon_{k,N_2}$ - follow-up = 0.4 eV = constant, which later we found corresponds to the $(SF_6^-)^*$ excitation level energy $\doteq$ SF_5^- + F decay channel energy.

A diagrammatic expression in Fig. 1 shows that if we add a peak value of $E(\Sigma qv,N_2)$ (N_2 vibrational excitation energy) = 2.5 eV to ε_{k,N_2} we can get another constant value 4.3 eV in all mixtures A, B, and C, equal to the threshold energy for $SF_6 + e \rightarrow SF_4 + F + F^- = 6$ eV at the peak of F^- production from SF_6 (Fig. 16). This threshold energy, 4.3 eV, is the most important threshold determining the critical F^- concentration in the tank insulating gas.

These facts indicate that when F_T is almost up to V_TMax., the major portion of the N_2 molecules are excited to the 2.5-eV excitation level and stay up there without help of deactivation from CO_2. The electrons are making superelastic collisions with N_2^* and its ε_k is boosted by 2.5 eV and reaches the threshold value (4.3 eV) and triggers the "critical gradient discharge."

After the "critical gradient discharge" many $(SF_6^-)^*$ are excited to 0.43 eV (SF_5^- + F predissociation level) and it only requires 3.9 eV to reach the "critical gradient discharge" condition.

Existence of high-pressure (70-100 psia) N_2 in the gas mixture helps to slow down, or moderate the electron energy, determine its characteristic energy ε_k, and to prevent it reaching the dangerous threshold value of 4.3 eV in the SF_6 case. And Table 3 indicates the necessity of having CO_2 in the gas mixture to thermalize the electron energy and enhance the thermal electron capture of SF_6 to kill the free electron population in the gas. In addition, it is necessary to have CO_2 (at least $N_2 = CO_2 = 4:1$) to deactivate the N_2^*, as I will describe later.

3. Table 5. V_TMax. 100% SF_6 vs V_TMax. Mixture; Critical Partial Pressure CF_4.

a. CF_4 Incident

After analyzing the experience with mixtures A, B, C, and D, and realizing we have to add more SF_6, besides more CO_2, we started adding to the mixture D a few SF_6 cylinders out of 18 cylinders supplied from Air Products. Suddenly the accelerator started discharging randomly, exactly the same as the so-called "critical gradient 18 cylinders from Air Products we stopped adding any more SF_6 and started recirculating the gas through the activated alumina dryers. After overnight recirculation V_T completely recovered to the V_TMax., and we decided to finish adding all 18 cylinders of SF_6. Immediately we realized that we were in much deeper trouble after adding the rest of the SF_6 and V_T eventually recovered but it required 3 weeks of continuous recirculation.

b. The SF_6 Decomposition Products and CF_4

This incident told us that 18 SF_6 cylinders supplied from Air Products contained some ingredients which poisoned the insulating property of the gas and which maybe the same as the poisonous SF_6 decomposition products or produce some substance in the tank insulating gas. Since that incident we have had two occasions to add more SF_6 which was supplied from Allied Chemical and no such phenomenon as the above has been observed. Through chemical analyses done by both suppliers at our request to look for any unusual impurity, especially any fluorine-related ingredients such as F_2 or HF, we found that there was a distinct difference in both companys' CF_4 contents; Air Products Average CF_4 = 409 ± 90 ppm (by weight), Mixture D, 18 cylinders: Allied Chemical Average CF_4 = 105 ± 9 ppm (by weight), Mixture E, 48 cylinders and 136 ± 39 ppm (by weight) Mixture G, 36 cylinders. Sure enough, Table 10 indicates $e + CF_4 + F^-$, like all other halogenated mixtures F^- is the most dominant negative ion, its threshold is near thermal, its peak energy is 1.3 eV, and its cross section is to $10^{-16}cm^2$.

Although ASTM Designation D2472-71 allows SF_6 to contain 500 ppm (by weight) CF_4 in the industrial grade, we should limit CF_4 content to a much lower value like 100 ppm (by weight) which depends on how much total SF_6 we need in the tank gas.

Utilizing the data we have gathered we tentatively estimate the "critical partial pressure" of CF_4 in our MP tank gas, as 0.016 psia--0.013 psia and choose 0.015 psia as the permissible limit (Table 5 and Table 17).

c. Removal of CF_4

One peculiar recovery pattern we found during the gas recirculation was it repeatedly took longer than a day after putting newly activated alumina into service before seeing some improvements in V_TMax. This proves that the removal process of the poison requires some moisture to make alumina effective, in other words, the suspicious poison like a free fluorine, cannot be trapped by the dry activated alumina unless "A" is changed into HF by reacting with H_2O. This also suggests that the original poison was CF_4 and produced F^- easily by slow electron dissociative capture reactions. Now we are almost certain that the most troublesome decomposition product from SF_6 must be F^-.

d. Effect of Additional CO_2 and SF_6.

Mixture D to Mixture E involved the addition of 10 psia of SF_6 and 17 psia of CO_2, which definitely helped to push V_TMax. up to 13.0 MV without triggering any sign of the "critical gradient discharge." We believe V_T = 13.0 MV is a few MV lower than $V_T$critical, and it is a good practice to stay a few MV lower than $V_T$critical. CO_2 looks like a good additional or complementing moderator to N_2, especially to thermalize the electrons and enhance SF_6's thermal electron capture like the moderator in the thermal neutron fission reactor.

4. Table 6. V_TMax. - follow-up in N_2/CO_2 Mixture H: Parabola Seven Sparks Cycle.

Table 6 shows a typical N_2/CO_2 Mixture H: its V_TMax.-tube was 11.75 MV after initial vigorous voltage conditioning and we ran experiments at V_T = 11.5 MV rather comfortably. Figure 2 shows V_T = 11.75 MV is the real N_2/CO_2 gas limit, i.e., $E(\Sigma q_V, N_2)$ N_2 excitation level energy 2.5 eV, Fig. 3) + electron characteristic energy $\varepsilon_k N_2/CO_2$ of 1.4 eV = 3.9 eV = the threshold energy for $e + CO_2 = CO + O^-$ (Figs. 4 and 5).

This evidence suggests that O^- is the V_T-limiting poison in the N_2/CO_2 mixture, although it is in much less severe than the F^- in SF_6.

During initial voltage conditioning using the vigorous continuous discharging technique, we observed repeatedly that the follow-up discharging voltages followed the fixed pattern which we named the "parabola seven sparks cycle," and now we can fully understand it, using the information about four distinct vibrational excitation levels of CO_2 (Fig. 6) and displayed in Fig. 2.

5. A Hypothetical Discharge Mechanism in the Electronegative Gas.

Figure 7 is familiar plot of F/P) as a function of pressure times distance for SF_6 and clearly indicates (F/P) critical = 11.7 f v/torr-cm, and Fig. 8 is another way to determine (F/P) critical using $\frac{\alpha^-}{P}$ plot as a function of (F/P) and gives us the same value of (F/PO critical 117 V/torr-cm. In general, below (F/P) critical no sparking should be possible, however, as we know well, the practical values of F/P for the MP's are well below 117.5 V/torr-cm as Table II shows.

MP	(F/P)Max. No Tube, Tube	= 19.5 - 33.5 V/torr-cm with 100% SF_6
	(F/P)Max. No Tube, Tube	= 39.1 - 82.2 V/torr-cm with SF_6 mixture.
XTU	(F/P)Max. No Tube	= 25.1 V/torr-cm with 100% SF_6
	(F/P)Max. Tube	=27.0 V/torr-cm with 100% SF_6.

We should remember that HVEC XTU data have rather low (F/P) values, even though V_TMax-no tube = 21.0 MV and V_TMax.-tube = 16.5 MV, and we believe that when using 100% SF_6 it is very difficult to reach a higher (F/P) value.

An additional feature which we should not miss in Fig. 7 is that all five different measurements went off from the asymptote of (F/P) critical = 117.5 V/torr-cm and discharged at much lower (F/P) values for higher pd values. This is possibly due to the non-uniform field, but it happens only in the SF_6 case.

From Fig. 9 we learn that near (F/P) critical = 117.5 V/torr-cm, ε_k is over 5 eV and definitely over the threshold energy for the SF_4+F+F^- dissociation reaction, and is in the middle of the 6-eV peak of the F^- production cross section for SF_6 (Fig. 16).

The flatness of the ε_k curve between (F/P) = 110 to 210 V/torr-cm indicates that SF_6 is an excellent moderator for electrons in this (F/P) range where we are never able to operate, and it might be rather useless as a moderator for the range where our MP's (F/P) values are.

Another point to be noted here, is we found using the same experimental set-up, anomalous diffusion of electrons near (F/P) critical due to the non-uniformity of the field and this is only observed in the SF_6 case.

These facts suggest that the combination of the existence of some concentration of negative ions like F^- or O^- and non-uniformity of the field, such as the concentric cylindrical structure of our accelerator field, or some radiation counter field, creates an anomalous condition which triggers the "critical gradient discharge" phenomenon.

Figure 10 shows the ionic mobilities of various negative ions in SF_6, and Fig. 11 shows the ionic mobilities of three negative oxygen ions in O_2, and gives us an idea about the ionic mobilities of heavy negative ions such as SF_6^- and SF_5^-. These are in the 0.5 - 0.7 $cm^2V^-S^-$ range and the ionic mobilities of light negative ions like O^- is 3.5 - 5 $cm^2V^{-1}S^{-1}$.

Table 7 collects the electron affinity values for possible negative ions in the tank insulating gas mixture. $E_a(F^-)$ = 3.448 eV and $E_a(O^-)$ = 1.465 eV indicate that F^- and O^- both have a rather high electron affinity.

From Table 10 we can extract some common characteristics of F^- and O^-. Both have a rather high electron affinity, a rather high ionic mobility, and both are negative ions.

Now if we have a critical concentration of negative ions which have a rather high mobility and a rather high electron affinity, which means stable negative ions such as F^- or O^-, in the non-uniform field of the concentric cylindrical structure of our accelerator [Fig. 13, Formula (1)], as Fig. 12 describes in a uniform field, there will be a strong field enhancement by the negative space charge made of F^- or O^- near the positive electrode, i.e., the accelerator terminal where the field is the highest. Even when V_T is creating a much lower (F/P) than (F/P) critical = 117.5 V torr-cm for SF_6, the enhanced local field easily reaches (F/P) critical, stimulates more F^- production, aggravates the situation and ends up in the situation which we have been calling the "critical gradient discharge" phenomenon.

We believe that this mechanism is the main contributor for all the puzzling mysteries we described in Section I, such as the so-called "surface area effect" (Fig. 14) or the "anomalous" phenomena reported by atomic physicists in Figs. 7 and 9, and an unusually strong coupling between the axial and radial activities besides the "critical gradient discharge."

III. FURTHER DETAILED ATOMIC PHYSICS STUDIES

A. SF_6 Electron Capture Processes.

The high dielectric strength of SF_6 is believed to be primarily due to its ability to capture free electrons to form negative ions, and as a 'scavenger' for slow electrons. Its rate constant for attachment has been reported as $2.2 \times 10^{-7} cm^3/sec$[1] and independent of temperature (293-523 K) and pressure (0.1-1.5 torr) using a flowing afterglow technique buffered with helium. But in this pressure range, this could only be because collision stabilization occurred so fast that almost all the SF_6^- ions initially formed were stabilized before autodetachment.

But Spence and Schulz[2] found no variation with temperature (up to 1200 K) of the rate of attachment of slow electrons to SF_6 (Fig. 17) which indicates

that the cross section for $(SF_6^-)^*$ formation does not depend on the initial vibrational state of SF_6.

The experiments which have been performed on the attachment of electrons to SF_6 can best be explained by the mechanisms represented diagrammatically by (2).

$$\begin{array}{ccccc} & & \overset{k_1}{\rightleftarrows} & & \\ SF_6 + e & & (SF_6^-)^* & \overset{k_3}{\rightarrow} & SF_6^- \\ & & \underset{t_1}{\leftarrow} \quad \underset{t_2}{\downarrow} \; \underset{k_2}{\downarrow} & & \\ & & SF_6^- + F & & \end{array} \qquad (2)$$

Electrons may associate with SF_6 to form a metastable complex $(SF_6^-)^*$. The rate constant for this association is labeled k_1, while the lifetime of the association complex against autodetachment is t_1. However, during this time the association complex may be collisionally stabilized to SF_6^-. The rate constant of collisions of the $(SF_6^-)^*$ with the background gas is designated k_3. Alternatively, the $(SF_6^-)^*$ complex may decay to form SF_5^- and F. The rate constant for this dissociative attachment channel is k_2, while the lifetime of the $(SF_6^-)^*$ against predissociation into SF_5^- and F is t_2.

In the flowing afterglow experiments[1] the primary reaction sequence results in the formation of SF_6^-. The lowest pressure attainable in the Fehsenfeld experiment is of the order of 0.1 torr. It seems probable that if the pressure were dropped, eventually the production of SF_6^- would exhibit three-body kinetics with a rate constant $k_1k_3t_1$. However, only binary kinetics were observed. Thus we conclude that the reaction is saturated, i.e., that the lifetime of the $(SF_6^-)^*$ against autodetachment is much longer than the longest mean free time between stabilizing collision of the $(SF_6^-)^*$ with the background gas or $t_1 >> (k_3[M])^{-1}$ where [M] is the concentration of the buffer gas. Consequently the measured rate constant for the production of SF_6^- is that expected for a saturated three-body reaction k_1. The flowing afterglow results indicate that argon and helium are equally efficient at producing stabilization.

Davis and Nelson[3] (Table 10) find essentially some stabilization efficiency for the attachment in He, Ar, H_2, N_2, CO, CF_4, C_2H_2, CF_6, C_4H_{10}, CH_4, CO_2, and C_2H_4. Under these conditions, the formation of SF_6^- appears to occur as a two-body process because the chance of stabilization is effectively unity, no matter what atoms or molecules act as third bodies.

It is not surprising that a complex ion such as SF_6^- should have a long lifetime against autodetachment. The energy brought in by the captured electron will be rapidly distributed among a number of internal modes of motion, so that on the average, a considerable time will elapse before autodetachment.

Herzenberg[4] points out that a theoretical maximum for electron attachment at near zero energy to polyatomic molecules exists, and that this maximum is insensitive to the initial vibrational state of the molecule. The theoretical maximum attachment cross section calculated by Herzenberg is of the order of that observed in SF_6 and CO_4 at room temperatures (Fig. 17). These molecules show no temperature dependence. The results obtained by Spence and Schulz[2] (the energy integrated cross section for $SF_6 = 2.5 \times 10^{-15} cm^2$ eV) and the earlier data of Wentworth et al[5] appear to verify this aspect of Herzenberg's theory.

The attachment rate $k_1 = 2.2 \times 10^{-7} cm^3/sec$ yields an "average" capture cross section of $\bar{\sigma}_1 = (2.2 \times 10^{-7}/\bar{v}) cm^2$ where $\bar{v}$ is the average electron velocity, and gives a capture cross section for thermal energy electrons of the order of $2 \times 10^{-14} cm^2$.

The main features of negative ion production in SF_6 were established in the experiments of Hickam and Fox[6] in 1956 (Fig. 15). They used both a total ion

collector and a 90° sectored-field mass spectrometer. Both the electrode systems used to produce the electron beam made it possible to use the retarding potential difference method to obtain results for electrons with energy defined to about 0.1 eV. Figure 15 shows the results they obtained with the mass spectrometer for the variation with electron energy of the currents of SF_6^- and SF_5^- ions. The variation of the total ion current agreed well with that observed in the total ion collection instrument.

Figure 15A gives a comparison between the shape of the SF_6^- current peak and the energy distribution of the electrons obtained from a retarding potential analysis. It can be seen that the shapes agree very well although the peak of the SF_6^- production is shifted by about 0.03 eV to a higher energy than that of the electron energy distribution.

Figure 15 shows the capture process occurs at less than 0.1 eV (0.03 eV from Fig. 15A) and only over an energy range estimated to be not larger than 0.05 eV. The estimated maximum cross section for the formation of SF_6^- based on a capture energy width of 0.05 eV is at least $10^{-15}cm^2$. Large attachment cross sections might be obtained by a process in which the negative ion is stabilized by collisions before dissociation can occur. The fact that the SF_6^- ion has not resulted from collision stabilization is evidenced by the linearity, with pressure, of the SF_6^- ion current at the low pressures used in the mass spectrometer (less than 10^{-5} torr). Furthermore, if the SF_6^- ion is metastable, it must have a half-life of at least a microsecond to have been detected in the mass spectrometer. This is a very long time compared to the vibration time of the molecule. Possibly, the explanation for such a large cross section is associated with the formation of the SF_6^- ion in an excited state in such a manner that the energy is distributed among several modes of vibration and/or rotation. This would conceivably allow the ion to exist in an excited state for a long time before the energy could be concentrated in a mode which would result in either the ejection of the electron or a dissociation process.

The long lifetime means that the capture process will only occur if the electrons have a sharply defined energy--the resonance width will be very small (for a lifetime of 10 μs the width is only 6×10^{-11} eV).

From the total ion-collection measurements, the cross section for capture of an electron by SF_6 at the peak must be of the order of $10^{-15}cm^2$. This is consistent with later experiments carried out by Rapp and Briglia[7] (1965) and by Buchel'nikova[8] (1958) who obtained peak values of 2.1×10^{-18} and $5.1 \times 10^{-16}cm^2$, respectively. The resonance peak is so sharp that the results obtained depend very much on the electron-energy distribution and this is limited by energy resolution of the electron source needs. This provides a convenient means of determining this distribution in certain circumstances.

It seems well established that stabilization is almost certain at pressures of 0.1 torr in all gases. Since the time between collisions is of the order of 1 μs, this is consistent with the lifetimes observed in the time-of-flight experiments.

The lifetime of the SF_6^- ion initially formed by capture of a slow electron has been investigated by time-of-flight and by ion cyclotron resonance techniques. Using the former technique, Edelson, Griffiths, and McAfee[9] obtained a lifetime of 10 μs, while a little later, Compton, Christophorou, Hurst, and Reinhardt[10] obtained a value of 25 μs by much the same technique.

The lifetime of $(SF_6^-)^*$ against autodetachment is quite long. In the Fehsenfeld[1] experiment the upper limit for t_1 is limited by the lowest pressure at which it is feasible to operate the experiment.

If one assumes that the collisions between the $(SF_6^-)^*$ and the buffer gas occur at the Longevin rate then

$$k_3 = 2\pi e(\alpha/\mu)^{1/2} = 2.34 \times 10^{-9}(\alpha/\mu)^{1/2} ,$$

where α is the polarizability in cubic angstroms of the buffer gas atom and μ is the reduced mass in atomic units (O=16). This yields $k_3 = 5.39 \times 10^{-10} cm^3/sec$ for a helium buffer and $k_3 = 5.35 \times 10^{-10} cm^3/sec$ for an argon buffer. Using these rates in the flowing afterglow the mean lifetime between collisions at the lowest pressure is 0.5 μsec. Since there was no change in the results as a function of pressure we conclude that $t_1 >> 5$ μsec. This result is in agreement with various low-pressure experiments which have measured t_1. Hickam and Fox[6] estimated a lower limit for this lifetime of one microsecond.

A very different result was obtained by Henis and Mabie[11] (1977) who measured the ion cyclotron resonance line width and derived a lifetime ≅ 500 μs. A probable explanation of the large discrepancy between these results is forthcoming from the experiments of Odom, Smith, and Futrell[12] (1974). These authors used the ion cyclotron resonance technique in a different way. SF_6^- and SF_5^- ions, and free scattered electrons, produced from a pulsed electron beam passing through SF_6, drifted with the same speed under the action of crossed electric and magnetic fields, towards a collector plate. With an SF_6 pressure of 3×10^{-7} torr many of the electrons were captured after a time interval of 400 μs. Free electrons could be removed selectively from the mixture by application of an rf electric field of appropriate frequency to one of the trapping plates.

The number of heavy ions remaining at any time was obtained by measuring the current to the collector plate immediately after a pulse of an rf field had been applied to remove the electrons. Going to autodetachment, the SF_6^- current so measured should decrease as the duration T of the electron ejection pulse is increased. Figure 18 shows that this is indeed so. However, if the ions possessed a single definite lifetime t_1 the slope of the plot in Fig. 18 should be a straight line with slope equal to 1/t. In fact, the apparent lifetime at any time T determined by the slope of the target to the curve in Fig. 18 at T, increased with T as shown in Fig. 19. It follows that the lifetime measured in any particular experiment will depend on the time since formation of the SF_6^-.

Although these preliminary experiments do not give information about the lifetime of SF_6^- within tens of μs of formation, they do suggest that the SF_6^- ions may be produced in a number of different initial states which differ in lifetime over a wide range.

The dissociative attachment process leading to the formation of SF_5^- maximizes at 0.2 eV and then decreases to zero at approximately 1.5 eV (Fig. 15).

The SF_6^- and SF_5^- curves in Fig. 15 seem to indicate possible errors that may exist in electron attachment cross-section measurements. Here it is noted that the ratio of the SF_6^- peak to the SF_5^- peak is of the order of 25 to 1. If instead of using the RPD method the mass spectrometer is operated in the conventional manner using low electron currents, a ratio of 5 to 1 is found. Using much higher electron currents, Ahearn and Hannay[13] report a one-to-one ratio for SF_6^- to SF_5^-. In the work on SF_6^- and SF_5^-, it is rather obvious that if one used a broad electron energy distribution the number of electrons in an energy range capable of undergoing capture to form SF_6^- is quite different from that for SF_5^-.

In fact, the sharpness of the SF_6^- curve indicates that electrons, even more nearly monoenergetic than those used for Fig. 15, are necessary if the ratio of SF_6^- to SF_5^- is to be observed directly. Under such conditions the indicated SF_6^- to SF_5^- ratio may be of the order of 100 to 1 or larger. It is calculated

from the measurements that the maximum cross section for the formation of SF_5^- is of the order of $10^{-17}cm^2$. A cross section of this order of magnitude is quite reasonable for a dissociative attachment process.

As we mentioned Fehsenfeld[1] found that the attachment has a rate constant of $2.2 \times 10^{-7}cm^3/sec$, independent of temperature and pressure. The primary reaction product over the measured range of temperatures (293-523 K) is SF_6^-. However, the rate of production of SF_5^- increases rapidly with temperature. Figure 20 shows the ratio of SF_5^- to SF_6^- as a function of temperature. At zero degrees centigrade, the SF_5^- signal is almost lost in the noise which is over four orders of magnitude below the SF_6^- signal. However, at 200°C the ratio of SF_5^- to SF_6^- is about equal to 25. There was a slight amount of F^- observed at 500 K with an intensity roughly 10% that of SF_5^-.

This same large variation in the ratio of SF_5^- to SF_6^- has also been observed by Chen and Chantry.[14] However, at all temperatures the ratio of SF_5^- to SF_6^- observed by Chen and Chantry is about two orders of magnitude larger than the results obtained by the flowing afterglow. Two values obtained by Chen and Chantry for this ratio with the SF_6 at 27 and 106°C are shown as the closed circles in Fig. 20. In their experiment a low-energy beam of electrons was impinged in SF_6 at low pressure and the negative ions produced were collected and mass analyzed. In the flowing afterglow experiments the negative ions are subject to thousands of collisions with a buffer gas prior to their sampling. This is a good example of the importance of stabilizing collisions. They find that on heating the gas there is a rapid decrease in the SF_6^- signal, with near zero electron energy, together with a corresponding increase in the energetically coincident SF_6^-. Above 600 K they observe an F^- peak which increases with temperature and is also energetically coincident with the SF_6^- and SF_5^- peaks. They further find an SF_5^- dissociative attachment peak at 0.38 eV which is insensitive to the target gas temperature.

Fehsenfeld[1] showed the data in the form of an Arrhenius plot in Fig. 21. This plot yields a straight line which would indicate that the production of SF_5^- from SF_6 has an activation energy of 0.43 eV.

The interpretation of this is that there is a curve crossing between a dissociating state of $(SF_6^-)^*$ which leads to SF_5^- and F, and a vibrationally excited state of SF_6 lying about 0.43 eV above the vibrational ground state of SF_6. It would be expected that the lifetime of this $(SF_6^-)^*$ against predissociation would be of the order of a vibrational time ($\approx 10^{-13}$sec). The very short lifetime of this $(SF_6^-)^*$ state once formed is compatible with the observation that the ratio of SF_5^- to SF_6^- in the flowing afterglow is independent of pressure. He finds that the SF_5^-/SF_6^- ratio is constant up to pressures where the mean molecule-molecule collision time is 3×10^{-8}sec. This is interpreted as SF_5^- arising from a repulsive $(SF_6^-)^*$ state, i.e., straightforward dissociative attachment via the 0.4-eV repulsive state. Thus the lifetime of this predissociating state, t_2, is much shorter than the shortest mean free time of the $(SF_6^-)^*$ state for collisions with the background gas, i.e., $t_2 >> (k_3[M]max)^{-1}$. In the flowing afterglow experiment this would indicate that $t_2 >> 3 \times 10^{-8}$ sec. These results are compatible with those of Edelson et al.[9] Edelson specifically noted that the lifetime of the states of $(SF_6^-)^*$ leading to dissociative attachment were so short they were not observed.

The results which have thus far been observed can be correlated by the potential energy curves for the formation of SF_6 from SF_5 and F, and SF_6^- from SF_5^- and F, as a function of the internuclear separation.

The electron affinities of SF_6^- and SF_5^- ((Table 7) values of 0.54 eV and 3.2 eV) have been measured by Kay and Page [15] to be 1.4 and 3.6 eV,

respectively. Kay and Page have also computed the bond dissociation energy $D_o(SF_5\text{-}F)$ to be 3.7 eV. This means that the dissociation energy of SF_6^-, $D_o(SF_5^- \text{-} F)$ is 1.5 eV. There are a variety of potential curves corresponding to the possible paths to reach SF_5^- and F. The curve which produces the ground-state configuration of SF_6^- is labeled (a) in Fig. 22. If $D_o(SF_6^-) > E_a(SF_6^-)$, there can be excited states of SF_6^- [labeled (b)], which are bonding and cut the SF_6 potential curve. Finally, there are non-bonding repulsive curves like curve (c) which cut the excited vibrational levels of SF_6.

The activation energy of 0.43 eV for the production of SF_5^- in the flowing afterglow suggests that the lowest dissociating level of $(SF_6^-)^*$ cuts through the SF_6 curve at about the fourth or fifth vibrational level. Hickam and Fox[6] (Fig. 15) found the production of SF_5^- peaked at about 0.2 eV. This production is probably due to a vertical transition from the low vibrational states of SF_6 to the lowest predissociating level. If all the SF_6 used in the experiment were in the ground vibrational state one would expect that the peak production of SF_5^- in the Hickam and Fox experiment would have occurred at an energy slightly greater than 0.43 eV. The lower value of 0.2 eV indicates perhaps that strong enough transitions occur from the small quantities of vibrationally excited SF_6 to mask the ground-state transition.

The following picture seems to give a qualitative explanation for the interaction of SF_6 with electrons. At low electron energy, electrons rapidly associate into the form $(SF_6^-)^*$ which is stable against autodetachment for tens of microseconds. Collision with the background gas stabilizes the complex. For most buffer gases the stabilization probability is about the same. Electrons may also react with electrons by dissociative attachment. This process has a small activation energy. Consequently the rate of production of SF_5^- increases rapidly with increasing energy. The lifetime of the predissociating state of $(SF_6^-)^*$ is much shorter than 3×10^{-8} sec.

Spence and Schulz[2] (Fig. 17) show that SF_6 does not exhibit a temperature variation, the cross section remaining constant up to 1200 K. This indicates that the cross section for $(SF_6^-)^*$ formation does not depend on the initial vibrational state of SF_6. It should be noted that at least three species of negative ions formed near zero energy (SF_6^-, SF_5^-, F^-) depend strongly on temperature (Figs. 20, 21), but this is not inconsistent with the present observations if one postulates that decay channels on the compound state depend on temperature. In other words, we postulate that there is one common electronic $(SF_6^-)^*$ complex initially formed and that the branching ratios for dissociation of this complex depend upon the thermal energy initially in the SF_6 molecule.

Spence and Schulz[2] have concluded that the dissociation limit for $SF_5^- + F$) lies only about 0.1 eV above the v=0 state of the neutral molecule (Fehsenfeld[1]), and that for ($SF_5 + F^-$) about 0.25 eV above SF_6(v=0) (Fehsenfeld[1], Barry and Reinmann[16]). In a complex molecule such as SF_6, discrete vibrational levels of the negative ion $(SF_6^-)^*$ with a long lifetime can exist above the dissociation limits ($SF_5 + F$ and $SF_5^- + F$). The compound state $(SF_6^-)^*$ eventually finds an energetically possible pathway for dissociation and it decays into fragments which are observed experimentally (Ahearn and Hannay[13], Chen and Chantry[14]).

Tiernan, Hughes and Lifshitz[17] reported an excited state of $SF_6^- \cong 0.5$ eV above the ground SF_6^- state. Our observation described in Table 4 and Fig. 1 indicates the existence of 0.4-eV excited state in $(SF_6^-)^*$.

Naidu and Prasad[18] reported that the most abundant ions observed are SF_6^- at $(F/P) < 60$ and SF_5^- and F^- at $(F/P) > 80$. In addition to these, Patterson[19] has observed clusters of $SF_6^-(SF_6)$, $SF_6^-(SF_6)_2$ and $SF_5^-(SF_6)$ over the range $5 < (F/P) < 40$ (Fig. 10).

Figure 16[13] shows the variation of the F^- negative ion current with electron energy. At the lowest energy (0.6 eV) F^- current appears to be rising rapidly with decreasing energy. The current falls to a minimum value at 2.6 eV and is followed by a series of three well-defined current onsets leading to maxima. At no point does the F^- current fall to zero, as contrasted with SF_6^- and SF_5^- currents, and this special feature of F^- current might be a major contribution to the "critical gradient discharge."

It is possible that

$$e + SF_6 \rightarrow (SF_6^-)^* \rightarrow SF_5 + F^-,$$

$$e + SF_6 \rightarrow (SF_6^-)^* \rightarrow SF_4 + F + F^-, \text{ etc.}$$

This may take place giving differences in critical potentials of about 4 eV, which is approximately the energy necessary to remove successive F atoms since the dissociation energy for $SF_6 \rightarrow S + 6F$ is about 22.4 eV. The multiple peaks are about 4 and 3 eV, and the kinetic energy term may provide better agreement when known.

Buchel'nikova[8] reported that the ratio of the maximum SF_6^- current to the maximum SF_5^- current is approximately 25. The F^- current is approximately 100 times smaller than the SF_5^- current (Hickam and Fox[6], Ahearn and Hannay[13]).

B. CF_4 Electron Capture Process (Spence and Schulz[2], Naidu and Prasad[20], Craggs and McDowell[21]).

Negative ion formation by electron attachment at low energies in the halogenated methane (e.g., CF_4) studied is of a different nature than in SF_6. In SF_6, the dominant attachment process at zero energy is the formation of a long-lived $(SF_6^-)^*$ ion which may then fragment according to the reaction scheme proposed above. For the group of halogenated methanes studied, no parent negative ion has ever been observed. The mechanism involved is that of dissociative attachment, in which the negative ion formed on electron attachment, is in a repulsive state which promptly dissociates. Dissociative attachment cross sections have been shown to be usually very dependent on the initial vibrational state of the neutral molecule.

All available data on negative ion formation by electron attachment of CF_4 have been gathered as follows (Table 10):

Reaction process = $e + CF_4 \rightarrow CF_3 + F^-$, F^- only one dominant negative ion found.

Threshold energy = near thermal.

Peak energy = 1.3 eV.

Cross section = $10^{-16} cm^2$.

We now believe that the following are the important factors which we have to take into account when designing the appropriate tank-insulating gas mixture. The gas should be able to prevent the electron avalanche and eventual gas discharge by decreasing the free-electron population as quickly as possible, by utilizing SF_6's large thermal electron caputure cross section.

For enhancing its electron capture process we should add some gas molecules (such as CO_2, H_2O) to thermalize the electrons and maintain the electron energy around the thermal energy as long as possible to give the SF_6 molecule more chance to fulfill its job as an electron "scavenger," before the electron is accelerated to higher energies and opens the other decay channels to destroy the SF_6 molecule and to produce SF_5^- or F^-. We should realize that, once we

accelerate the electrons to a higher energy SF_6 becomes not only almost useless, but also dangerous. The mixing with an efficient slowing-down media, such as high-pressure N_2, to limit the "electron characteristic energy ε_k" below the threshold energy for the poisonous F^- production cross-section peak (especially 6-eV peak, its threshold energy = 4.3 eV) will be very important.

Another feature which we have discovered recently and will be discussed in a later section and could be the most important aspect for designing the tank insulating gas for the much larger future machines is the deactivation or de-excitation through the transfer of energy between the vibrational excitation, rotational excitation, and translational motion. For example, it is desirable to deactivate the already excited molecules such as N_2, CO_2, SF_6 as quickly as possible to prevent reaching the critical gradient too early or to create many unnecessary follow-up discharges which destroy the gas molecules or the accelerator components. For this purpose, later, we will discuss that among SF_6, SF_6 and CO_2, N_2 and CO_2, CO_2 and H_2O, etc., the mutual vibrational deexcitation is the best way to shorten the relaxation time and return them to their ground states quickly and maintain them in their ground states. As we mentioned repeatedly, $(SF_6^- + F)$, $(SF_5 + F^-)$ dissociative attachment channel cross sections are strongly dependent upon the initial thermal energy in the SF_6 molecule.

The two poisonous negative ions, F^- and O^-, both have a rather high ionic mobility. To slow down the ionic mobility of these dangerous negative ions, utilizing charge exchange reactions or ion clustering by some polar substances like H_2O or CO_2, is also important.

Finally, we should investigate all possible negative ion productions out of all ingredients and all possible ionic reactions and avoid any unnecessary additional complications.

Taking even the above-mentioned factors into consideration, we should realize that the pure SF_6 would never be the best tank insulating gas available, and we have to investigate further the atomic physics data of the gas molecules which might become the important ingredients in our future insulating gas mixture.

C. Momentum Transfer Cross Section and "Electron Characteristic Energy ε_k".

As we have discussed briefly in II, Table 3 shows that in the cases of mixtures (SF_6/N_2) A, B, and C V_TMax.-mixture is always higher than V_TMax.-100% SF_6 using the same pressure of SF_6. Table 2 shows the (F/P)Max.-mixture is much higher than the (F/P)Max.-100% SF_6 and using the same pressure of SF_6 we could get much closer to the (F/P)critical = 117.5 V cm^{-1} $torr^{-1}$, especially, the fact with over 100 psia N_2 (Mixture C and D) we could reach $V_T$critical without any radiation source indicates N_2 over 100 psia has an effect which eases the strong coupling between the axial activities and the radial activities. However, increasing the pressure of N_2 from 72 psia (Mixture A) to 114 psia (Mixture C) did not contribute any toward improving V_TMax., and this suggests the amount of SF_6 becomes a determining factor of V_TMax., once N_2 pressure exceeds 70 psia (the minimum necessary amount). If we want to reach V_TMax.-tube of 13 MV, we should have over 50 psia of SF_6 (minimum necessary amount), although we could reach only V_TMax.-no tube of 10 MV with 100%. 50 psia, SF_6 (HVEC data). Also, in the Mixtures A, B, and C, the V_TMax.-SF_6/N_2 are much lower than V_TMax.-30%SF_6+N_2/CO_2 (HVEC). This suggests some important roles of CO_2 in the gas mixture. In fact, since we have mixed with over 20 psia CO_2 we have never observed any signs of the so-called "critical gradient discharge." Figures 24 and 25 show that N_2 has a prominent peak around 2.5 eV in its momentum transfer cross section which is made of at least eight vibrational excitation modes [Fig. 41, Schulz (1964)].

Figures 23, 24, and 26 show that CO_2 has much higher momentum transfer cross section towards zero electron energy than N_2, and H_2O has one order of magnitude higher cross section than CO_2. Table 9 shows that CO_2 has a much higher average fractional energy loss (λ) than N_2. In summary, H_2O and CO_2 are excellent thermalizing media for low-energy electrons and N_2 is an excellent slowing-down medium for intermediate energy electrons (0.6 - 4 eV) and $N_2/CO_2/H_2O$ makes a good complementary combination for the electron moderator. Figures 27, 28, 29, 30, and 9 show the "Electron Characteristic Energy" ε_k in N_2, CO_2, H_2O for the ranges [2 < (F/P) < 30 V/torr cm], H_2O[10 < (F/P) < 60 V/torr cm], and SF_6[100 < (F/P) < 210 V/torr cm], respectively.

$$\text{"Electron characteristic energy"}\ \varepsilon_k = D/\mu \quad (D = \text{electron diffusion coefficient}) \quad (\mu = \text{Electron mobility}) \qquad (3)$$

$$\text{"Average electron energy"}\ \bar{\varepsilon} = \frac{3}{2}\varepsilon_k = \frac{3}{2}D/\mu \ \text{(Maxwellian distribution)}$$

This is a convenient parameter to use, to figure out the gross behavior of electrons in the gas, as we have already demonstrated in Figs. 1, 2, and Table 4, for describing the energy relationships of the various discharge phenomena in our tank insulating gas mixtures

As we expect, Fig. 27 shows that N_2 is an excellent medium to limit electron energy below the thresholds for some poisonous negative ion production (for example: 4.3 eV for F^- out of SF_6, 3.9 eV for O^- out of CO_2). If we have 100 psia of N_2 in our gas mixture, ε_k in the N_2 alone will be 1.8 eV for V_T = 18 MV (MP) and V_T = 25 MV (XTU), and it will be a safe choice even when many N_2 molecules are excited to their 2.5-eV vibrational excitation level. Figures 28, 29, and 30 show that CO_2 and H_2O are both excellent for thermalizing and maintaining the electron energy near thermal where SF_6 would be an excellent "scavenger." Figure 9 shows SF_6 is excellent to maintain ε_k around 5-6 eV over the range: 100 < (F/P) < 200 V/torr-cm where our accelerator is never able to reach, however it is not an efficient moderator for (F/P) < 100 V/torr-cm where our accelerator's (F/P) values usually are. Summarizing again, SF_6 alone does not make an ideal insulating gas for higher V_T. Only when one mixes the following complementary ingredients together does one get close to an ideal one.

SF_6 = Large thermal electron capture cross section.

N_2 = High momentum transfer cross section for intermediate energy range.

CO_2 = High momentum transfer cross section for thermal to intermediate energy range.

H_2O = High momentum transfer cross section for thermal to intermediate energy range.

D. Possible Negative Ion Production, Ionic Reaction, Charge Transfer Reaction, Ionic Mobility, Ion Cluster Formation.

We have already listed two negative ions, i.e., F^- and O^- as candidates which limit V_TMax. to much lower values and which trigger the "critical gradient discharge" at much lower (F/P) values than (F/P)critical and to be major contributors for the strong coupling between the axial and radial activities. As we mentioned before, both have a rather high ionic mobility and electron affinity.

We list all possible ions produced by electrons in our tank gas mixture with their reaction processes, electron affinities, ionic mobilities, threshold energies, peak energies, and maximum cross section in Table 10.

1) $e + SF_6 \rightarrow SF_6^-$ (Table 10, Fig. 15).

There has been no sign of this being involved in triggering the "critical gradient discharge." Its electron affinity is 0.54 eV, its lifetime = 10 μs or longer, and its ionic mobility is 0.65 $cm^2V^{-1}s^{-1}$ and through its repeated autodetachments and attachments it is probable that it reduces its ionic mobility further.

2) $e + SF_6 \rightarrow SF_5^- + F$ (Table 10, Fig. 15).

$F + e \rightarrow F^-$ (Fig. 16).

This is one of the many ways to deplete rather expensive SF_6 and through a secondary reaction, $F + e = F^-$, to become a source of F^- also. We would like to avoid or minimize this reaction by maintaining the electron energy close to thermal and maintaining SF_6 molecules at their ground states. However, there has been no sign of SF_5^- itself causing the "critical gradient discharge" yet. Its electron affinity is rather high, and its lifetime is much shorter than 3×10^{-8} sec., but its ionic mobility is 0.65 $cm^2V^{-1}s^{-1}$. Its threshold energy is too low (≅ 0.4 eV) to be prevented.

3) $e + SF_6 \rightarrow SF_5 + F^-$

$\rightarrow SF_4 + F + F^-$, etc. (Table 10, Fig. 16.)

Its electron affinity is rather high, 3.448 eV, and its ionic mobility is also rather high, 2.7 $cm^2V^{-1}s^{-1}$, and these are qualifications for the poisonous negative ions. Its cross section's first peak at 0.6 eV is too low to be prevented, but the second peak at 6.0 eV, is definitely preventable by carefully mixing gas ingredients. As we discussed before, this dissociative attachment cross section is a strong function of electron energy and the initial SF_6 thermal energy. We should maintain the electron energy as low as possible and as many as possible of the SF_6 molecules in their ground state.

4) $e + CF_4 \rightarrow CF_3 + F^-$ (Table 10).

Its threshold is near thermal and the peak energy is 1.3 eV. It is too low to be prevented efficiently and its maximum cross section (10^{-6} cm^2) is rather high. It is better to eliminate or minimize this impurity content by requesting the manufacturer to lower its level [e.g., 100 ppm/cylinder (by weight)]. Its permissible level has been set at 0.015 psia tentatively. This is a troublemaker, but by accident helped us to pinpoint the poison in our gas mixture.

5) $e + CO_2 \rightarrow CO + O^-$ (Table 10, and 6, Figs. 2, 31, 35, and 37).

Its electron affinity is 1.465 eV and its ionic mobility is 3.0 $cm^2V^{-1}x^{-1}$, and its 4.4-eV peak (Fig. 31) has been the determining factor for V_TMax.-N_2/CO_2 for a long time. We have to limit the amount of CO_2 to 25-30 psia to prevent the creation of some complications, because its threshold energy is rather low, 3.4 eV. Figures 35 and 37 show the ionic reaction products in pure CO_2 and $O_2 + CO_2$, respectively [Moruzzi and Phelps (1966)].[23] Figure 35 indicates that most abundant negative ion in pure CO_2 is CO_3^- instead of O^- because of ion cluster formation of CO_2 (characteristic of polar substance), and Fig. 37 shows CO_4^- and CO_3^- are more popular than O_2^- of O^- in the O_2-CO_2 mixture as a result of ionic reactions (third-body reactions). We should notice that all these ionic reactions (ion cluster formations) contribute to a decrease in their ionic mobilities.

6) $e + H_2O \rightarrow OH + H^-$,

$\rightarrow 2H + O^-$ (Table 10, Figs. 33, 36, and 38).

The H^- has an electron affinity = 0.754 eV and an ionic mobility = 12 $cm^2V^{-1}s^{-1}$, but its threshold energy is 5.6 eV which is rather high. If we maintain ε_k below 4 eV, there will be no problem with H^-. For the O^- case the

cross section is much lower than the H^- production and its threshold energy is higher (7.5 eV) (Fig. 33) [Schulz (1960)[24], Compton and Cristophorou (1967)]. Figure 36 shows the results of ionic reactions in triply distilled H_2O. We can see many OH^- and the clustering products of H_2O such as $(H_2O)\cdot OH^-$, $(H_2O)_2\cdot OH^-$, $(H_2O)_3\cdot OH^-$, $(H_2O)_4\cdot OH^-$, and $(H_2O)_5\cdot OH^-$ besides OH^- and H^-.[2] Figure 37 displays the similar ion clustering on an O_2-H_2O mixture. These are $O_2^-\cdot(H_2O)$, $O_2^-\cdot(H_2O)_2$, $O_2^-\cdot(H_2O)_3$, and $O_2^-\cdot(H_2O)_4$ besides O_2^- in the low (F/P) region. All these phenomena are helping to slow down their ionic mobilities.

7) $e + O_2 \leftarrow O + O^-$ Table 10, Figs. 32, 34, 37, and 38).

Figure 34 indicates O_2^- is a dominating negative ion at low (F/P) and may cause some complications, but is involved quickly in some ionic reactions as Figs. 37, 38, and 39[23] suggest. The O^- production cross section has a threshold energy = 4.63 eV (Fig 32), and it will be safe as long as we maintain ε_k below 4 eV. However, it will be better to minimize the contamination by O_2, because there is O_2^- production.

8) Ion clustering (Figs. 10[19], 35, 36, 37, and 38[23]).

These figures demonstrate how readily ion clusterings are made in the cases, SF_6, DO_2, H_2O, O_2-CO_2, and O_2-H_2O, respectively. As we mentioned before, the ion clustering phenomena of polar substances like CO_2 and H_2O are stabililizing influences on some unstable negative ions by decreasing their ionic mobilities and their formations are very quick.

9) Charge transfer processes.

$$O^- + SF_6 \rightarrow SF_6^- + O,$$

$$O^- + SF_6 \rightarrow SF_5 + F + O \text{ (Fig. 39)}^{26}.$$

We are not discussing the charge transfer processes in detail here, and show an example in Fig. 37 of the dissociative charge transfer processes between O^- and SF_6. These processes may be playing many important roles in the ionic reactions and change the ionic population and definitely slow down the ionic mobility and may eliminate some dangerous negative ions or make them less dangerous. Another example which we are not discussing here is the detachment process that might be playing an important role.

F. De-excitation, Vibrational and Rotational Excitation and De-excitation, Relaxation time.

We have been mentioning repeatedly the importance of quick de-excitation of gas molecules and maintaining them at their ground states all the time even after the gas discharges, for the purpose of being more efficient thermalizers such as CO_2 and H_2O, for slowing down mediums like N_2, and to avoid opening the decay channels [i.e., $(SF_6^-)^* \rightarrow SF_5^-$, $(SF_6^-)^* \rightarrow SF_5 + F^-$, $(SF_6^-)^* \rightarrow SF_4 + F + F^-$, etc], and to stabilize the SF_6^- by collision with other gas molecules. Figure 40 shows an unusually long relaxation time (at 300 K, $t \cong 10^3$ s) for N_2 and only when its temperature is up to 8000 K does t drop down to 10^{-6} x.[28] Figures 3, 25, and 41 show that the prominent 2.5-eV peak of the momentum transfer cross section of N_2 is made of v=1,------, 8 vibrational excitation modes, and makes N_2 an excellent slowing-down medium to limit or maintain the ε_k around eV.[29,23] For example, if we have 100 psia N_2 in the tank, the ε_k in the gas will be 1.8 eV under the condition of $V_TMP \cong 18$ MV (Fig. 27). However, as Fig. 40 suggests, once they are excited to the 2.5-eV vibrational excitation level through gas discharges, they will never be able to be de-excited among themselves down to their ground states, and the electrons start to do superelastic collisions with N_2^* (2.5 eV) and you end up with the critical gradient discharge condition.

Table 4 and Fig. 1 indicate N_2^* (2.5 eV) and F^- production at 6 eV from SF_6 have been major troublemakers in the N_2/SF_6 mixtures and damaged the column

structure members in many accelerators (Yale MP-1, Heidelberg MP-5, Munich MP-8, Los Alamos, and Queens) , because N_2 * (2.5eV) cannot absorb the released energy from gas discharges and forces it to be dissipated into the column structure members. Even in the N_2/CO_2 mixture case, and to a much lesser degree the N_2 * (2.5 eV) and O^- production at 4.4-eV peak from CO_2, are a determining factor for V_TMax.-N_2/CO_2. However, as we mentioned before the N_2 has a much shorter relaxation time at higher temperature (Fig. 40) and the existence of CO_2 in the gas mixture allowed us to use our vigorous continuous discharge technique to voltage condition our Aℓ electrode accelerating tubes.

Figure 42 shows CO_2's relaxation time is about 6 x 10^{-6}s at 1 atm and 300 K. Figures 6, 26, 45, 46, 47, and 48 give more detailed information to explain the CO_2's excitation and de-excitation mechanisms. For CO_2, the frequency of the bending mode (ν_1 = 672.2 cm^{-1}, Figs. 45 and 47) is very close to being exactly twice that of the symmetrical stretching mode (ν_2 = 1351 cm^{-1}, Figs. 45, 47, and 48) so that transfer between them should be abnormally rapid through the resonance effect. We have already witnessed that CO_2's four distant vibrational excitation levels at 0.08 eV, 0.3 eV, 0.6 eV, 0.9 eV (Figs. 6 and 46) are deciding factors for the so-called "parabola seven sparks cycle" in a N_2/CO_2 mixture (Table 6 and Fig. 2), major contributing factors for CO_2's excellent thermalizing characteristics.

Figure 43 shows the relaxation times for H_2O are very short (0.02 x 10^{-6} sec. at 1 atm and 300 K) and vary quite slowly with the temperature T. For CO_2, at room temperature, the presence of a small amount of water vapor certainly changes the relaxation time drastically [Fig. 44, and Formula (4)]. Most of the experimental results indicate that deactivation by collision with a water molecule is about 1000 times as probable as by collision with a second CO_2 molecule. Whether in these cases the deactivation process is one of vibrational states (0,0,0), (1,0,0), (2,0,0), (0,1,0), and (0,0,1), in which the respective numbers refer to the number of quanta in the bending mode, the symmetric longitudinal mode, and the asymmetric longitudinal mode, respectively (Figs. 45 and 47), are designated as the states 0,1,2,3, and 4. Cross sections for transitions between all these states were calculated up to collision energies of 2.5 eV. Having all these cross sections it became possible to study the effective relaxation allowing for transfer between the different modes.

In further calculations Marriott[31] investigated the effect of water vapor on relaxation in CO_2. This is well known to be very pronounced and indeed has probably contributed substantially to the unreliability of many experimental observations of relaxation times. It has been suggested that the effects are large because of chemical forces such as those that lead to the production of carbonic acid H_2CO_3. Marriott ignored such forces but included in addition to the Lennard-Jones (12,6) interaction, a specific additional contribution due to interaction between the permanent dipole moment of the water molecule and the induced dipole moment in CO_2. He succeeded in explaining the most outstanding features of the relaxation of CO_2-H_2O mixtures.

$$\text{Lennard-Jones Potential} = V(r) = \frac{\varepsilon}{n-6}\left\{6\left(\frac{r_m}{r}\right)^n - n\left(\frac{r_m}{r}\right)^6\right\},\ n=12. \qquad (5)$$

Figure 49 shows SF_6's $\nu_{min.}$ = 344 cm^{-1} excitation level has a probability of vibrational deactivation per collision among themselves at room temperature of 10^{-3} and CO_2's ν_1 = 672.2 cm^{-1} level has a probability of 5 x 10^{-5}. If we may add two values for comparison, N_2 = 4 x 10^{-7} and H_2O = 7 x 10^{-3}. But Table II indicates CO_2 might have an excellent chance to make an inter-molecular vibrational energy transfer in a near resonance fashion and its probability of

vibrational deactivation per collision between SF_6 and CO_2 through SF_6's $\nu_{min.} = 344\ cm^{-1}$ to CO_2's $\nu_1 = 672.2\ cm^{-1}$, $\Delta\nu = 2\nu_m - \nu_1 = 16\ cm^{-1}$ may be 10^{-2}, and between N_2 and CO_2 through N_2's $\nu = 2330\ cm^{-1}$ to CO_2's $\nu_3 = 2344.2\ cm^{-1}$; $\Delta\nu = 19.2\ cm^{-1}$ may be 10^{-3}.

Now we have an excellent deactivation channel for $(SF_6^-)^*$ and $(N_2)^*$ through

$(SF_6^-)^* \rightarrow CO_2$, a near resonance intermolecular vibrational energy transfer

$(N_2)^* \rightarrow CO_2$, a near resonance intermolecular vibrational energy transfer

$(CO_2)^* \rightarrow H_2O$, through a Lennard-Jones (12,6) interaction and a coupling between the permanent dipole monent of H_2O and the induced dipole moment of CO_2.

We could expect a final relaxation time in our gas mixture on the order of 10^{-8} sec, which is shorter than the $(SF_6^-)^*$ lifetime ($\sim$ 10 μs) and is comparable to the lifetime of the predissociating state $(SF_5^-)^*$ of $(SF_6^-)^*$ ($t_2 << 3x\ 10^{-8}$ sec).

This information proves again that CO_2 and H_2O are indispensable ingredients in the gas mixture to deactivate the excited gas molecules. We have witnessed the proof that Yale MP-1 has never faced any signs of the "critical gradient discharge" phenomenon, the wild follow-up discharge phenomenon, and any necessity of gas recirculation, since we started to take care of the gas mixture ($SF_6/N_2/CO_2/H_2O$) utilizing the above discussed atomic physics information.

IV. CONCLUSION

Present Status of the Optimum Gas Mixture. In Table 17 we list all major ingredients we like to have and two possible impurities we would like to minimize in our gas mixture, including the minimum necessary amounts, which we have gotten through our experiences, the maximum permissible quantities, which we have determined only from the point of view of limiting the concentration of poisonous negative ions below the limit which we have tentatively set utilizing our CF_4 incident experiences (Table 5), the maximum quantities that have been tried thus far in existing accelerators, and finally, the tentative optimum gas mixture values. In Table 18 we list an optimum gas mixture recipe separately.

If we use this suggested gas mixture, the "electron characteristic energy" ε_k should be about 1.8 eV (100 psia N_2 alone) for V_TMax. MP = 18 MV (18-ft-diam tank) and V_TMax. XTU = 25 MV (25-ft-diam tank) and is safe even when many N_2 are excited to the 2.5-eV level, because ε_k total is below the threshold of F^- production at 6 eV in SF_6.

Also we may add that running the accelerator about 2 MV below the V_T critical is essential for a quiet and trouble-free operation.

In conclusion we have found an optimum gas mixture which guarantees a comfortable operation at V_T = 16 MV for the MP (tank diameter = 18ft and V_T = 22 MV for the XTU (tank diameter = 25ft and it lets us enjoy about a 30% higher V_T using the $SF_6/N_2/CO_2/H_2O$ (210 psia) than the 100% SF_6 (90-130 psia).

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Table 11

SF_6	CO_2	344	672.2 $= \nu_1$	16 $= 2\nu_A - \nu_{B1}$	10^{-3}	10^{-2}	10^{-4}
N_2	CO_2	2330	2344.2 $= \nu_3$	19.2	10^{-8}	10^{-3}	10^{-4}

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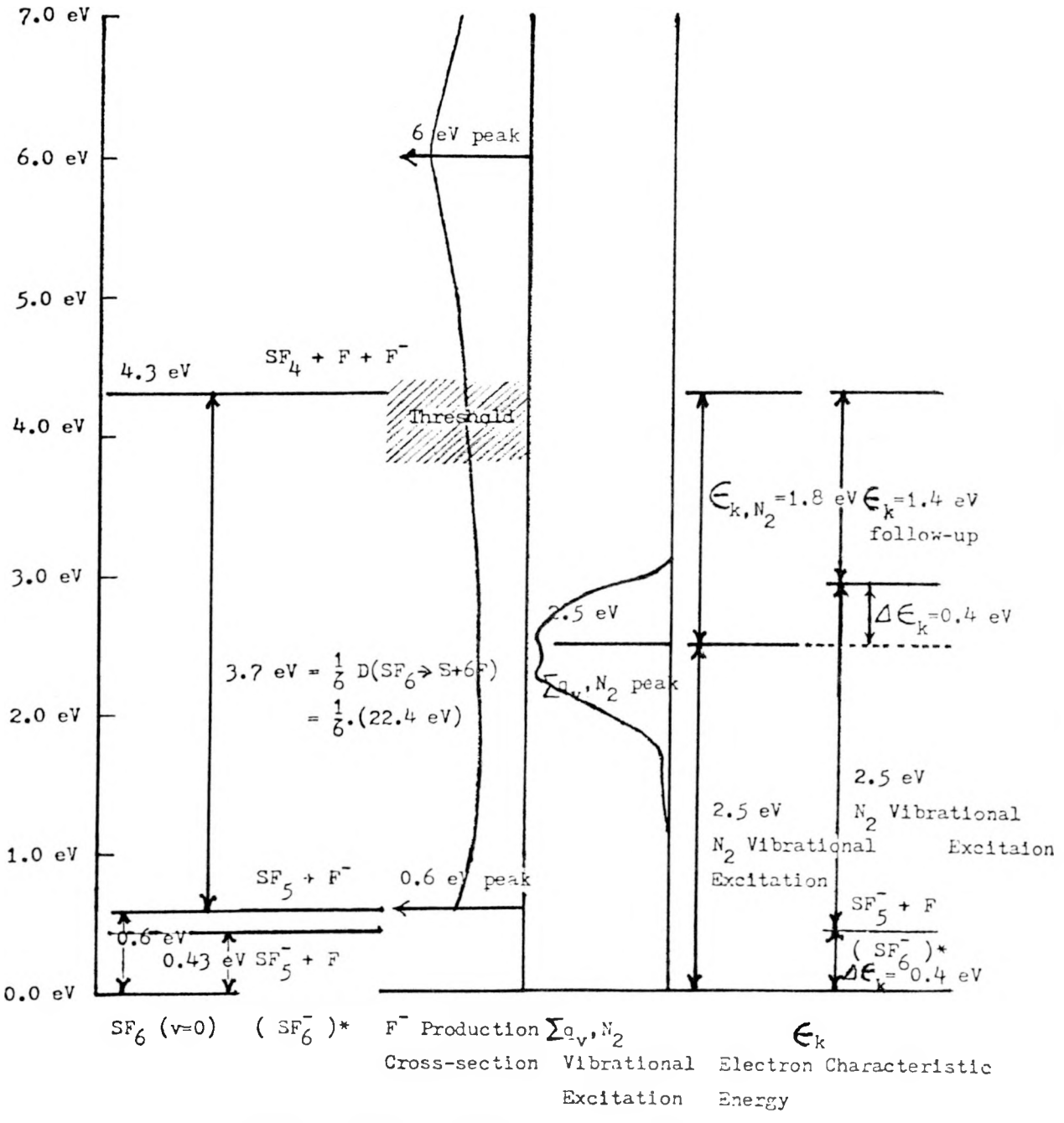

FIG. 1. : Critical Gradient Discharge Energy Relations in Gas Mixture A.

(42 psia SF_6 (37%) + 72 psia N_2 = 114 psia).

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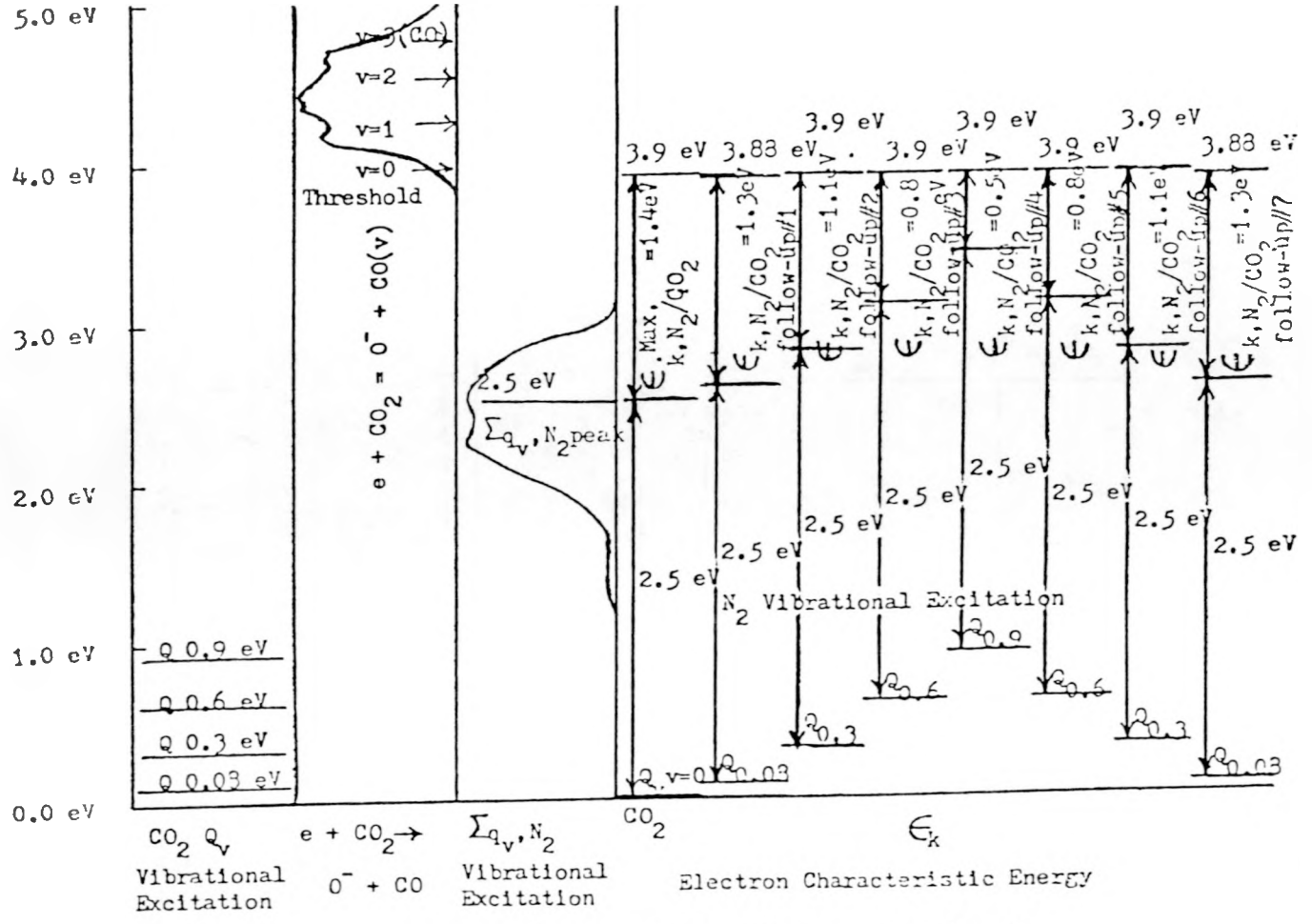

FIG. 2. : Discharge Energy Relations in Gas Mixture H (N_2/CO_2).

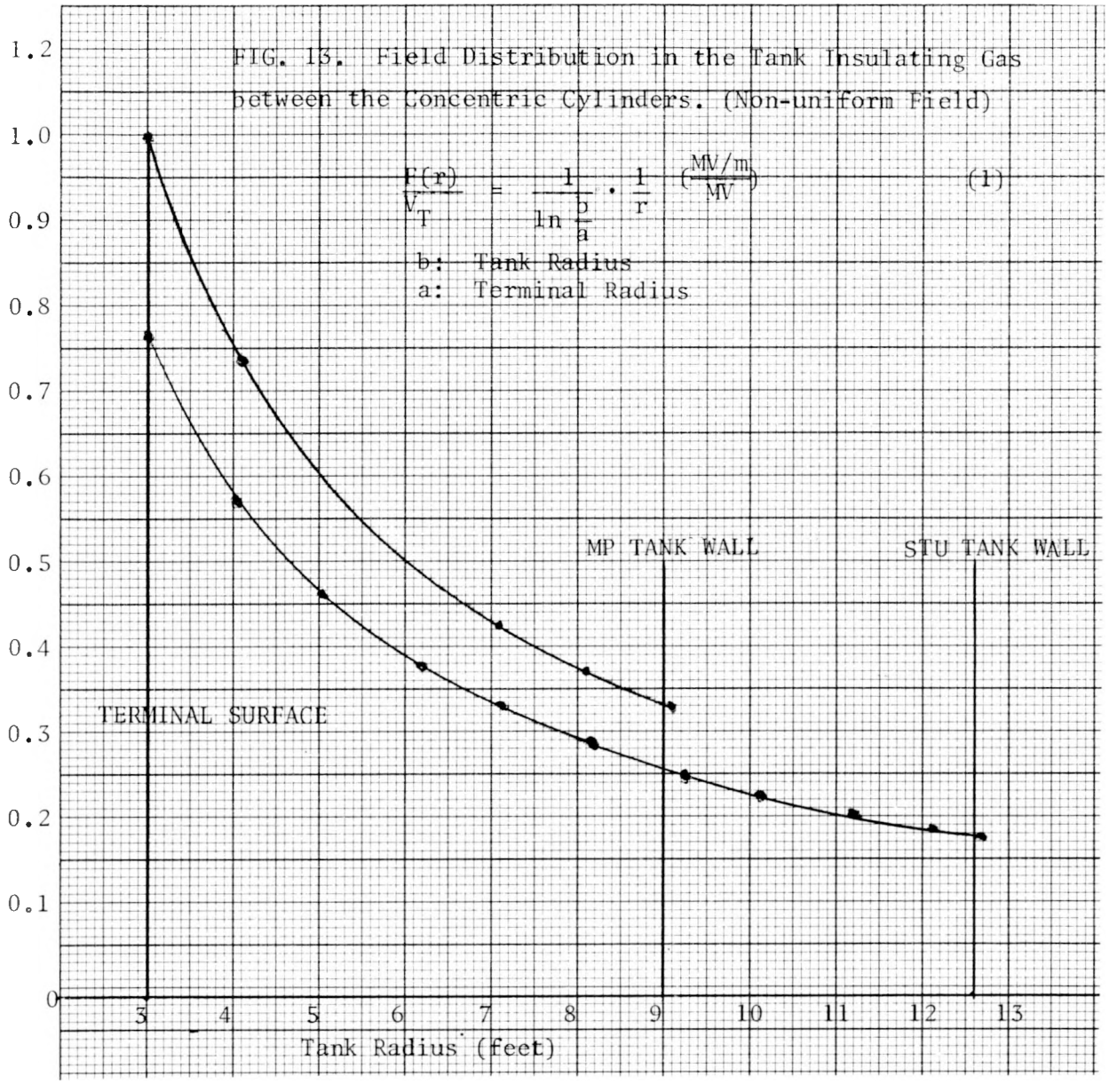

FIG. 13. Field Distribution in the Tank Insulating Gas between the Concentric Cylinders. (Non-uniform Field)

$$\frac{F(r)}{V_T} = \frac{1}{\ln \frac{b}{a}} \cdot \frac{1}{r} \quad \left(\frac{MV/m}{MV}\right) \qquad (1)$$

b: Tank Radius
a: Terminal Radius

TABLE 1. : V_T Without Tubes VS V_T with Tubes (Al or S.S.).

Accelerators		V_T Max. without Tubes	V_T with Al Tubes	V_T Max. with S. S. Tubes	ΔV_T (1)	GAS
Strasbourg	MP-10	17 MV	8 MV	13.2 MV	3.8 MV	100% SF_6 130 psia.
Chalk River	MP-3	16.6 MV	10.5 MV	13.7 MV	2.9 MV	100% SF_6 95 psia.
Orsay	MP-9	15.3 MV	8.8 MV	13 MV	2.3 MV	100% SF_6
Heidelberg	MP-5	14.6 MV	10 MV	13.5 MV	1.1 MV	18% SF_6 + N_2/CO_2 (34 psia SF_6) = 187.5 psia.
Yale	MP-1		11.75 MV	13.0 MV		34% SF_6 + 52% N_2 + 14% CO_2 = 56 psia SF_6 + 86 psia N_2 + 23 psia CO_2 = 165 psia.
Munich	MP-8	13.5 MV				30% SF_6 + N_2 = 60 psia SF_6 + 120 psia N_2 = 180 psia.
S N L	MP-7		10 MV	14.1 MV		47% SF_6 + 42% N_2 + 11% CO_2 = 69 psia. SF_6 + 61 psia N_2 + 16 psia CO_2 = 146 psia.
H V E C	KTU	21 MV			4.5 MV	100% SF_6 123 psia.
H V E C	KTU			16.5 MV		100% SF_6 90 psia.

(1): ΔV_T = V_T Max. without Tubes - V_T Max. with S. S. Tubes.

TABLE 2. : $(F/p)_{crit.} = 117.5\ Vcm^{-1}torr^{-1}$ vs (F/p) Max. and (F/p) Max.

Accelerators	Gas	SF_6 Press.	V_T Max. No Tube	(F/p) Max. No Tube	V_T Max. Tube	(F/p) Max. Tube
Strasbourg MP-10	100% SF_6	130 psia	17 MV	25.0 $Vcm^{-1}torr^{-1}$	13.2 MV	19.5 $Vcm^{-1}torr^{-1}$
Chalk River MP-3	100% SF_6	95 psia	16.6 MV	33.5 $Vcm^{-1}torr^{-1}$	13.7 MV	27.6 $Vcm^{-1}torr^{-1}$
H V E C XTU	100% SF_6	123 psia	21 MV	25.1 $Vcm^{-1}torr^{-1}$		
H V E C XTU	100% SF_6	90 psia			16.5 MV	27.0 $Vcm^{-1}torr^{-1}$
Yale MP-1	Mixture A(1)	42 psia			12.3 MV	56.1 $Vcm^{-1}torr^{-1}$
Yale MP-1	Mixture E(2)	56 psia			13.0 MV	44.5 $Vcm^{-1}torr^{-1}$
B N L MP-7	Mixture (3)	69 psia			14.1 MV	39.1 $Vcm^{-1}torr^{-1}$
Munich MP-8	Mixture (4)	60 psia	13.5 MV	43.1 $Vcm^{-1}torr^{-1}$		
Heidelberg MP-5	Mixture (5)	34 psia	14.6 MV	82.2 $Vcm^{-1}torr^{-1}$	13.5 MV	76.1 $Vcm^{-1}torr^{-1}$

(1): Mixture A: 42 psia SF_6 + 72 psia N_2 = 114 psia.
37 %

(2): Mixture E: 56 psia SF_6 + 86 psia N_2 + 23 psia CO_2 = 165 psia.
34 % 52 % 14 %

(3): B N L Mixture: 69 psia SF_6 + 61 psia N_2 + 16 psia CO_2 = 146 psia.
47 % 42 % 11 %

(4): Munich Mixture: 60 psia SF_6 + 120 psia N_2 = 180 psia.
30 %

(5): Heidelberg Mixture: 34 psia SF_6 + 153.5 psia N_2/CO_2 = 187.5 psia.

TABLE 3. : V_T Max. 100% SF_6 vs V_T Max. Mixture; Critical Gradient Discharge.

Mixture	SF_6	N_2	CO_2	V_T Max. (1) 100% SF_6 No Tube	V_T Max. (1) 30% SF_6+N_2/CO_2 No Tube	V_T critical Tube	V_T Max. Tube	Radiation Source 4 c ^{137}Cs
A	42 psia 37 %	72 psia	0	8.9 MV	11.9 MV	12.3 MV		Yes
B	48 psia 36 %	85 psia	0	9.8 MV	13.3 MV	12.4 MV		Yes
C	46 psia 29 %	114 psia	0	9.3 MV	14.8 MV	12.3 MV		No
D	46 psia 29 %	106 psia 67 %	6 psia 4 %	9.3 MV	15.0 MV		12.5 MV	No

(1): H V E C data; Skorka, S. J., " Tandem Accelerators", International Conference on Physics of Tandems at Trieste, April 27. - 30., 1976.

TABLE 4. : ϵ_k Electron Characteristic Energies in Gas Mixtures.

Mixture	V_T critical	V_T Max. follow-up	ϵ_{k,N_2}	ϵ_k follow-up	$\Delta\epsilon_k$ (2)	$E(\Sigma q_v, N_2)$ (3)	$\epsilon_{k,N_2} + E(\Sigma q_v, N_2)$ (4)
A	12.3 MV	8.0 MV	1.8 eV	1.4 eV	0.4 eV	2.5 eV	1.8 eV + 2.5 eV = 4.3 eV.
B	12.4 MV	8.4 MV	1.7 ev	1.3 eV	0.4 eV	"	1.7 eV + 2.5 eV = 4.2 eV.
C	12.3 MV	10.8 MV	1.6 eV	1.3 eV	0.3 eV	"	1.6 eV = 2.5 eV = 4.1 eV.

(2): $\Delta\epsilon_k = \epsilon_{k,N_2} - \epsilon_{k\ \text{follow-up}}$ = Excitation energy for the $(SF_5^- + F)$ level of $(SF_6^-)^*$. (FIGS.1, 15

(3): $E(\Sigma q_v, N_2)$ = A peak energy of the vibrational excitation cross-section of N_2. (FIGS.1,3).

(4): $\epsilon_{k,N_2} + E(\Sigma q_v, N_2)$ = Threshold energy for $SF_6 + e = SF_4 + F + F^-$. (FIGS. 1, .16).

TABLE 5. : V_T Max. 100% SF_6 vs V_T Max. Mixture; Critical Partial Pressure of CF_4.

Mixture	SF_6	N_2	CO_2	V_T Max.(1) 100% SF_6 No Tube	V_T Max.(1) 30% SF_6+ N_2/CO_2 No Tube	V_T crit. Tube	V_T Max. Tube	R. S. 4c $^{137}C_s$	Δ CF_4
D	46 psia 29 %	106 psia 67 %	6 psia 4 %	9.3 MV	15.0 MV		12.5 MV	No	
D'	52 psia 32 %	106 psia 65 %	6 psia 4 %	10.0 MV	15.0 MV	2 MV - 12.5 MV		No/Yes	ΔSF_6=18 cyls. (Air Products) (5) (6) Av. CF_4=409± 90 ppm(weight) ΔCF_4=0.0041± 0.0009psia
E	56 psia 34 %	86 psia 52 %	23 psia 14 %	10.7 MV	15.0 MV		13.0 MV	Yes	ΔSF_6=48 cyls. (Allied Chem.) (5) (6) Av. CF_4=105± 29 ppm(weight) ΔCF_4=0.0028± 0.0008psia
F	48.psia 33 %	76 psia 53 %	20 psia 14 %	9.8 MV	14.3 MV		12.6 MV	Yes	
G	64 psia 39 %	80 psia 48 %	21 psia 13 %	12.4 MV	15.3 MV		(12.9 MV)	Yes	ΔSF_6=36 cyls. (Allied Chem.) (5) (6) Av. CF_4=136± 39 ppm(weight) ΔCF_4=0.0031± 0.0008psia

(1): H V E C data; Skorka, S. J., " Tandem Accelerators ", International Conference on Physics of Tandems at Trieste, April 27. - 30., 1976.

(5): Private communications from Air Products and Allied Chemicals.

(6): A S T M Designation: D 2472 - 71; Requirements, Carbon tetrafluoride Max. weight percent = 0.05 % = 500 ppm (weight) = 330 ppm (volume).

0.016 psia $\geq$ (Critical Partial Pressure of CF_4 in MP-Tank Insulating Gas) $\geq$ 0.013 psia
0.83 Torr $\geq$ $\geq$ 0,68 Torr
830 µ $\geq$ $\geq$ 680 µ.

TABLE: 6.: V_T Max., V_T follow-up in N_2/CO_2 Mixture H; Parabola Seven Sparks Cycle.

Mixtue	N_2	CO_2	V_T Max. Tube	V_T follow-up # 1 & 7	V_T follow-up # 2 & 6	V_T follow-up # 3 & 5	V_T follow-up # 4
H	144 psia 80 %	36 psia 20 %	11.75 MV	11.2 MV	10.3 MV	7.5 MV	6.1 MV
$\epsilon_{k,N_2/CO_2}$			1.4 eV	1.3 eV	1.1 eV	0.8 eV	0.5 eV
$E(\Sigma q_v, N_2)$			2.5 eV	2.5 eV	2.5 eV	2.5 eV	2.5 eV
$Q_v(CO_2)$			0.0 eV	0.03 eV	0.3 eV	0.6 eV	0.9 eV
			+	+	+	+	+
			3.9 eV	3.88 eV	3,9 eV	3.9 eV	3.9 eV = Threshold for $e + CO_2 = O^- + CO(v)$

TABLE 7. : Electron Affinities E_a (in eV)

References	Method	$E_a(F)$	$E_a(O)$	$E_a(O_2)$	$E_a(SF_6)$	$E_a(SF_5)$
(1)	Surface ionization	3.62 ± 0.09				
(2)	"	3.47				
(3)	Lattice energies	3.48				
(4)	Shock wave absorption	3.448				
(5)	Empirical extrapolation	3.50				
(6)	"	3.47				
(7)	"	3.34				
(8)	Equilibrium constant		3.1			
(9)	"		2.33 ± 0.03			
(10)	Mass spectrograph		2.0			
(11)	Photodetachment		1.45			
(12)	"		1.465 ± 0.005			
(13)	Appearance potential		1.89			
(14)	Hot-wire equilibrium method		1.45			
(15)	Retarded potential difference method		2.0			
(16)	"		1.45			
(17)	Detachment experiment			0.46		
(18)	Electron impact			0.58		
(19)	Threshold for charge transfer reaction			($O_2^- + SF_6$)	>0.44	
(19)	"			($S^- + SF_6$)	0.6	
(19)	"			($O^- + SF_6$)	0.67	$0.63^{+0.1}_{-0.17}$
(20)	Threshold for $SF_6 + C_s \rightarrow C_s + SF_6^-$					
(19)	" $X^- + SF_6 \rightarrow SF_5^- + F + X$ (X = S, O, and B_r)					$\geq 2.8\pm0.2$
(21)	" $SF_5^- + NO_2 \rightarrow SF_5 + NO_2^-$					3.2

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TABLE 10. : Possible Negative Ion Productions in Tank Gas Mixture.

Gas Molecule	Process	Negative Ion	Electron Affinity	Ionic Mobility	Threshold Energy	Peak Energy	Max. Cross-section
SF_6	$e + SF_6 = SF_6^-$	SF_6^-	0.54 eV	0.6 $cm^2V^{-1}s^{-1}$	0 eV	0.03 eV	$2.5\times10^{-15}cm^2eV$
	$= SF_5^- + F$	SF_5^-	3.2 eV	$0.65cm^2V^{-1}s^{-1}$		0.43 eV	$10^{-16}cm^2$
	$= SF_5 + F^-$	F^-	3.448eV	2.7 $cm^2V^{-1}s^{-1}$		0.6 eV	$10^{-18}cm^2$
	$= SF_4 + F + F^-$	F^-	"	"	3.5 eV 4.3 eV(1)	6.0 eV	$10^{-18}cm^2$
CF_4	$e + CF_4 = CF_3 + F^-$	F^-	"	"		1.3 eV	$10^{-16}cm^2$
CO_2	$e + CO_2 = CO + O^-$	O^-	1.465eV	3.0 $cm^2V^{-1}s^{-1}$	3.4 eV 3.9 eV(2)	4.4 eV	$1.7\times10^{-19}cm^2$
						8.2 eV	$5.0\times10^{-19}cm^2$
H_2O	$e + H_2O = OH + H^-$	H^-	0.754eV	12 $cm^2V^{-1}s^{-1}$	5.45eV	6.4 eV	$4.8\times10^{-18}cm^2$
			.	.	.	8.6 eV	$1.3\times10^{-18}cm^2$
	$= 2H + O^-$	O^-	1.465eV	3.0 $cm^2V^{-1}s^{-1}$	7.5 eV	8.6 eV	
O_2	$e + O_2 = O + O^-$	O^-	1.465eV	3.0 $cm^2V^{-1}s^{-1}$	4.63eV	6.2 eV	$1.3\times10^{-18}cm^2$

(1): Yale MP-1 data (Ref. Table 4, Fig. 1.).

(2): Yale MP-1 data (Ref. Table 6, Fig. 2.).

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TABLE 12. : Minimum(Necessary), Maximum(Permissible), and Optimum Ingredients in MP Gas Mixture.

Gas Ingriedients	Minimum (necessary)	Maximum (Permissible)	Optimum V_T = 18 MV	Max. Quantities tried so far.
SF_6	50 psia		85 psia	130 psia, 100% SF_6(Strasbourg). 70 psia, Mixture (B N L).
N_2	70 psia		100 psia ϵ_{k,N_2}=1.8 eV for V_T=18 MV	117 psia (H V E C). 114 psia (Yale).
CO_2	15 psia	36 psia	25 psia	25 - 29 psia (H V E C). 23 psia (Yale).
H_2O	0.012 psia D.P.=-45°C (-49°F)	0.31 psia D.P.=-13.4°C (+8°F)	≥0.02 psia D.P.≥-40°C (-40°F)	0.02 psia, D.P.=-40°C(-40°F) (Yale).
CF_4		0.015 psia	None	0.013 psia: safe(Yale) 0.015 psia: critical(Yale).
O_2		1.5 psia	None	

TABLE 13. : An Optimum Gas Mixture for MP Tank.

SF_6	N_2	CO_2	H_2O	Total Pressure
85 psia	100 psia	25 psia	≥ 0.02 psia	210 psia
40 %	48 %	12 %	D. P. ≥ -40°C (-40°F)	

OPERATING EXPERIENCE WITH AN 834 HICONEX SPUTTER ION SOURCE

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INTRODUCTION

For about a year now, we have been using an 834 sputter ion source manufactured by General Ionex Corporation. This ion source is a standard cesium gun sputter source as developed by Roy Middleton with the addition of a cylindrical einzel lens/steerer to refocus the positive cesium beam onto the sputter cone. In my talk today, I will summarize our operating experiences with the source, both the good things and the bad. There have been some of each. I'll also mention some of the beams that we have observed, although we have hardly done more than verify some of the beams initially reported by Roy Middleton, and recently published in Nuclear Instruments and Methods.

SOURCE OPERATION

Before I go into the details of our operating experiences, I would like to take a few minutes to describe the operation of the refocus-electrodes in this source. Figure 1 is a schematic representation of the operation of the source. This figure shows the operation of the lens in producing a focus in the

cesium beam at the sputter cone target. The lens is designed to operate at the extraction voltage, and a small trim power supply is used to adjust the focus. Figure 2 shows the arrangement of the various power supplies used to operate the source. The einzel lens electrode is made up of four 90° sectors which can be biased to produce a steering effect on the beam. This helps take care of any small misalignment in the source electrodes, and some target cones benefit from being able to steer the beam around while tuning up the source.

SOURCE PERFORMANCE

When we began running the source on a regular basis as part of the experimental program, we began to have problems with the source lifetime, in particular with the ionizer heater. As initially delivered to us, the ionizer heater required an input power of up to 350 watts in order to reach the desired operating temperature of about 1150°C or so. Typical operating parameters were 9.5 volts and 34 or 35 amps. This level of input power was very close to the absolute limit of the heater, and we had several heater failures. By slightly reducing the input power by 10-15 % or so, the heater lifetime was acceptable, but there was a persistent tendency for the extraction and suppressor current to climb after some hours of operation. Operating the ionizer at a lower temperature results in a build up of neutral cesium on the extraction electrodes with the subsequent increase in current drain.

We also had a couple of failures of the braze joint which joins the feed tube to the stainless steel cesium reservoir.

A couple of mechanical things were also troublesome. The ratchet indexing mechanism would not reliably index the cones accurately. Also, several parts of the source are joined with 4.40 stainless screws which have the nasty tendency of breaking off if an accelerator technician (graduate student!) is the least bit overzealous in tightening the source parts.

The suppressor insulators have also given some trouble by coating over, in spite of fairly careful shielding, and drawing current, and also by breaking off a few times. The broken insulators tend to misalign the electrode which results in erratic operation of the source.

Things were not really as hopeless as they might sound at this point. The source did work most of the time and it was a valuable addition to the research program, but clearly some improvements could be made. The most fundamental problem was that of the ionizer heater reliability, and we made some changes to reduce the heat losses, so that the input power could be reduced.

A rear heat shield, toward the reservoir, was added to reduce the radiation losses. This heat shield was just two layers of tantalum foil, .010" thick cut with scissors to clear the heater leads and the ionizer feed tube. This also helped reduce the temperature at the braze joint in the feed tube,

and subsequently, GIC has moved the braze joint back to the reservoir itself, on their current generation of cesium guns. Another way to reduce the heat losses was to reduce the heat conducted in to the beam forming electrode at the ionizer. This was accomplished by reducing the surface contact between the ionizer heater and the beam forming electrodes by relieving all of the concentric diameters to leave only 3 or 4 points for locating the parts. After making these changes, the heater power required was about 200 watts. The typical operating parameters are now 7 volts and 29 amps or so.

The target wheel mechanism was made to work moderately well by periodic replacement of the spring plunger, a standard purchased part, in the rachet mechanism. Mike McKeown from Brookhaven told me that GIC modified their source to improve the indexing mechanism, but I don't have the details of the change.

BEAM PERFORMANCE

Table I is a summary of some of the more unusual beams we have produced with this source. The main feature of these numbers is the usefulness of molecular beams for higher intensity. As I mentioned , this data is really just verifying a few of the many beams reported by Roy Middleton in his survey of negative ion beams from a sputter source.

We tried Lithium from the sputter source for an experiment once, and we were not pleased with the results. The intensity of 1 or 2 microamps at the low-energy faraday cup was acceptable

for most any of our experimental requests, but the transmission of the beam to the target was not as good as we were used to with a charge exchange source. Table 2 summarizes our experience with the lithium beam. In addition to the poorer transmission, the sputter source would require some attention to tweak up the beam a couple of times a day, maybe a couple of times in a shift. That type of experience is not at all like our operation with the charge exchange lithium source where we have many times run for more than one week without touching any knobs for the ion source. Of course the virtue of the sputter source is still the ability to quickly change from one beam type to another, and in this respect the performance with lithium is good.

SUMMARY

All in all we are pleased with this ion source. Recently, it has probably been used 80% of the time for experimental work. We have a couple of things planned for additional improvements to the source that I'll just mention in closing. The reflected beam operation is an exciting possibility for an improved emittance from this source, and also should allow production of expensive separated isotopes into useful negative ion beams at a modest cost. This concept has been reported by Klaus Brand in Nuclear Instruments and Methods, and also at the Strasbourg Conference earlier this year. Also we want to install a vacuum lock and a "ladder" of target cones to provide quick change of

the cones without letting up the source vacuum; also, the last cone on the ladder will not get contaminated by passing over it as is necessary with the standard target wheel. There is also work to be done to explore the possibilities of enhancing various beams using appropriate gas feeds, as also previously mentioned by Roy Middleton.

Discussion:

Liebert: We now supply the ion gun with the shielding actually in place. A lot of the suggestions we have made to Rochester were, in fact, incorporated in their own source after we delivered it. Our present source now operates with 170 watts on the heater. The gun itself also has a somewhat different construction in that it doesn't have a brazed joint in the position where Terry had indicated there were failures. In fact it's now all welded until one is at the reservoir where the temperatures are quite a bit lower. We think we have improved the reliability of the gun part of the source to a point where it's as reliable as any other source around and I mean sputter and otherwise.

Chapman: What was the mode of failure of the heaters before you made this change? What surprises me a little is that you can cure it by reducing the wattage. We formerly had the Extrion-type-heaters and we had many failures with those until we made our own. The problem with those was not a question of wattage, it was a question of temperature. The insulation between turns would fail. Recording the temperature, and reducing the wattage and running at the same temperature wouldn't help that, obviously.

Lund: I think we had failures of all different kinds. We had indications that we were, in fact, evaporating the heater element. The resistance of the heater was going down with time as the wire was getting smaller. We had heaters short out and we had heaters open up inside. I guess one other problem we had was the fit between the heater and the feed tube was not consistent from heater to heater or from gun to gun. In fact, we had one or two guns where the heater would be running clearly above its temperature and yet the ionizer was a dull orange or way below temperature. There was not enough conduction heating between the two.

Chapman: I see. We made our own heaters and since then we have not had that problem at all. I believe Oak Ridge has made their own heaters, probably of a nicer design than ours. We use a boron nitrite former. They just use a free wire spiral with small ceramic beads on it and this completely gets rid of the transmission current between turns which was the nemesis of the original Extrion heaters.

Adams: Ken, we have found that boron nitride does sublime at some of the temperatures we are operating at, around 1100 or 1200. You must be careful that you don't poison or don't actually coat the ionizer surface itself, so watch for this.

Chapman: We have certainly noticed that we get a white deposit, but at least in our construction it doesn't come in the place where it's likely to coat or poison the ionizer. It seems to depend very much on the particular batch or quality of boron nitride. Occasionally you get some and it appears to coat everything and you get another batch which is much less likely to do this.

Richardson: We have tried a number of things and at the moment on the sputter source, we are simply using free-turned spiral molybdenum wire and there is no insulation on it at all. We just let it stand out there in space close to the ionizer with some reflective heat shielding around it. Gerald has also tried using a cone-like boron nitride or Macor which is another machineable glass insulating material. We have also used the spiral wire where you spray it with

magnesia oxide--either of these two works fairly well. I can run an uninsulated moly wire for weeks and weeks.

Adams: What kind of power levels do you put into it?

Richardson: I don't know the voltage, we sort of have an arbitrary limit of about 25 amps. I am sorry that I don't know the voltage, but that gets us up to a fine operating temperature and at that level the heaters don't seem to wear out.

Lund: Do you know what size the moly wire is, what gauge or what diameter?

Richardson: I am going to guess but I think I will be pretty close, like 50 to 60 mils with 7 to 8 turns--even the return leg we just leave spaced out. We tried the beads that Ken mentioned and they seem to be creating more problems than they offered solutions.

Liebert: I think the question was asked, "Why should they fail and why should the failure rate be reduced with lower wattage?" The temperature is actually reduced in the heater itself. You have actually raised the temperature of the heater quite a bit higher because you had losses from all ends. In fact, our power reduction scheme is not only a shield at the back. We made some changes in the electrode structure in the front to reduce the area of the electrode which gets hot. We calculated, at one time, that we could radiate up to 200 watts up front out of 350 watts, under certain conditions. I made a measurement which indicated about 150 watts was being radiated out of the front just by looking at the temperature distributions on a beam forming electrode in the test chamber. When we changed the structure and material of the front electrodes so that it's a much smaller central area which is getting hot, it will not radiate anywhere near the wattage that the old one used to. Since we were using moly wire in the heaters, we were up near the recrystallization temperature of the moly in the heater before. I think both effects of assembly and disassembly, particularly on the lead-in wires, and the fact that he mentioned earlier, evaporation of the moly inside the heater, were present. In fact, you could see discoloration of the alumina potting when you raised them up high. One heater at Rochester was inadvertently run at 1300°C not too long ago, which is considerably higher than you need to run for operation, and there was a resistance change that I noted when I was there on the wire due to running it at that elevated temperature. You see, the heater is coaxial and sits over the tube of the ionizer gun. The beam-forming electrode is in the front and it actually locates on the heater, in our scheme, so that the entire beam forming electrode would very often go up near a 1000°C which was part of the problem.

Middleton: I would just like to make a comment on the transmission with lithium. We found that lithium seems to be abnormal and the transmission we can get through our machine is always less than with other ions, even when we have a system where, essentially, the emittance of the beam is fixed. For some reason I don't think we have ever had a transmission of lithium better than 30% from the sputter source. I began to wonder if this was not a characteristic of the lithium negative ion. The very fact that with a charge exchange source you can get higher transmission would appear to rule that out. Unless, is it possible that the sputter source makes lithium in a different negative ion-configuration than charge exchange? This may be a possibility, or alternatively, when I say that the system we have defines the emittance of the beam which is well within

the acceptance of the accelerator, this is all predicated on the assumption that the negative ion comes from inside the cone. If, for instance, it came from the back face of the cone, the emitting area could be much larger than we realize and therefore the emittance could be much worse. Certainly lithium is very much worse than say carbon or hydrogen.

Lund: Lithium out of the charge exchange source has always been one of our best beams in terms of transmission, since it is the one that we use the most. People are used to putting a microamp in and having a microamp on target in the spectrograph chamber when that was clearly not the case with the sputter source, we began to wonder what was really going on. Clearly it was not as good from the sputter source as it was from the charge exchange source. But you may be right about the production of the negative ion.

Schultz: Does anyone have any updated information in the process of pushing the cesium beam through the cone and utilizing the back side for scattering with the 834 source? There was some talk, many months ago, about the steering and focused capability of getting some fine optics off the backside.

Lund: Yes, there is the paper by Klaus Brand from Bocum in Nuclear Instrument and Methods. I don't know the exact reference but I think he is the guy that initially was pushing this idea of the reflective beam sputter source. I think Reuel has a target wheel that's built to offset the cone. The idea is to offset the cone from the axis then steer the cesium beam through the cone and let it get reflected back in the extraction field focused on to the back side of the cone. It turns out to be a very small sharp spot.

Middleton: Recently we have actually (visually) seen the reflected beam. I think it was with a sintered boron cone and the reflected beam was visible through an alignment telescope. In fact, we were using the alignment scope and we could measure the size of the beam and the size of the beam is about the size of a cross hair inside the telescope. The size couldn't be more than 0.002 or 0.003 of an inch. Certainly there is a scope for super emittance beams from the back beam. But one thing that is very, very important. You have the negative-ion extraction electrode grounded. In our source we can bias that. It turns out that if you put a bias of about 70-volts positive on that so-called ground electrode, the spot really sharpens. If you are working at ground potential the spot is somewhat diffuse, maybe about the size of a millimeter, but just increasing the ground electrode bias to plus 70 volts, it's very very critical, you can see that spot really sharpen up.

Schultz: This bias that you are talking about is very important. All of my ion sources, up until the 834 source, have had the capability of utilizing 2 high-voltage power supplies for what I call extraction and acceleration. The 834 source due to its construction precludes being able to do that. I find it a distinct disadvantage. I think the fact that you can tune, so to speak, these two voltages is quite important and does enhance the performance of most of the ion sources I have had.

Liebert: While the 834 source does not have a provision for biasing that electrode, it's a rather simple modification. We do not intend to make that modification a standard model immediately. It's something that is a lot simpler than most of the modifications our customers make on our sources anyway. The geometry makes it fairly simple to do that. In fact, Klaus Brand was out at our

plant one time trying to run one of his offset cones and we did it for him on the spot to one of our sources that was in the plant. I might mention that he has reproduced his results on Walter Pritchard's setup, so that the data has been seen in two laboratories at least at this point. We have a cone wheel which is designed to retain pellets pressed into a few millimeter diameter recess in the back of the wheel which we have not attempted to test yet. I anticipate we will be able to get some data soon. When Harvey Wegner was up visiting us we ran a comparison between a calcium beam which we got from a normal machined cone, and kind of a trick cone that Harvey brought up, which was just a webb or kind of a spider with a little calcium pellet on the axis, which he was trying to get some calcium beam for separated-isotope work. We saw, if I remember the numbers correctly, somewhere in the order of 3/4 of a microamp out of the normal cone and about a factor of 3 lower on the webb. I interpret this to mean that the actual shape of the body of the cone is a strong influence on the way that the beam is re-imaged on the rear of the system. The disadvantage is that we didn't have a bias to work with when we did the test and that may have messed up the results.

McKay: Phil is working on one of these sources and the idea is our source will be able to move the ionizer and everything up to and including the cone, slightly off axis so that as the beam comes through the cone it's reflected back on to the back side. The spot will be on axis for extraction and so on downstream. Most of the parts are in the shops, so we should have that on-line before too long.

Schultz: Just a quick question, it seems that there is more progress than I had realized. Could someone give me a round number for the offset in the hole?

Lund: I think Brand says a few millimeters, a few being two or three, as far as I know.

Middleton: As I mentioned a few moments ago, we have seen this back beam and I would look at it and focus it. If you offset the cesium by more than a millimeter, very obvious astigmatism appears in the beam spot. If the cesium beam is definitely offset then the refelective cesium beam develops sort of wings, like cusps. I think that one has to be really careful about the degree of offset which should be minimal.

Billquist: Who makes these heaters commercially?

Adams: Spectromatt from Watsonville, California, have made our heaters. I might add, that if you can afford the expense get rhenium wire in there because you always find that the tungsten wire snaps off where it comes out of the potted body. Rhenium retains its ductility and it doesn't recrystallize like tungsten or the other refractory metals.

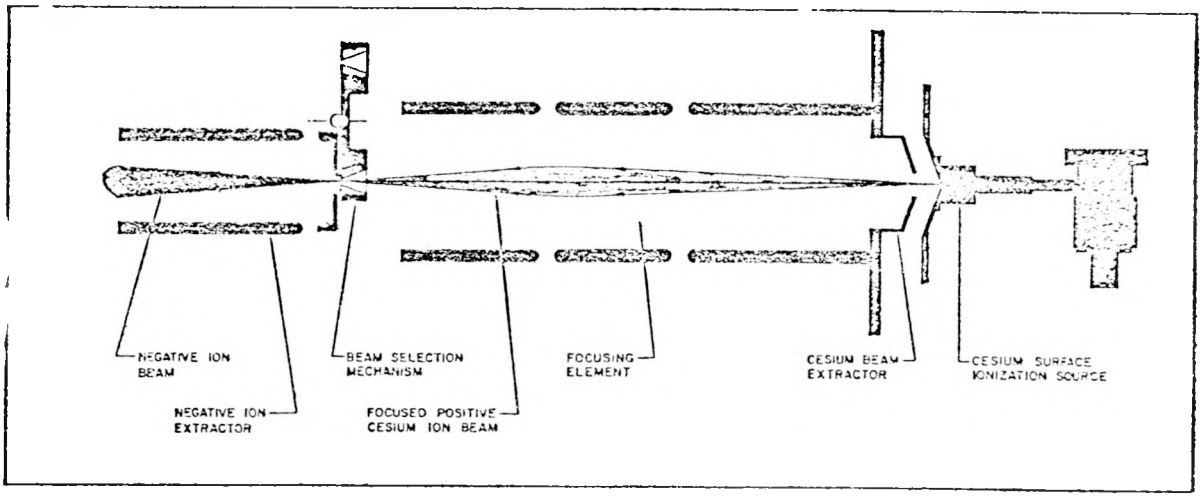
NEGATIVE ION BEAM
NEGATIVE ION EXTRACTOR
BEAM SELECTION MECHANISM
FOCUSED POSITIVE CESIUM ION BEAM
FOCUSING ELEMENT
CESIUM BEAM EXTRACTOR
CESIUM SURFACE IONIZATION SOURCE

Fig. 1

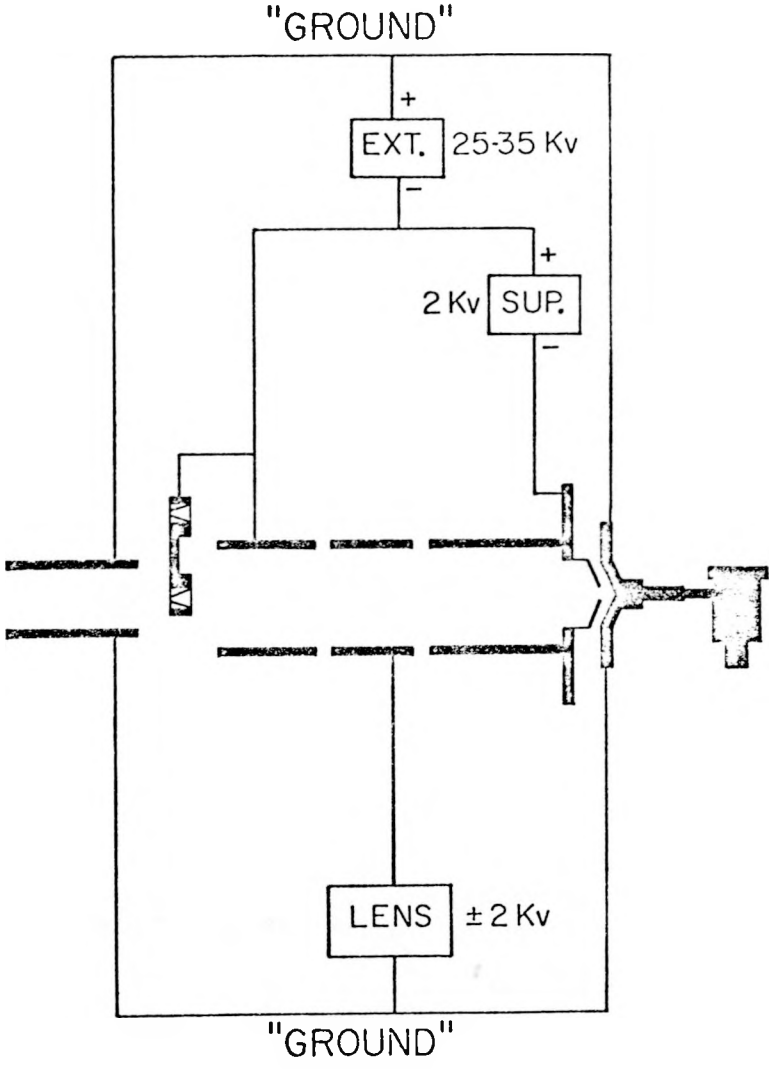
"GROUND"
+
EXT.
25-35 Kv
−
+
2 Kv
SUP.
−
LENS
± 2 Kv
"GROUND"

Fig. 2

TABLE I

Ion Beams

	With NH_3	Without NH_3
BeH	55na	130na
BeOH	2 μa	

Sn 45na

Sn_2 230na

Sn_3 100na

Tungsten	Without oxygen $1x10^{-6}$ Torr	With oxygen $2.5x10^{-6}$ Torr
O_2	5.5 μa	11 μa
W	13 na	-
WO	210 na	150 na
WO_2	390 na	1.85 μa
WO_3	470 na	4.3 μa

TABLE 2

Lithium Transmission Data

36 MeV, ^6Li, +3, 9 MV, gas stripping (N_2)

Charge Exchange Source

LEFC = 185 na

HEFC = 340 na

Image = 295 na

Target = 185 na

Sputter Source

LEFC = 800 na

HEFC = 750 na

Image = 650 na

Target = 350 na

All readings are electrical nanoamps

Session VII, Chairman, C. Adams

THE BROOKHAVEN TERMINAL ION SOURCE

M. McKeown
Brookhaven National Laboratory

As part of the upgrading program at Brookhaven National Laboratory, we installed a General Ionics model 832 cesium sputter source in the terminal of MP-6 which is used as an injector for MP-7. This source has been in operation since September 1976, with a minimum of downtime due to source failure. In one year of operation we had only one source failure due to a tank spark. In that incident the cesium boiler heater was destroyed. We added a G.E. Varistor across the heater and have had no failures since. The terminal Faraday cup logarithmic amplifier has been destroyed many times due to tank sparks in spite of double shielding and filtering of the input and output circuits. The failures were finally stopped by adding ferrite beads to the input and output leads. Faraday cup failure doesn't stop source operation. We also have had normal failures due to the nature of the Cs sputter source.

Dan Larson's optics program helped us to lay out the source components. Optical fitting was very good in spite of tight space limitations. The best transmission (about 50%) is obtained with up to 7 MV on the terminal and 100-KV ions injected into tube 5. As the terminal voltage increases transmission goes down, since 100 KV is the maximum ion energy we can use. This voltage is across the inflector insulators which is about all they can handle. Nine MV is the maximum negative terminal voltage we have run on MP-6.

SLIDE #1 shows a schematic drawing of the terminal source. To the left is the General Ionics source; second from the left, Einzel lens; third, crossed-field analyzer; fourth, preaccelerator (Dynamitron, Westbury, L.I.); fifth, double inflector with aperture for the mass selector in between the two inflectors. The inflectors are a scaled-up version of a design by John Benjamin. I would like to thank John for the help he gave us with the inflectors. Both inflectors have through-holes which allow MP-6 to be operated as a normal two-stage tandem and also to measure the ion beam with terminal Faraday cup before it is inflected.

SLIDE #2 shows the complete terminal source setup on the test bench. The General Ionics source is on the left, followed by the Einzel lens, crossed-field analyzer, preaccel, two inflectors and a cryo-pump on the top. The total weight of source and electronic supplies is about 950 lbs.

SLIDE #3 shows the General Ionics source, Einzel, and crossed-field analyzer. The air-operated cone-wheel changer has been replaced with a mechanical linkage connected to a control rod. The cesium boiler requires some air cooling to keep the extraction current to a reasonable value. There is no air in the terminal

so we had to heat-sink the boiler to the boiler flange with heavy pieces of copper braid.

SLIDE #4 shows the exit end of the crossed-field analyzer. There are two Alnico "C" magnets with soft iron pole pieces in between. The electrostatic deflection plates are mounted on ceramic insulators and have electrostatic field-shaping tips. The resolution is such that mass-12 and -16 can be separated by the crossed-field analyzer.

SLIDE #5 shows the exit side of the second inflector. The ground plane electrode with its slot is just inside the vacuum box. Just behind the ground plane is the deflection electrode with its slot. We had considerable difficulty getting the insulators to handle 100 KV. With the proper shaping of the insulator shields the inflectors were able to take the voltage.

SLIDE #6 shows the power supplies for the terminal source mounted in two shielding boxes. The covers are at the top. The boxes operate at preaccel voltage and are insulated from the terminal with 1in. Lucite. In the upper left corner is the decel power supply (300 volts). This supply operates at extraction level. It is insulated from the main box with a sheet of polyethylene. There is a switch on the cone which gives us the position of the number 1 cone. A pilot light is turned on by the switch and the light is carried across the extraction level with a 12-in-long light pipe. This system gives us an absolute check on cone position. The next box down contains the meters and indicator lights for monitoring the power supplies. A closed-circuit T.V. camera is mounted outside the MP-6 pressure tank and monitors these and other meters.

The extraction power supply (30 KV, 10 MA) is just below: followed by a 30-KV isolation transformer. The isolation transformer supplies 110-Vac power to the decel power supply and to four thermal gas leaks mounted below.

In the upper right corner of the box on the right is the crossed-field analyzer range switch. Just below it are the second set of meters, the Einzel lens power supply, then the preaccel Variac control followed by the crossed-field analyzer power supply. The ionizer and Cs boiler heater controls are next with the shields removed. Last are the main circuit breaker and coolant pump for the source.

SLIDE #7 shows the ion-source coolant pump, about 7in. long, which uses Freon FC-77 as a coolant. The most difficult problem in operating these power supplies in the terminal of an MP is keeping out tank sparks. As you have seen in the past slides, the supplies are shielded as much as possible. All the leads that carry voltages are also shielded with copper braid or tubing.

SLIDE #8 shows the surge protection circuit used on 110-V input to the power supplies. The first Varistor dissipates the surge by decreasing its resistance as the surge voltage increases. The 60-μH inductance and the second Varistor further subdivide the surge to a harmless value.

SLIDE #9 shows the surge protection circuit used on the output of the Einzel power supply which is the same for other supplies (the 30-KV gas-filled spark gap fires on overvoltage and its resistance drops to a fraction of an ohm). The one-ohm resistor limits the power dissipated in the spark gap. A 50-kohm

wire-wound resistor has a few hundred microhenries inductance and in conjunction with the capacity in the power supply further subdivides the surge to an insignificant value.

The surge protection circuit has proved itself many times. The gridded lens at the LE end of MP-7, the terminal ion-pump supply, the terminal steerers, and down-charge supplies in the terminal all gave problems with breakdown due to surges. These problems disappeared with the incorporation of surge protection. The latest problem was with the charging supplies for the new Pelletron installation in MP-7. There was continual breakdown of the RG-213U coaxial cable mounted outside the MP-7 tank which supplied the power to the Pelletron charging inductors.

SLIDE #10 shows the circuit for the protection of the Pelletron charging supplies. The circuit, except for the 5-μH choke, is mounted in a shielded box enclosing the tank feedthrough insulator. There are two spark gaps set to fire at 80 KV in the MP-7 tank gas. A 250-ohm, wire-wound, 50-watt resistor is used for the 250-μH choke. The 5-μH inductance and the first spark gap reduce the surge to a fraction of the original surge. The 250-μH inductance and 150-pF capacitor further reduce the surge, keeping it to a safe value. The second spark gap was added for protection but it has not fired, therefore, the inductance and capacitor does the job of voltage division.

SLIDE #11 shows the Pelletron surge protection circuit mounted in its shield box. The 250-ohm resistor and 2 spark gaps are shown. There are 2 sets mounted in one box, one for the positive supply and one for the negative. We've had no failures since the installation of this circuit.

Discussion:

Schultz: Is your extraction power supply a packaged commercial encapsulated unit?

McKeown: It's a Del and it is encapsulated in epoxy. None of the power supplies is regulated and they are all tied to one generator and that seems to be good enough so there doesn't seem to be any jumping around. You know, if one supply moved with respect to another one, then the beam would be inflected incorrectly or lose its focus, but they all seem to move together and keep the beam fairly well.

Berners: What is the pressure in the terminal source when it's running and how does the performance depend on pressure?

McKeown: We don't know the actual pressure. We have an ion pump and the ion pump is at the end of all this string and that says the pressure was 10^{-6} torr. I think the pressure in the source itself must be pretty high because, as soon as we start adding a little gas through our thermal leaks, we start getting stripping in the ions and we have even gotten positive ions in direction of the source. So we tend to stay away from adding gas and, mainly, we use oxygen at this point to help clean the cesium out of the source. If we need an oxygen beam we put in, say, an iron oxide or some oxide cone to get the oxygen beam that way.

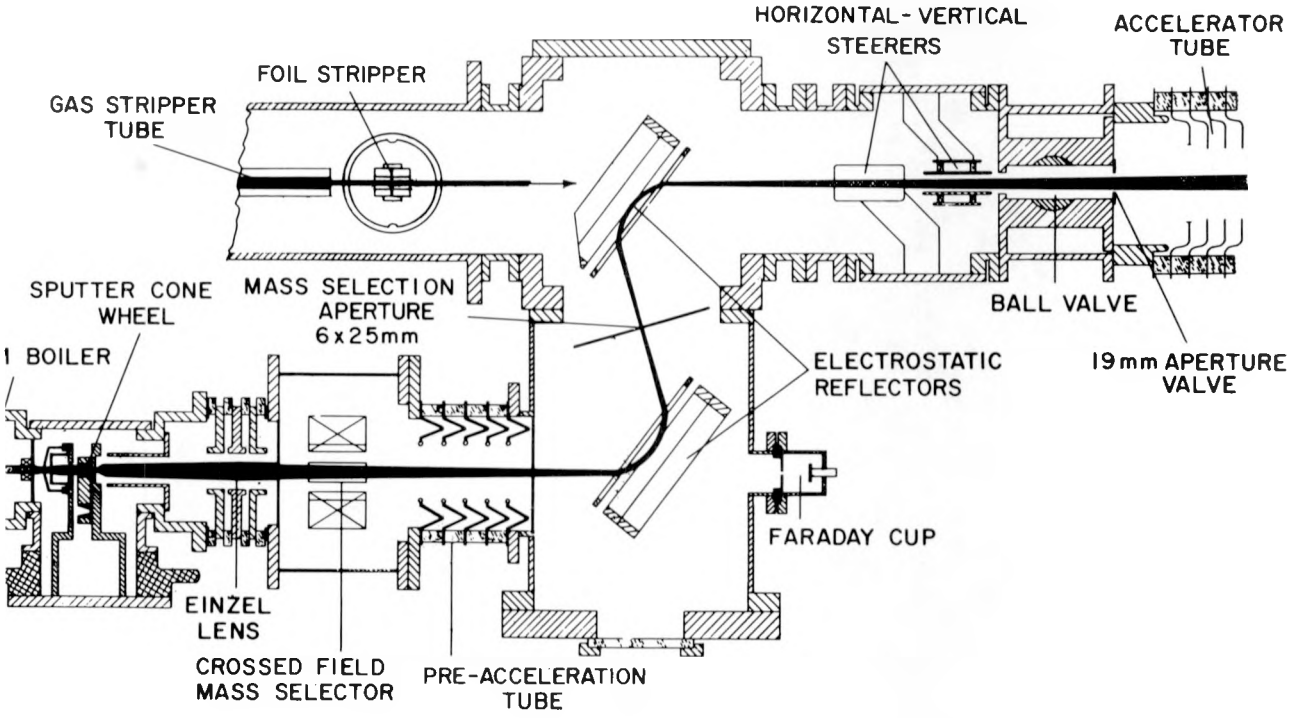
HORIZONTAL- VERTICAL
STEERERS
ACCELERATOR
TUBE
FOIL STRIPPER
GAS STRIPPER
TUBE
SPUTTER CONE
WHEEL
MASS SELECTION
APERTURE
6x25mm
BALL VALVE
ELECTROSTATIC
REFLECTORS
19mm APERTURE
VALVE
FARADAY CUP
EINZEL
LENS
CROSSED FIELD
MASS SELECTOR
PRE-ACCELERATION
TUBE

Fig. 1

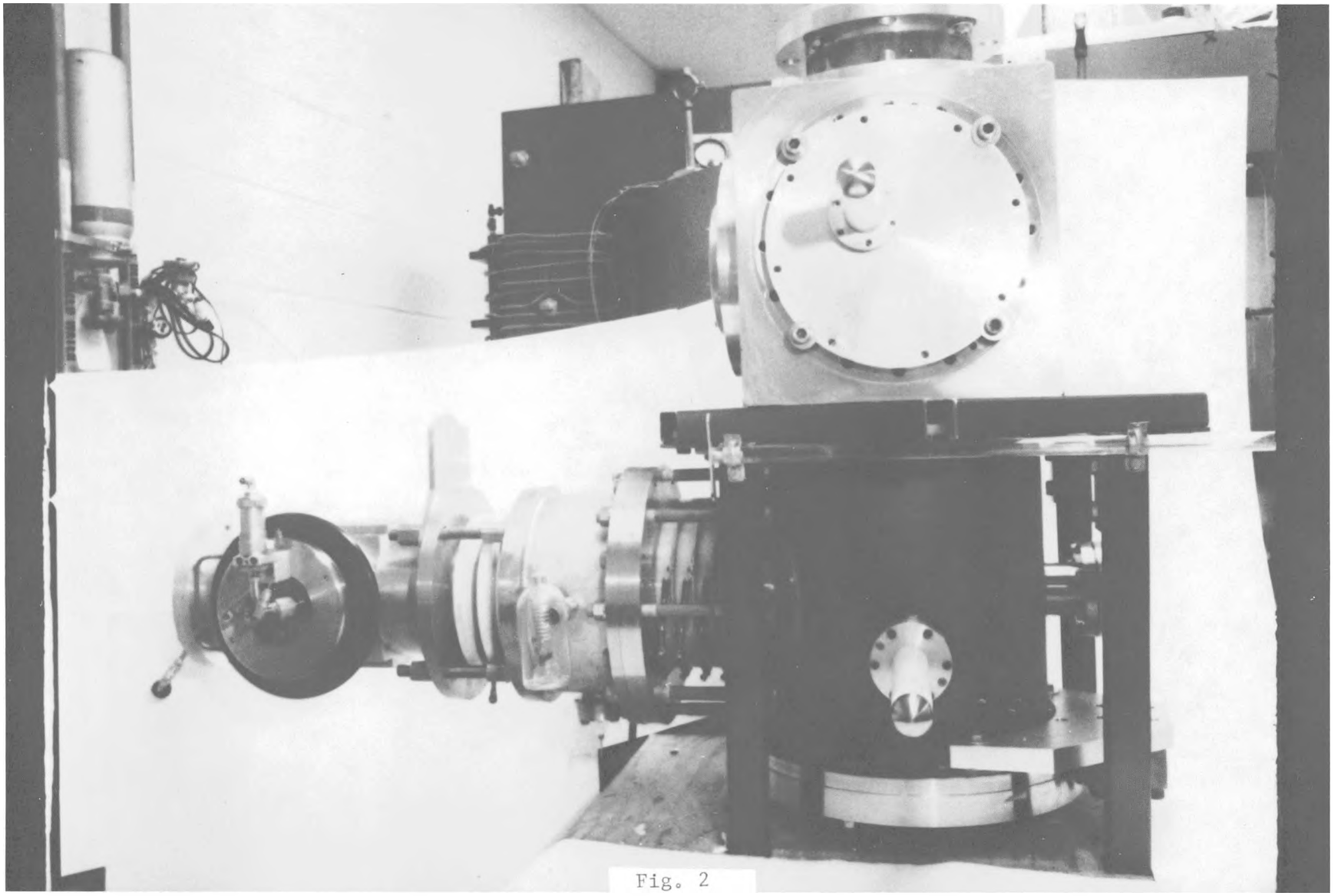

Fig. 2

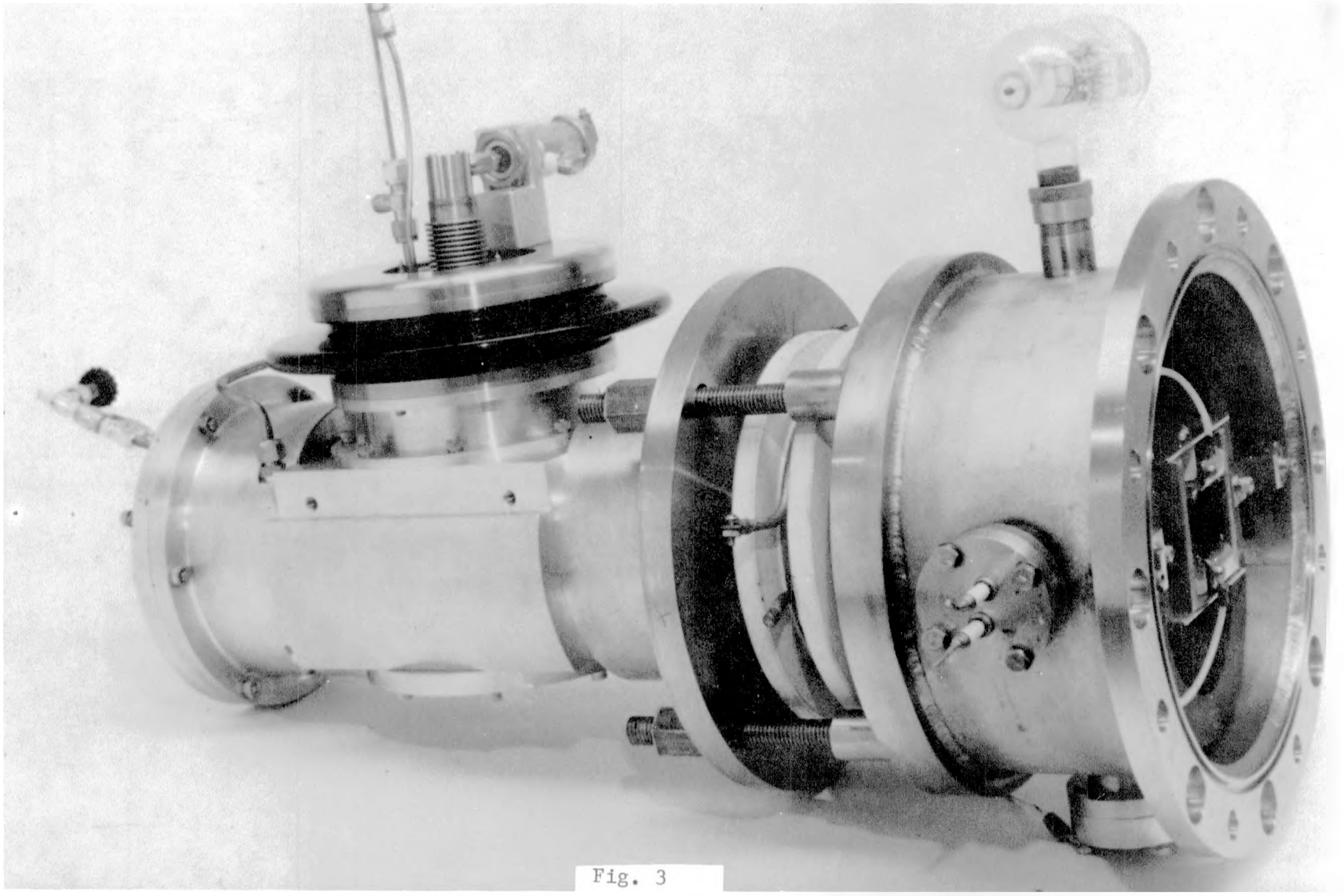

Fig. 3

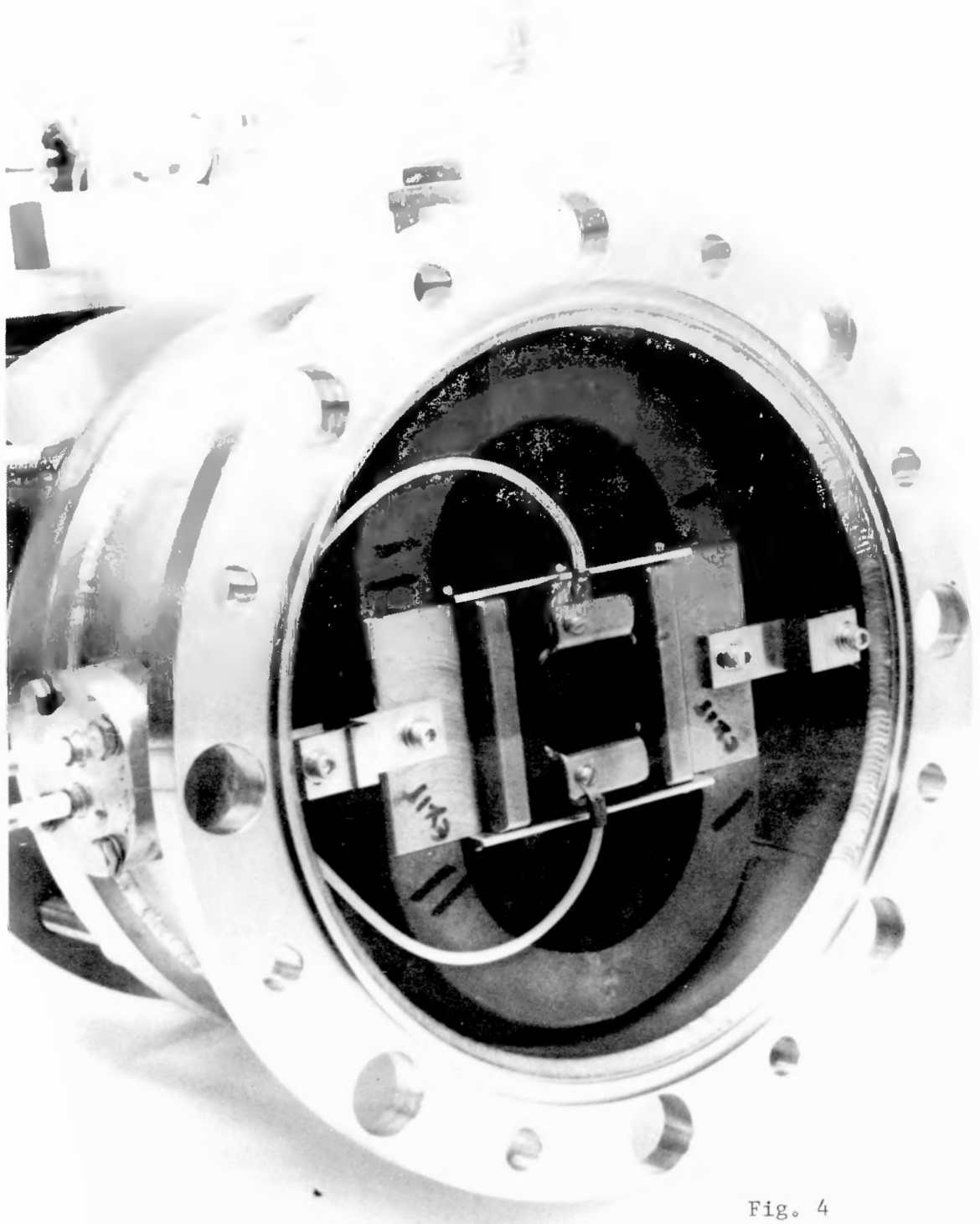

Fig. 4

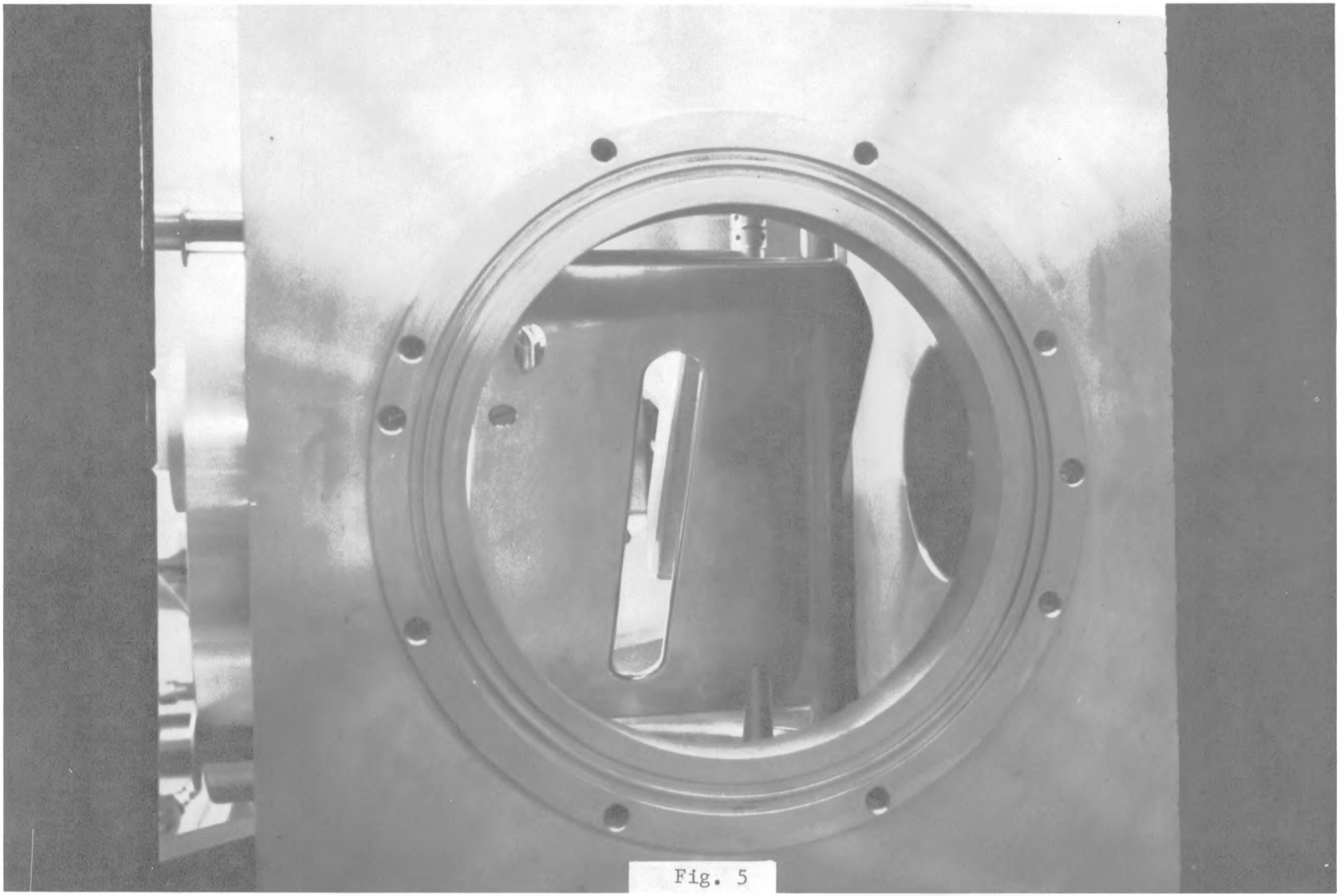

Fig. 5

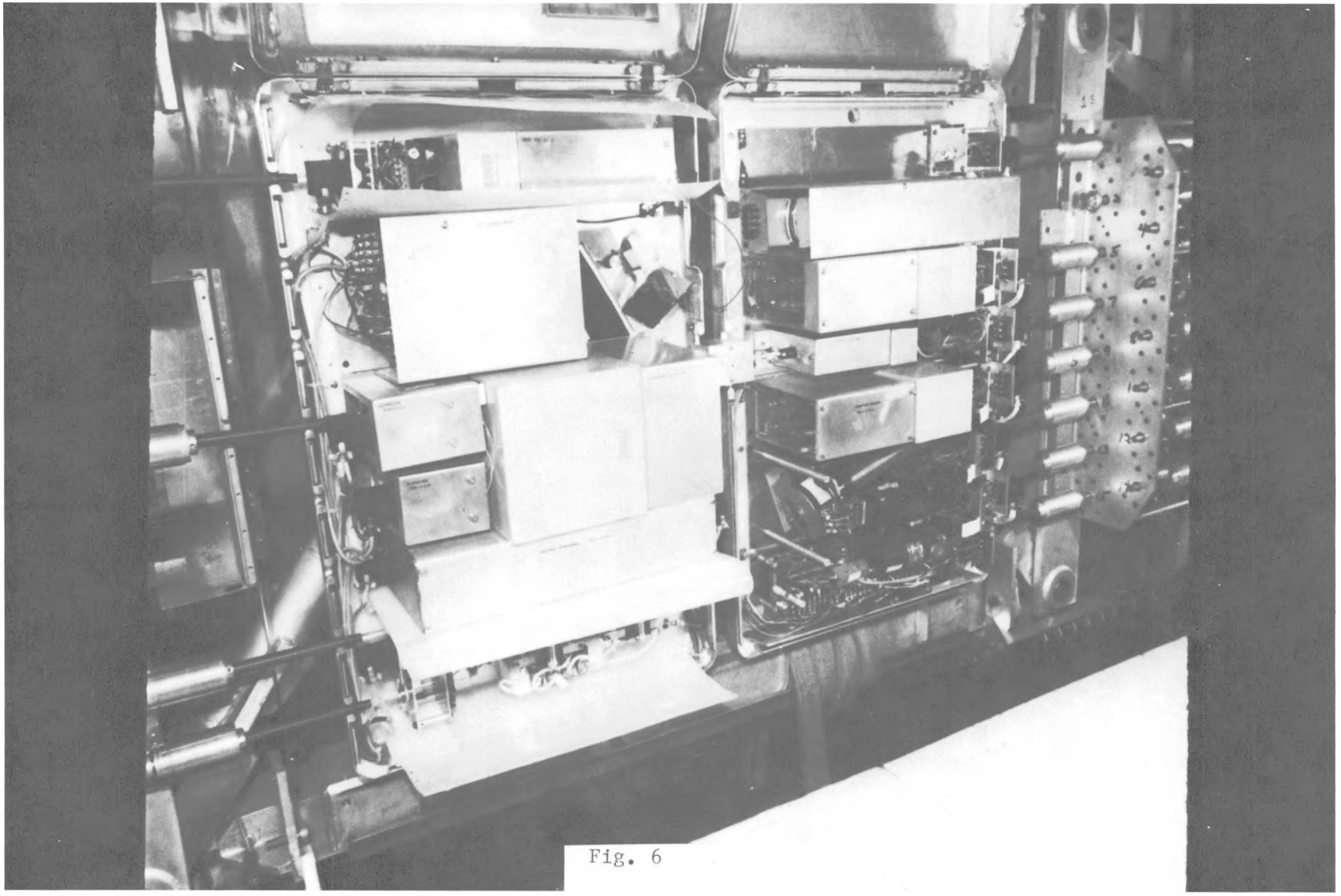

Fig. 6

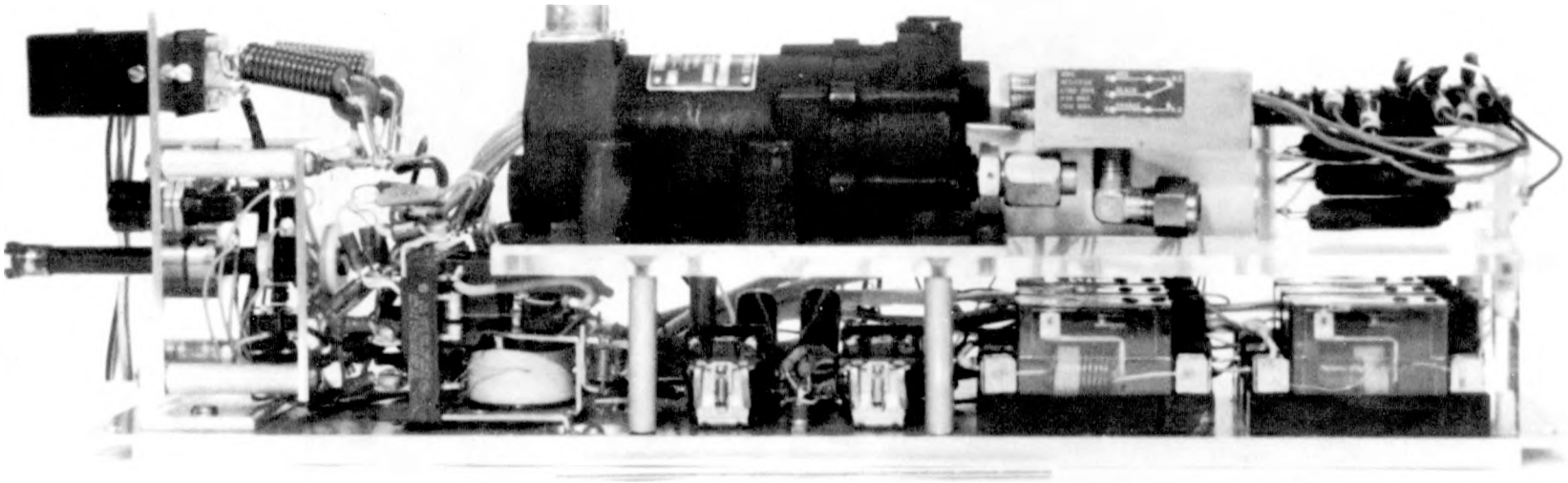

Fig. 7

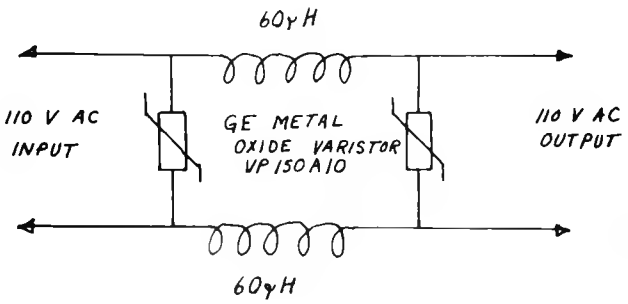

Fig. 8

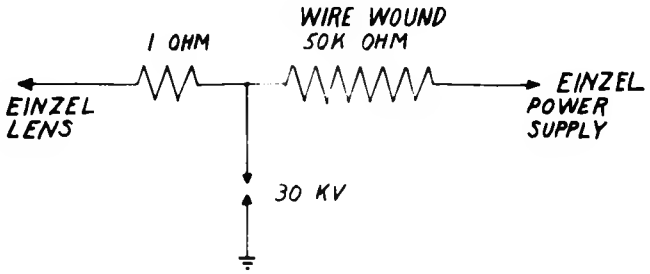

Fig. 9

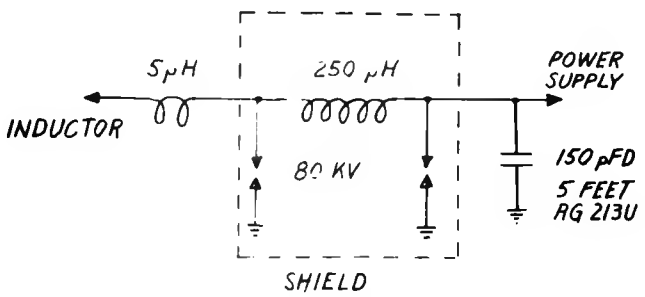

Fig. 10

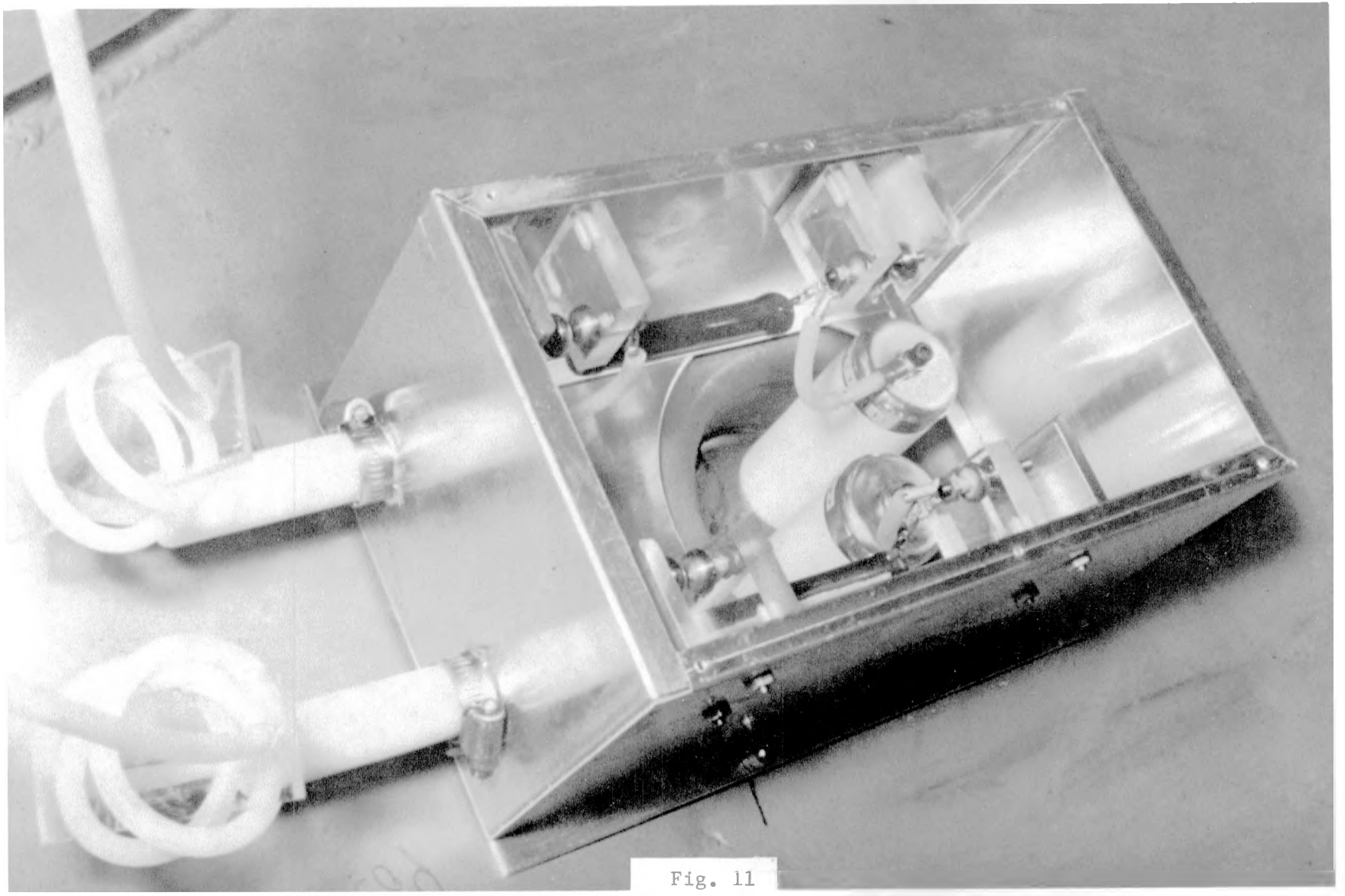

Fig. 11

SYNOPSIS OF TRITIUM RUNNING AT PENN.

Roy Middleton
University of Pennsylvania

Last year at the SNEAP meeting I had plans to accelerate tritium. I am very pleased to be able to report back that this went off exactly as planned and was very, very successful. In fact we started to accelerate tritium on February 14 and finished April 1, Valentine's Day to April fool's day; I don't know if that helped at all. We ran for 735 hours and I think there were just over 600 hours of triton beam on target. I have written up the technical aspects of the run and I have a couple of copies here. This has been submitted to Nuclear Instruments and Methods. If anybody would like a copy maybe we could get a few more Xeroxed. Figure 1 shows the source we used which we would call the standard cesium source. This has provision for 18 sputter cones. These turned out to be quite useful. We were able to put in cones with hydrogen and deuterium that enabled us to line up the system. Also it was very useful to have a carbon cone in because then we could accelerate carbon-two plus and simulate the magnetic rigidity of the triton beam. That proved to be very useful for testing our quadrupoles because everything was strained a little because of the very high magnetic rigidity of the tritons. You will also notice that there is a built in Einzel lens in the source. We have a gridded Einzel lens here.

The actual tritium cone was the result of a fair amount of work. We did quite a large number of tests before we arrived at this rather peculiar cone geometry shown in Figure 2. The cesium beam comes in from this side and the tritium is contained in a small titanium insert that weights about 210 mg and contains about 35 cc of tritium. We developed the methods of filling these cones ourselves but the actual tritium loading was done by US Radium Company. I just can't praise them too highly; they were incredibly cooperative and really did a first-class job. They supplied us with two of these and with a total of about 70 cc of tritium, that's about 180 curies, just about half the amount of tritium that was in the emergency exit signs on the stretched DC-8 we came on.

We supplied US Radium with this little vacuum-tight container and asked them if they would supply the cones inside this thing, but not evacuated. One of the big uncertainties was that the tritiated cone differs from the usual tritium target that is used for neutron generation in that it is a bulk absorption of the tritium. One of the big questions was how much of this tritium would sort of ooze out of the titanium and be in the container when we opened it. So what we did was ask US Radium to supply it in the cones in this container, and then when we got it we connected it to a pre-evacuated 10-ℓ vessel and evacuated it that way. Then we closed this valve off, having reduced the pressure in here down to about a micron or so, and then we opened the container and inserted the cones into the source. There was a slight release of tritium when we did this, I think it was only about 35 $\mu Ci/m^3$. It was a very small amount.

Figure 3 shows a drawing of the entire source system, and by the system I mean the source and the lens, here. For the vacuum system, we chose to use a Ti-Ball and that was supplemented by a small 11-ℓ/sec ion pump. The optics of the

system are shown here and you can see the Einzel lens was designed to focus the triton beam to produce a waste, at this point, in the middle of the gas restriction tube. The purpose, of course, was to try to contain the tritium in this region, here, and get the minimum amount of gaseous tritium into the low-energy end of the machine. In fact, the pumping speed of the Ti-Ball for tritium is calculated to be about 1500 ℓP/sec and the conductance of this restriction to tritium is somewhere around about 1.7 ℓ/sec, so that ratio tells you how much tritium you are going to get in the low-energy end of the machine. Assuming that we released all the 70 cc of tritium that was in the 2 sputter cones, then, through here, we are going to get about 0.1 cc or about 250 millicuries of tritium. To cope with that we added a second Ti-Ball immediately after our inflection magnet. I think, last year when I talked about our plans, we intended to put the tritium source up at potential in our 150-kilovolt injector. But at the very last minute we decided to put the source at ground potential and this makes life much easier. This shows the ion optics that we used and this is a 20-kilovolt beam all the way. We injected it into the machine at 20 kilovolts. This was a little bit of a gamble. I wasn't sure of the transmission but we do have a gridded lens at the entrance to our tube. So before we put the triton cones into the source, we did make some transmission measurements using identical sputter cones containing hydrogen and deuterium.

I was almost surprised by the transmission. We got about 79% transmission with hydrogen and deuterium. Later, when we did put the tritium cones in, we got identical transmission, 79%, after allowance had been made for about 10% losses in the grid, but after 350 hours of operation, shared roughly equally between the two cones, transmission began to fall off. The transmission toward the end of the run was down to about 33%. The reason for that is fairly clear now. What had happened was the hole in the sputter cone, which is a 1/16 of an inch, enlarged to almost a full 1/8 of an inch and I think this had worsened the emittance of the beam. If we do accelerate tritium, again there are two things that we can do to minimize this effect. The simpliest thing is to use an even smaller titanium insert. We may choose to use the same amount of tritium but distributed over 5 or 6 cones using much smaller titanium inserts. As I said at the beginning, the run went quite successfully and we got 600 hours of beam on target. I think something like 140 experiments were conducted with our multiangle spectrograph and a number of t,pγ experiments were done. We had Dave Alberger from Brookhaven as a visitor and he made some measurements on delayed neutrons and, generally, it was quite successful. After the run, came really the worst part, the cleaning-up part. We removed the entire source system as a unit. The solenoid valve was closed and this was removed as a complete unit. Perhaps our biggest mistake of all was in not thinking ahead about how to get rid of this. At the present time we have this rather unwieldy structure with 180 curies of tritium in it and we don't know what to do with it. We have to make a special container for it because there are regulations that have to be met, and if I had it to do all over again, the source, from the very start, would be designed to fit into a standard 55-gallon disposal drum. I think one can make it much more compact, the Ti-Ball can be brought much closer and so on. I think that's really one of our biggest headaches. We have this source sitting there with 180 curies and we just can't get it out of the building. As we anticipated there was tritium contamination and that was largely in the magnet inflection box. Very gratifyingly, the contamination level between here and there dropped very markedly, that second Ti-Ball was really doing its job. The tritium contamination here was about 2 orders of magnitude less than in this region. In this region swipes were giving something like 2 to 10 mCi/cm^2.

We did the run with our old original aluminum tubes. Since we cleaned up we have replaced our tubes with the new high-voltage titanium tubes. We have been able to make measurements on the tritium contamination of the tubes. This really did turn out to be very minimal. Some of the swipes at the stripper region and beyond showed the activity was often not that of tritium. It was a higher energy than the tritium 20-kilovolt β. Really, I think on the whole, it was very, very successful, the contamination was absolutely minimum. We did have to thoroughly wash the inflection magnet box and I think something like 25 to 30 curies were removed from that.

Discussion:

Chapman: Did you remove the second Ti-Ball pump because of contamination, or what did you do with that?

Middleton: In fact, when we removed that Ti-Ball, you know it was fastened with the usual Dependex flange arrangement we did drop the O-rings and part of the inner ring dropped on the floor. We had quite high contamination on the floor. That Ti-Ball clearly had a lot of tritium in it, and we removed it very quickly, capped it off and that's going to be disposed of.

Wegner: In making a crude estimate of the overall contamination of the bending magnet and the second Ti-Ball, and so forth, would you conclude that the calculated amount of tritium migration through your gas restriction agreed with the experimental observation of contamination?

Middleton: No, I think it was probably less. We certainly did not release the full 70 cc of tritium that were in the cones, in other words, our operation wasn't really 100% efficient. Just from looking at the cone, there is a window in the source and we can actually see the cone, I would say roughly 1/3 to 1/2 of the cone has been sputtered away, and therefore, it would be that amount of tritium, roughly 1/3 to 1/2 of the amount of tritium in the source,that was liberated. The contamination that we got through that gas impedance tube corresponds to about 1/20 of an atmospheric cc. Incidently, if one does assume that roughly half of the tritium was used, namely 35 cc, the mean negative ion current was about 1 microamp and that corresponds to an efficiency of generation of the tritium negative ion on the order of 1%.

Session VII, Chairman, C. Adams

CARBON-14 DATING USING A TANDEM

John McKay
McMaster University

The topic I would like to talk about fits in very well with our trip yesterday. I think we are giving up physics and going into archeology. Perhaps you have heard some rumors and talk about this ^{14}C-dating project. We are very interested in following this up at McMasters. I will give a brief sketch of what the system is and what we know about it and get some feedback from other people. I know a lot of people are interested in doing this. A little bit of the history of it--there was an article in Science Magazine by the Berkeley group who were looking for quarks using a cyclotron. Basically, they were using the accelerator as a very high-energy mass spectrometer. They suggested that you could use the same techniques to do ^{14}C dating. The next bit of inspiration, I believe, came from Roy Middleton, suggesting that it was very important to use the tandem. The main contaminant that you have to worry about in ^{14}C dating of this type is ^{14}N. Of course it is rather hard to make negative nitrogen. We thought that it was impossible but we found out that it isn't. By using negative ions to start with, you get rid of the major contaminant. This means that tandems are much better for this sort of thing than cyclotrons. This information got to a gentlemen named Earl Nelson at Simon Fraser University, who is a Professor of archeology, but he is a graduate of our lab and is a nuclear physicist, in fact. He came to us saying, let's do it, at virtually the same time the Rochester people were doing it in collaboration with Ted Litherlin, Ken Purser, and I am not sure who else is involved there. There have been people trying it at Texas A & M. I understand there is interest at Yale and Oxford; I imagine there will be some more.

Basically the idea is this; if you get a high-energy mass spectrometer you get better dispersion so, therefore, this gives you greater selectivity. If you can have selective acceleration you can get rid of some of your possible contaminants. The final thing that makes this whole thing work, as pointed out by the Berkeley people, is the use of a counter telescope to look at particles. Instead of just counting current or counting particles, you run through a counter telescope with dE/dX plus an energy detector. You can pick out the particles you are interested in, against a very high background of other things. We are looking at something like a sensitivity of one part in 10^{13}. Something like that, or better, is what you need to do ^{14}C dating. In our first experiment, which was rather crude, we got a nice plot out and the ^{14}C peak just sits there with virtually no background around it. Our only problem is counting rates in the front counter due to everything else that is coming in. There is no problem in picking out the ^{14}C. There are two advantages to this type of dating. (There are three; one, that keeps tandems busy and that is an advantage to some of us.) The two advantages to the archeologist look to be those. Small samples can be used in the milligram range, they are used to sample in the gram range, so this means that very, very small samples can possibly be dated now or you can take a small part of an archeological find and date it. The archeologists don't like it when you take an artifact and grind it

up. They get very nervous about these things. The second possibility, and I think it still has to be called just a possibility, is that this may turn out to be more sensitive than standard ^{14}C dating techniques. The basic reason is this, instead of looking at decays of ^{14}C, we actually count atoms. If you are very careful, I understand you can get back to something like 60,000 years by standard radiocarbon techniques. From the data gained at Rochester and our own lab, it looks like this is not very difficult with our scheme. We are hoping that we can push it back to maybe 100,000 years. That is still a hope and we have not proved that we can do that.

It is interesting to accelerator engineers because there are all sorts of things that we are learning about operating machines in strange modes. You put the sample in the source, whereas I am used to putting the target at the other end, this seems like a backward start. One of the big problems is trying to figure out the proper ion source. Our first run was done on a piece of charcoal that was stuck in a cone. We did't even grind it up, so it was a lump of charcoal that was in the cone a little bit crooked with the hole off center, this gave us sufficient beam that we could look at ^{14}C. We have tried some other things, we ran this weekend with KBr and charcoal, ground up. We are getting about one tenth of the beam out of this that we would get out of a graphite cone. We want to learn more about this type of ion source so that we can get bigger beams, which makes the counting time reasonable and so we can have consistent sample preparation. It has to be consistent from the accelerator point of view and from the archeologist's point of view, so I think there is a great deal of work to be done on this.

It is a little difficult to stablize on ^{14}C beams. Our beam was 0.01 attoamps, and that's about 1 count per minute, I think. Our amplifiers don't have quite enough gain to pick that up. The technique we used was this; we set up on ^{12}C-scaled things and, when the beam was nicely set up with minimum steering and so on, the ^{13}C fell right in place. If we made that jump, we figured, O.K., you can go to ^{14}C and be reasonably confident. It looks as though you can do this. You now have this phantom beam coming down in the target room and you very, very slowly, swing a detector in. We run the beam, whatever it is, through a gold foil so that we can look at the scattered beam as we come in close, and make sure that we haven't made a few mistakes, and that there is some small beam in terms of Faraday cups but large beams in terms of a detector. I believe that Rochester used gas counters so they are a little more resilient. We were using a 20-micron-thick transmission counter worth about $2000.00, so we were fairly careful about that. We did a number of things to try to clean up the signal. We, of course, went to GVM stablization once we were on the ^{14}C beam. We have a window set on our GVM which indicates when we are right on voltage. This was done for other reasons in running heavy ion beams where you have a lot of beams close together. We took this signal and put it on a very crude pulser which was past the analyzing slits. At any time we were outside of that window, the beam was pulsed up. If we had a bit of roll-off of the machine or something like this, and swung some substantial beam on line towards the target, this pulser protected our detector, and it also cleaned up our signal quite considerably. Last weekend we were operating without the pulser on a cheap detector and this pulsing with the GVM window does clean things up considerably. Now there are some limits to this technique. The first limit is noise in the detector. Now I am trying to distinguish between noise and background. Background is something that sits under our ^{14}C peak. Noise is everything else that is easy to separate out with the dE/dX plus energy signal. If there is too

much of this noise, the front detector is just being kept too busy. It would appear that we can get rid of enough of this just by closing the slits down and replacing the slits at the low-energy end. The Rochester people are using a very precise injection through a 90° magnet, I believe. We have the old 15-degree HVE magnet, and for other reasons, that I will get into later, we don't want to change this. By restricting the inflection slits a little bit, it looks as if we can get our noise problem down quite nicely.

The next problem is background, and, if you get down to the very old samples, you are talking about extremely small signals. It looks as though we can certainly get back further than the current techniques, but I won't claim that we have proven this. There is a possibility of sources of ^{14}C other places in the machine. Since we have the dE/dX plus magnetic deflection by the analyzing magnet, I think any contamination must come from the source itself. This is one of the problems we are going to look at very carefully; just how clean does the source have to be,and how much carbon can you pick up from other runs in the same source or from materials used in the source . This could make dating, going back to the very ancient times, more difficult and a time-consuming process,and we probably have to clean the source in between every run. This has actually been a very exciting thing and everybody in archeology seems to know about it already and we haven't published anything. Now Rochester has a little bit out, but they know about us, too, and we don't know who is talking about us. Science Magazine has accepted articles from Rochester and McMaster. They were posted within two days of each other telling what we have done so far. They have just pulled that out of the normal stream of publication to advance the publication date. Physics Today has been talking to both groups, and other groups, because they want to put something out on it. I don't really recall as much excitment as this in anything that we have done before. It is really moving. The things that we want to do in the future are more work on the ion source to find out how to prepare samples. We want to think about whether the sputter source really is the right source or whether we should go to some sample preparation in the form of gas. We have one idea, which I think is unique to our lab, but things are moving so fast I won't make that claim for sure. We are going to accelerate carbon-12, -13, and -14 at the same time and this is why we say we want a sloppy magnet. If you look at the curvature through the analyzing magnet, the magnet is big enough to do this, providing we put a new chamber in . We have something like a 1-1/2-in. separation of the beam at the exit of the magnet and about 8 degrees divergence. Our plan is to run the three beams out, to stablize on the ^{12}C and count it in the Faraday cup, to count the ^{13}C,and then to put the ^{14}C into the detector. This means we will have a way of stabilizing the machine and instead of making absolute measurements, we will be able to count, to look at ratios. We feel that this will give us a much easier operating mode as we try to get into the production of dating. We are not putting all the 12 and 13 beam down. The trick will be to restrict this to a point that is not causing us too much trouble, but still leaves enough to stabilize on, and it looks as though this should be quite easy. Our inflection magnet really dumps an awful lot of beam down the tube, so that, even when we are running ^{14}C beam on the detector, there is still a very reasonable current at the high-energy cup. It looks as though this scheme should work.

There are two questions that I would really like to have answered. Perhaps there should be some comment on whom I have missed. A lot of people deserve credit in this field and I haven't tried to list them but I know people who know about it should let us know who is in on it. I would like to have comments

from people on what they think we should be doing especially in the ion source region.

Discussion:

Thieberger: I believe we were the first ones to use methods like these, but not for looking for Carbon 14. We tried to look for super heavies by using the modified sample in the sputter source. This was about 1-1/2 or 2 years ago. In our case, we were able to establish a limit, which was not as good as yours, it was of the order of 1-2 x 10^{-10}, and for such very heavy ions the main limitation comes from residual gas charge exchange mainly in the high-energy tube of the accelerator, and also outside of the machine. For the first time we saw a charge exchange consisting of electrons being picked up by the heavy ions as they went down the acceleration tube. Usually, you expect more stripping to take place. That, of course, happened too, but it did not interfere with our count. A small fraction of the heavy ions, which picked up electrons, produced a background which limited our sensitivity to about 1 part in 10^{10}.

McKay: We see this pickup as well. We were hoping to get rid of this by various methods. We realize that, no matter what magnetic rigidity you pick, there is always some particle generated somewhere within 6 to 10 inches of the beam tube that's going to come through. So we have to rely on good vacuum to cut this down a little bit and the dE-by-dX discrimination. We also saw something, that we very tentatively labeled as negative nitrogen coming from the source. We were a little bit leery about doing this, but about two weeks later it came out in Phys. Rev., I think, someone who has been looking for negative nitrogen, in fact, has proven that it does exist. We think we saw that as well in very very small quantities.

Liebert: The group at Rochester is actually a collaboration of Toronto, Rochester, and General Ionex, for the record or whatever. We have gotten count rates from modern charcoal of around 300 counts per minute in the focal plane or counter. That is actual ^{14}C counts per minute. The beam currents we were capable of getting out of the ion source with ground charcoal samples were 2 to 5 microamps. The last time I actually tuned up we were getting, regularly, 3 to 5 microamps, which depends very much on the sample. Very often these samples have large mixtures of other crud and when you hit them with beam they out-gas, substantially. The result is the negative ion yields seem to be depressed. We, at one time, added a liquid-nitrogen trap on the port underneath our target assembly in the 834 Hiconex and it seemed to improve the yields by about 20% or so. Also there was a problem of cross contamination in the ion source. I think, ultimately, people who get in this business in a serious way, are going to need to get rid of the 12-cone wheel, make a modular unit which seals the contamination in and can be removed, and run one sample at a time the way people would in an electron microscope. If the nuclear physics community wants, seriously, to get into this business we are going to need to become analytical chemists in our thinking. It is possible to make a ^{14}C instrument out of a substantially simpler machine than the type we are using at Rochester, which is the world's most expensive mass analyzer. We are, at the moment, designing a system which uses a small tandem of 1 MeV. Then one has to solve the detector problem at the other end, since the traditional gas counters we were using won't function, at least not with standard windows, and gains that we have at the moment. At Rochester, we currently have the magnetic analysis which we are using on the beam transport system and electrostatic

analysis as well. There are substantial advantages in taking out some of the stages of magnetic analysis and putting in velocity separation in terms of a crossed-field analyzer. We are looking into a cost-effective system. Cost-effective means that we are planning to sell it to fit in somebody's laboratory room. This is General Ionex, not Rochester, and we are talking in the ball park of about a half-million dollars. That will give you an idea of what kind of money you need to build an installation of this sort.

McKay: There is quite a bit of confusion in archeological circles. Earl Nelson has been putting together a standard radio carbon lab out at Simon Fraser University and has a 10-ft -square hole in the ground he is using for shielding. When he came back from McMaster and told his fellow archeologists about this great system and how the tandem looks, as though it were really going to work, they said, well, if we get one could we put it in the same shielded room? There is a real communication gap between the two fields here.

Wegner: It sounds like possibly the background problems would indicate that ultra-high vacuum acceleration systems might decrease this considerably, like the vacuum they try to achieve in the NEC machines. There aren't any in the U.S. but it sounds like a golden opportunity to pack up some of your specialized gear and technology and visit Weizman or Canberra to see if you can push the background down by an order of magnitude in a very high-vacuum-type accelerator. Has this been considered or are any of these other groups doing this work?

McKay: This is why I wanted to distinguish between noise and background. The stuff that comes out of the tube is noise. The dE-by-dX counter moves that right out of the ^{14}C beam. The problem is the ^{14}C that might come from the source,and I think that we have got to look at the vacuum there. But, as far as the stuff that comes out of the tube, it just keeps the counter busy so it decreases our counting rate. It is simple to distinguish between what comes out of the tube and what really is ^{14}C, but I would like to go to Canberra too.

Thieberger: I just wanted to mention that we used a very crude crossed-field analyzer in addition to the magnetic analysis system, and at least from our experience, it seems that you really need both and some more stages if you could have them, too.

McKay: We are thinking about it but we haven't reached the point of really planning that .

Broadhurst: I was just going to ask if you are using oil diffusion for pumping or oil-lubricated turbines, and, if so, what the archeological age of the oil was in the residual vacuum.

McKay: We have silicon oil in the ion source area, but if it's a petroleum derivative there should be no ^{14}C.

Broadhurst: Yes, but it can also be synthetic.

McKay: I figure that we are probably a year of fairly intense work away from starting to do useful work. We can start giving dates and saying we get the same data as you do for conventional techniques, but, really getting down to believing our work, I think there is a long way to go. I am thinking in terms of a year and we are asking for money to start building specialized machinery to

look at these things. We are wondering if you get ^{14}C out of the foil and where does that foil originate. Can you have a kinematic collision and get something out of that, which could contaminate things? These are all questions that we don't know as yet. The Rochester people, I believe, tired a zero-age sample or at least an infinite age sample. We haven't done that yet. We have to have a lot of runs that way.

Chapman: Just one short, perhaps obvious remark, it would seem for this work either the inverted-or the reflected-type sputter source would have a great advantage, in that you don't have to make a hole through the cone or line a hole through a cone, you can use just a small pellet which would be much more convenient, I presume.

McKay: This is one of the reasons Phil is working on the reflected source and it's in the shop. We are hoping that will be a big advantage, yes.

Liebert: I would like to make a couple of comments on background. We have actually attempted to locate where the backgrounds are coming from. We have, in fact, dated some samples which were supplied to us by people who dated them with traditional ^{14}C dating methods to 30,000 years, which is the usual practical limit. You can push a little further but 30,000 is the one that people who are skeptics in the field accept. We actually established our results with a very crude ratio-metric technique, in the sense that we looked at the ^{12}C beam at the source, and switched off and put the ^{14}C beam through the machine. We were able to generate an analytical curve which agreed, within the error limits of the ^{14}C dating technique, with the dates that they had established. We were stunned, as we expected to be off by orders of magnitude. I think that's very promising and especially if you can date some of the older samples that way because that indicates that your background contamination in the source is not bad. Now we have, in fact, taken graphite, which is old carbon, and we have put it together with an organic binder in the traditional way that is used by many labs. This is a Nicrobraze binder which we assume comes from petroleum derivatives and, therefore, from old carbon. As the oil crisis gets worse people may start using plants to generate their hydrocarbons and maybe that will become an impossibility. We found that the sources of background that were in the source were, in fact, not coming from the cone so much as the material on the wheel. I also would like to caution people about using cesium that hasn't come through a tungsten frit. Tungsten is a getter for carbon at those temperatures and there is a possibility of carbon in the cesium that one has to worry about. At these intensities a primary carbon beam could be a disaster. It is something that one should worry about. We actually scanned the beam around the outside of the cone in a grid and found that there were hot spots of ^{14}C, which had been produced by cones that we had run, that had ^{14}C in them. The selection of the materials that one puts in the cone wheels is extremely critical, I think. One has to be very careful with the pill cones because the beam comes back and you have to know for sure exactly what the beam is hitting. I think this business that Roy talked about of getting a sharply focused spot is essential, if one ever wants to do this work successfully. One is probably going to need baffling and take care about eliminating beams which are coming from nearby materials in the cone wheel. At Rochester right now, I think that a run has just ended in which we are attempting to verify the consistency of previous results and we are thinking of methods of baffling between the cones in order to prevent cross contamination. At one time we ran with an enriched ^{14}C set of cones, when we first started out

and I would not recommend that. It was a disaster. The reason for that is, here is so much ^{14}C around that one can never use the ion source again. In fact, that was good for General Ionex--Rochester bought another source to do the dating work. It was not planned, by the way. I think it is really essential that one not be very cavalier about the amount of ^{14}C that is in that source. We could not remove the contamination successfully by normal cleaning techniques. I am not sure why.

Middleton: Recently, we analyzed the cesium beam from the surface ionization source. The motivation was that I was very curious whether any molecular ions come from it. Could you get a cesium carbon beam in the cesium beams for example? It turns out that the beam is incredibly pure, in fact, the only contamination we saw was at the level of about 1 part in 10^4 and, not very surprising, they were sodium and potassium. The cesium contained a little of both, I think, and something like two parts in 10^5 of rubidium. There were just no signs of cesium oxide, cesium carbide, or any molecular beam, and certainly no dimers or Cs^{2+}.

Roth: I am interested in finding out what people do to switch between the ^{12}C or ^{13}C and the ^{14}C beam. One of the things that we thought of, for example, was the vacuum can change substantially when you run a large beam through in certain areas and also your focusing parameters and things like this can change.

McKay: In our case we scaled the inflection magnet, quads, the analyzing magnet, and also the Einzel lens and it seems to go from 12 to 13 quite nicely and 13 to 14. I think the problems that you are talking about are very real and this is why we want to run the three beams at the same time. Archeologists do strange things. They put in modern carbon, which is defined as carbon from the year 1870, and use this as your standard, then put in your unknown. I think that it has to be a technique like this that we will use and I think we will have to have all three beams to make sure the conditions don't change. We are hoping, as long as the 12-to-13 ratio remains reasonable, that we can then make the assumption that the 13 to 14 is reasonable. Of course, we will have to run known samples to prove this, but it looks as though this is the way to go so that you cancel out all these changes with time in the machine.

Roth: We thought about running at least two beams but then you are more susceptible to heating and changing things because you are running at least two of the beams not on axis.

McKay: If you have poor injection selection like most of us do, you are running three beams whether you want to or not, so it's a question of whether you let them get past the analyzing magnet. What we want to do is, instead of just dumping them on the side of the chamber, take them through the analyzing magnet and measure them.

Larson: There is no reason why you can't run the three beams on axis and still separate those three from other masses. It takes a little bit of design at your source, but you can build a filter that will take out, essentially, everything else.

McKay: We have thought of 0° injection plus a Wien filter, and that's definitely on the books to look at.

Larson: You could use a system of magnets where you disperse the beam, select out a central portion that you are looking for centered at mass-13 in this case, and bring it back on axis.

McKeown: We have been pressing our cones with rather high pressures, say 5-15,000 psi, and we find that we don't need binders. Some carbons, when we press that hard, become very hard and almost glassy, so that might be one way of getting an easy cone.

McKay: What material are you starting with, ground charcoal or lampblack.

McKeown: Exactly. We have tried ground charcoal and lampblack. I think one thing that we had was bone charcoal and that was unbelievable when we pressed it out. It came out like a black glass.

McKay: That sounds like it might be the way to go. We have been pressing cones but not with very high pressures.

Adams: Do you heat them, Mike, when you press them?

McKeown: No, we press them cold, heating would be another step. The other thing we did, we had to make some sodium choloride of separated isotopes. We only had a little bit and instead of making a cone we made a small cylinder, roughly a 1/16-inch hole, and it had about a 150 mg of sodium chloride in it, that worked as well as an ordinary cone.

Middleton: I would like to make two comments. One, there is bound to be an isotope effect in the electron affinity and the ^{14}C is going to be different from that of ^{12}C. I don't know what this is, but the electron affinity may well be 10 to 20% different and, therefore, the efficiency of generation of ^{14}C negative ions will be different from ^{12}C and ^{13}C. At the Strasbourg conference, Heinemeier, from Aarhaus reported some charge exchange cross-section measurements for carbon in sodium vapor and observed sodium vapor. I think that this was at 5 to 10 kilovolts. Although I am somewhat enthusiastic about sputter sources, an alternative approach, maybe a very good approach, is to use a charge exchange source. Most ^{14}C labs prepare their sample in the form of carbon dioxide or benzine. I really don't know how much carbon plus one might expect to get from the duoplasmatron operating on carbon dioxide. We are at the point of measuring this, but if anybody has any information, I would be interested. Many years ago we reduced our negative carbon beam by using a mixture of methane and helium or nitrogen in the duoplasmatron and charge exchanging in lithium. We were certainly able to get 2 or 3 microamps, if not more, of carbon.

McKay: We have tried running carbon dioxide in the charge exchange canal. Our best figure analyzed was a little over a microamp but, unfortunately, we then overhauled the source and replaced the worn-out parts, and so on, and now we can get a few hundred nanoamps out. It would appear that you must have a large worn-out aperture in order to get the carbon dioxide out, it does crude up the source rather badly. We think this would be a nice way to go. This is one of the other things we are looking at in ion sources.

Liebert: I would like to ask Roy if he knows what intensity of carbon beam you can get by bleeding the CO_2 into the Hiconex or the Middleton version right

beneath the target, on a target that is not made out of carbon.

Middleton: I don't know.

McKay: When we tried that we didn't get very much at all. I suggested that we try this and our target maker said, "I don't think that this is going to work," and I didn't have enough time to find out why. But he was right and I have to get back to him and find out what his thoughts were.

Middleton: Some work was started some 3 or 5 years ago at Stanford Research Institute to try to detect ^{14}C with a mass spectrograph. Their approach was rather interesting, they used a direct-extraction duoplasmatron which was set on, I believe, CO_2 gas. The idea was to extract a $C^{15}N^-$ which is a very prolific negative ion, but, of course, this would be ^{14}C and ^{15}N or mass-29. It was thought that there would be very little contamination of mass-29. This turned out not to be right, I mean, there is contamination. You see, here is another possibility for acceleration in the tandem that one might be able to accelerate the $^{14}C^{15}N$, or $^{14}C^{14}N$ for that matter.

McKay: I don't think I would like to see that much ^{14}N around. I think that we can distinguish between the two in the counters, but I think that might tend to lead towards loading of the counter; that's something we have not thought about at all.

PROPOSED UPGRADING OF AN EN TANDEM FOR HEAVY IONS

W. Laughlin
University of Pittsburgh

INTRODUCTION

The general goals of this upgrade are to increase system performance and to minimize system downtime for implementation. The specific goals are to add local pumping to improve the vacuum at the terminal and accelerating tubes and to increase the number of stripper foils. In order to achieve these goals, there are certain requirements which are:

1) Provide power in the terminal. This has been achieved by the installation of the 3 KVA alternator;
2) Rebuilding of the terminal canal to accommodate the foil changer(s) and the local pump(s);
3) Design of the titanium sublimation pump(s);
4) Necessary control for the pump(s) foil changer(s).

STRIPPER/PUMP CANAL

The stripper/pump canal interfaces the pump(s) and the foil changer(s) to the low energy and high energy accelerating tubes as shown in Figure 1. The canal provides six 4" standard dependex ports, two 6" standard dependex ports and a 29" x 6" access port on a 10.75" ID x 33" long stainless steel cylinder. The 6" ports mate to the accelerating tubes and the 4" ports are for foil changer(s) and the sublimation pump(s). Figure 2 shows a cross section of the EN terminal. The foil changer(s) will be mounted at 90° to the beam axis extending from the accelerating tube axis towards the center of the column. By using this geometry, the foils have minimum sensitivity to pressure transients along the tubes and more than one changer could be installed in the terminal. Similarly, the titanium pump port(s) are 90° with respect to the beam axis pointing towards the bottom of the machine. The pump can(s)

extends towards the bottom and its diameter was determined by the distance to the terminal shell. Since pumping speed is related to the diameter of the can, one would like to maximize this dimension. In this system, the can O.D. will be 5".

TITANIUM SUBLIMATION PUMP

Figure 3 is a drawing of the proposed titanium sublimation pump. Since this type of pump is conductance limited, the port should be as large as possible. The port is 4" to the canal. The diameter of the can is 5" and will be constructed of mild steel since the titanium adheres to this material best.[1]

The titanium sublimation assembly will be made from NEC 1-C cartridges consisting of a one inch cylinder of Ti and a heater.[2] The assembly together with the can make a better pump employing production of chemically active titanium film on the can wall by direct sublimation from a radiation heated Ti source. Based on some preliminary calculations we expect to obtain a maximum pumping speed of 300 liters per second for N_2 at 20°C when the system pressure is in the 10^{-5} torr range. For this maximum, the sublimation rate will be approximately 0.05 grams per hour and require an input of approximately 300 watts.[3] We are in the process of making some pumping speed measurements to see if the sublimation rate and this geometry agree with the predicted value. In the proposed prototype system, one of these pumping systems will be installed.

FOIL CHANGER

The foil changer will be a NEC FS6-46 modified by NEC to hold 230 foils.[4] Some features of the changer are mechanical detents to get the foil position accurately in the beam line and magnetic coupling across the

the pressure interface. The foils are mounted on a metal sprocket belt and are loaded from the rear flange.

There is one modification which we will make to the changer and that is to place an index marker on the metal belt to give an absolute position. One way of doing this is to insulate a small section of the belt and allow a mechanical contactor to ride on the belt. We will be able to identify an absolute position when the contactor opens because of the insulated section.

CONTROL

Figure 5 shows the crude control for the prototype system. An existing stepping motor and lucite rod will be mechanically coupled to an auto transformer which in turn sets the power for the titanium pump. The addition of a set of strings which are "driven" by a linear solenoid and a snap action switch acting as the "receiver" form the communication channels for the foil changer. One channel is used to command the changer to advance one foil position and the other to send the absolute position to the outside. The relative foil position will be presented in a three decade display and the absolute position by the index status. The procedure for knowing which foil is in the beam line will be:

1) Determine the absolute foil location (indicated by the index status being true);
2) Reset the relative three decade counter to 000;
3) Advance the foil position as desired. Each command to advance the foil into the beam line also increments the relative foil counter.

Thus there is a one to one relation between the foil in the beam line and the number displayed. In this arrangement, though not optimum, we will have the

ability to always find the reference foil by searching for the index status.

The shortcomings of the control and acquisition system are obvious. There are practical limitations to the amount of information being passed to and from the terminal by using the strings and lucite rods as the communications medium. To control one pump cartridge, control the foil changer and receive one bit of information, the index from the foil changer, requires elaborate mechanical linkages. In addition, valuable information could be obtained by monitoring the canal pressure, sensing the titanium cartridge heater current, and to have the ability to extract more information concerning the foil which is in the beam axis. If it turns out that the foil changer and the local pumping enhance the system operation, the next step is to add another foil changer, add more cartridges and/or pumping systems to minimize the tank openings. It is obvious to me that mechanical linkages to and from the terminal for control are not satisfactory and sensing parameters at the terminal is just about impossible. By the time that this prototype system is implemented and evaluated, the reliability of the injector telemetry system will be known.[5,6] If the reliability is satisfactory, then a possible telemetry system for communications to and from the tandem terminal would be like that shown in Figure 6.

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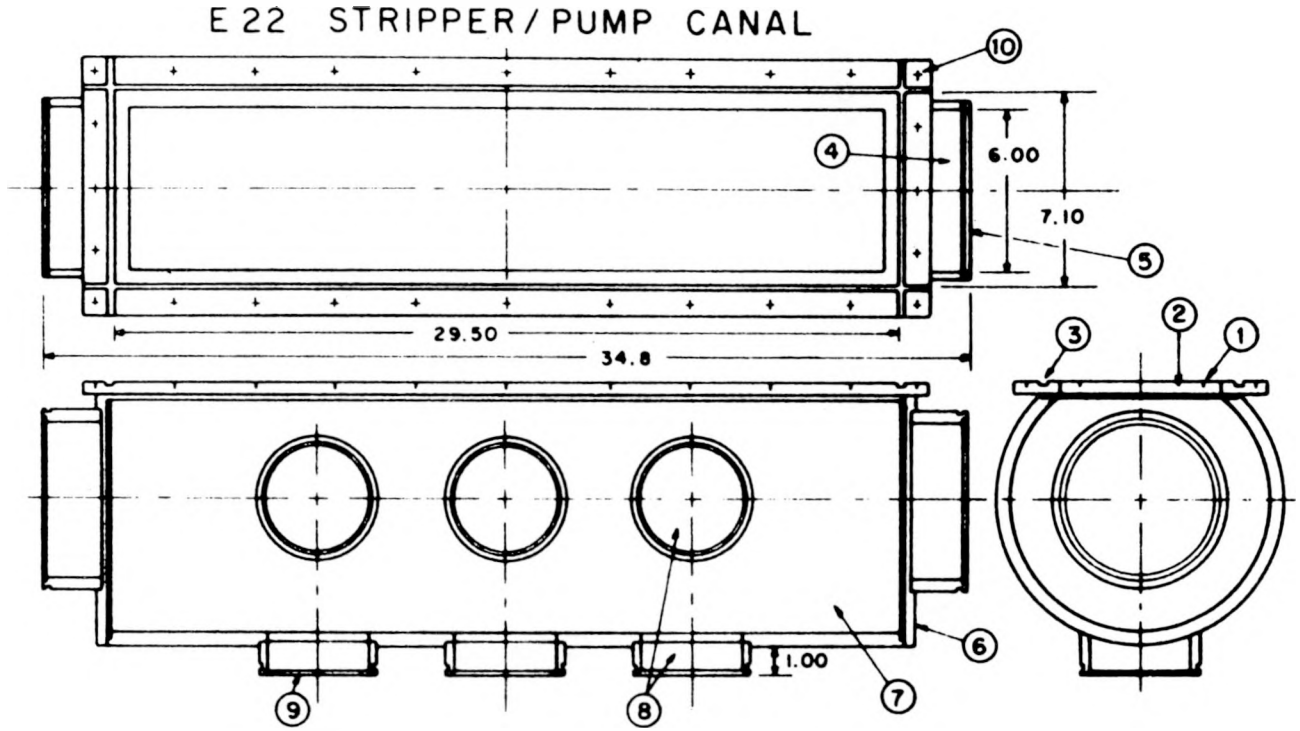
1. W. S. McEver et al., Nucl. Inst. Meth. 126 (1975).
2. National Electrostatics Corp NEC Sublimation Arrays.
3. National Electrostatics Corp TSC-10 DWG No. 7-818.
4. National Electrostatics Corp NEC FS5-46 Foil changer.
5. W. Laughlin, S.N.E.A.P. Conference, 1976 Tallahassee, Fla.
6. J. G. Alessi, W. Laughlin et al., Nucl. Inst. Meth (to be published 1977).

Discussion:

Lund: A comment on the NEC foil changer. There are all kinds of schemes, I guess, to keep track of the absolute position. We tack-welded in a beam stop, something thick enough to stop the beam, and on the next position, a very small hole, about a millimeter, so that we could look at the beam in the high-energy cup and go around, if we think we have lost track of where we are. The beam stop is absolute, the beam must go to zero and the small hole, the pinhole as we call it, is a very small beam, just a few nanoamps. It is a little complicated because we have two of these foil changers in our machine, like most of the MP's, but that system seems to work fairly well for us. We have also installed what I think is quite a nice isolation valve for the terminal foil stripper, which was mentioned at the Strasbourg meeting, but perhaps, everybody doesn't know about it. It's a cup-shaped valve that can come in over the end of the foil changer to isolate it from the vacuum system and you can take the foil changer off to change the foils without letting up the accelerator tube vacuum. I think the advantages over the ball valves are,first of all, it is quite a bit simpler to put in, and secondly, I think it is more reliable. Most of the people who have ball valves tell me that they leak from time to time. This is just a flat seal O-ring, also, it is very easy to put into the dead section. The ball valves are not so easy to put into the dead section if you have a midsection stripper.

Hurley: We have had titanium sublimation pumps in the terminal for about five years now, and we have two 90-degree angles between the stripper housing and the Ti-Ball pump. In the event of a sudden bleed up to atmosphere, the titanium flakes that form in a pump will not get blown around, or it decreases the probability. We are using a Lucite rod, as you are, to drive the Ti-Ball. We are also using a Lucite rod to turn the foil changer and we have modified the NEC foil changer by putting another foil holder in between each of the original 110 foil changer positions. We also changed the size of the roller to get the adjacent foils over the beam path and it's working pretty nicely.

Schultz: I would also like to say that we have an NEC foil changer and we have a solid control so that we can microswitch off the low-energy end to get the indexing read-out. It's a 100 foil holder and we mount 90 foils leaving every tenth hole open to make it faster to get to gas stripping and it also helps to check the indexing.



1 - 1/4 × 20 TAP × 1/4 DEEP

2 - 0.625 # 304 SS PLATE

3 - CUT FOR 23 1/4" I.D. × 3/16 O-RING

4 - 6.625" O.D. × 0.432" WALL # 304 SS PIPE

5 - 6.00" STD. DEPENDEX FITTING

6 - 0.500" # 304 SS PLATE

7 - 10.75" O.D. × 0.500" WALL # 304 SS PIPE

8 - 4.50" O.D. × 0.337" WALL # 304 SS PIPE

9 - 4.00" STD DEPENDEX FITTING

10 - 1/4 × 20 BORE

Fig. 1

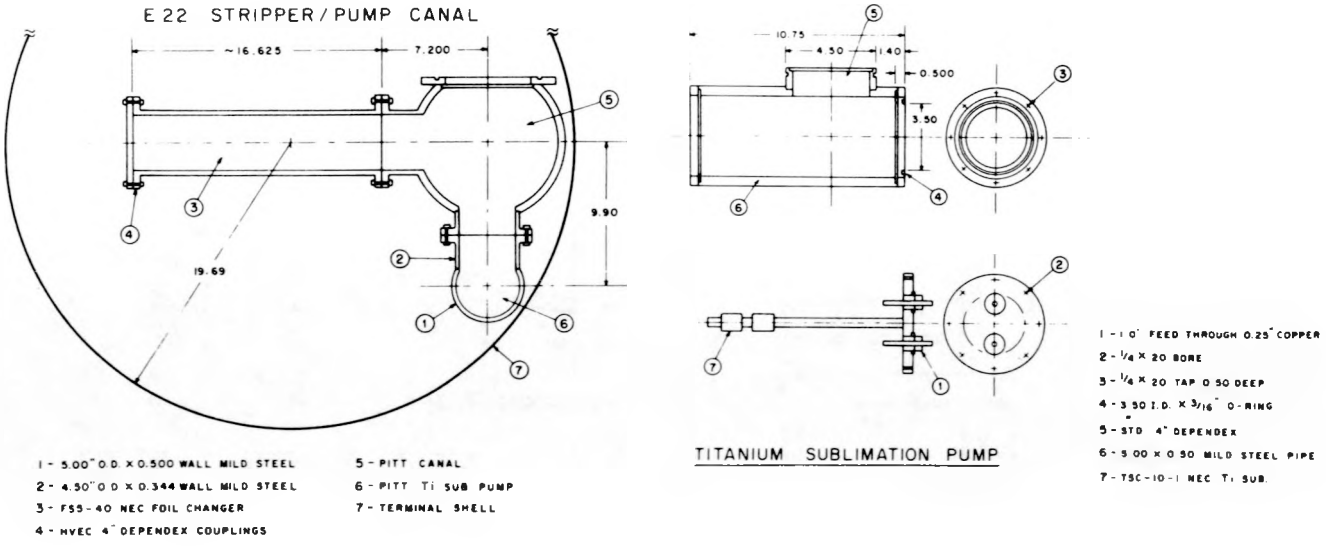


Fig. 3

Fig. 2

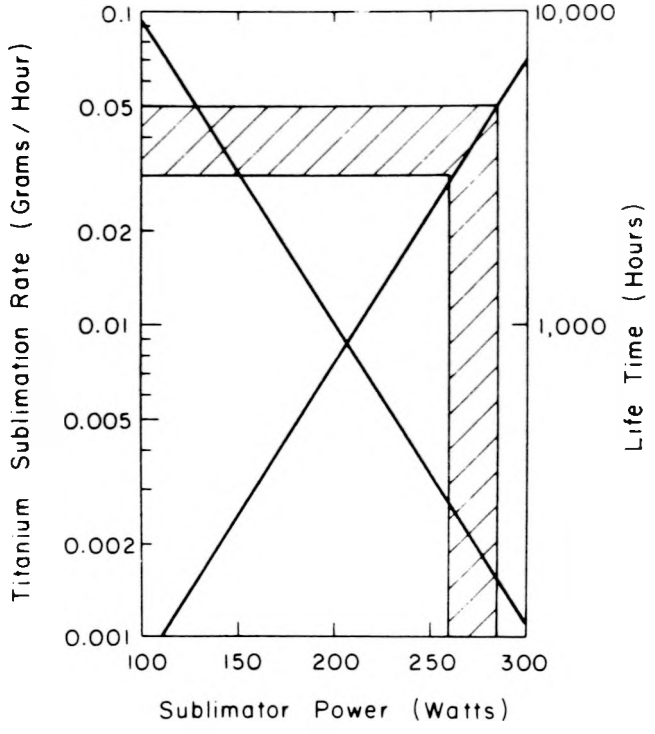
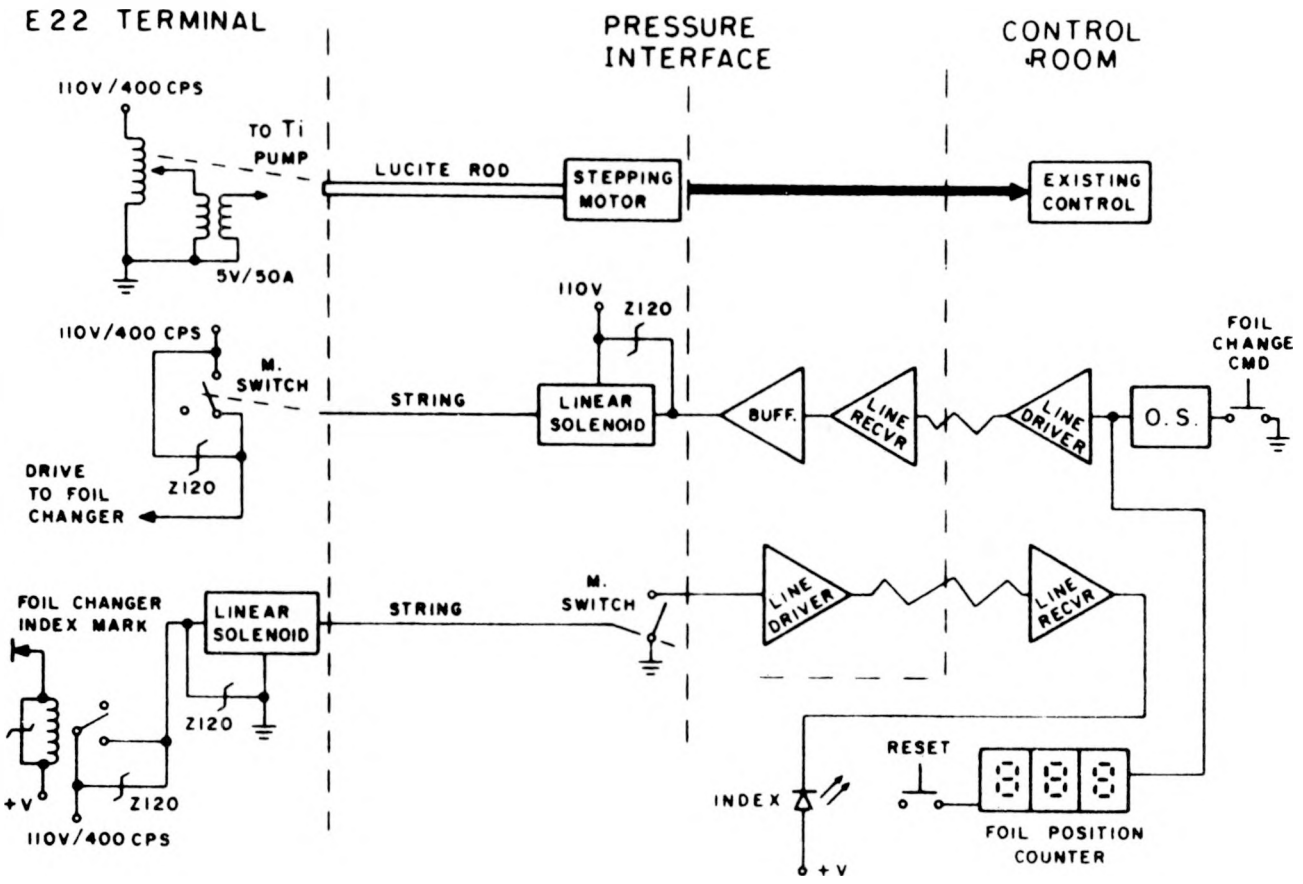


Fig. 4



E22 STRIPPER / PUMP CONTROL

Fig. 5

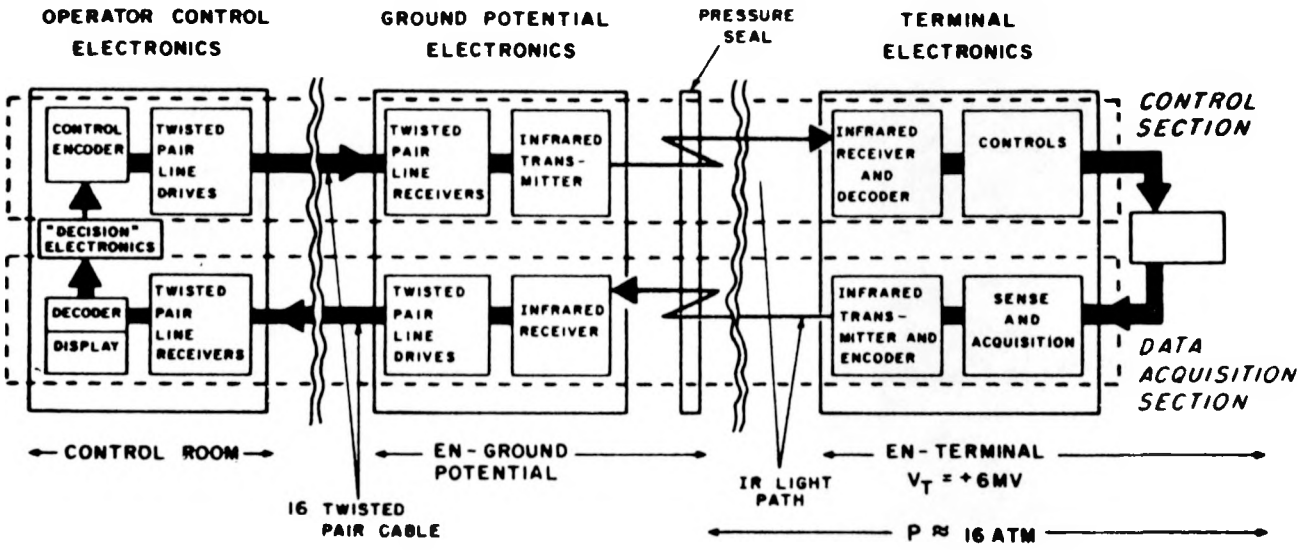

OPERATOR CONTROL ELECTRONICS
GROUND POTENTIAL ELECTRONICS
PRESSURE SEAL
TERMINAL ELECTRONICS
CONTROL ENCODER
TWISTED PAIR LINE DRIVES
TWISTED PAIR LINE RECEIVERS
INFRARED TRANS-MITTER
INFRARED RECEIVER AND DECODER
CONTROLS
CONTROL SECTION
"DECISION" ELECTRONICS
DECODER
DISPLAY
TWISTED PAIR LINE RECEIVERS
TWISTED PAIR LINE DRIVES
INFRARED RECEIVER
INFRARED TRANS-MITTER AND ENCODER
SENSE AND ACQUISITION
DATA ACQUISITION SECTION
CONTROL ROOM
EN-GROUND POTENTIAL
EN-TERMINAL
V_T = +6MV
16 TWISTED PAIR CABLE
IR LIGHT PATH
P ≈ 16 ATM

Fig. 6

PROGRESS ON THE OAK RIDGE TANDEM

Joe Bair
Oak Ridge National Laboratory

I have a few slides here of the construction work that has been going on at Oak Ridge. Figure 1 is just a general view of the accelerator tower as of a couple of weeks ago. Figure 2 is a picture taken from the top of the ORIC building, the cyclotron building, showing the subassemblies of the tank. The tank was made by Chicago Bridge and Iron. They trucked in the curved sections. To each of the cylindrical sections you see there, there are about four subsections. They then welded these up into the cylinders that you see in Fig. 2. Figure 3 shows a picture of the tower and one of the end sections going into the tower. The crane that you see there is a 350-ton crane. There are no larger portable cranes in the world. This crane arrives on its own rubber tires followed by many trucks full of pieces. Figure 4 shows a section going into the tower. Figure 5 shows it about to go into the tower. Putting these sections into the tower was a very expensive operation for the Laboratory. Great herds of people appeared from nowhere to watch. Figure 6 shows what it will look like in the future. Thank You.

Discussion:

Wegner: How are they holding the cylindrical-accuracy specification on the i.d. of the tank during the assembly?

Bair: The specification which NEC set is one half of the ASME specification and it looks like they are meeting that very nicely--or did you mean physically?

Wegner: Did they have a support spider inside that they adjust so that the sections are perfectly round before they weld them up so the tank doesn't get flat areas in it and things like that?

Bair: I have no idea.

Norton: The sections that are welded together are already rounded in the Birmingham plant so all they have to do is make sure these individual curved sections meet very exactly. I don't know exactly how that is done.

Larson: Greg may think that they were very rounded in the plant but he didn't see what happened to them when they were being lifted.

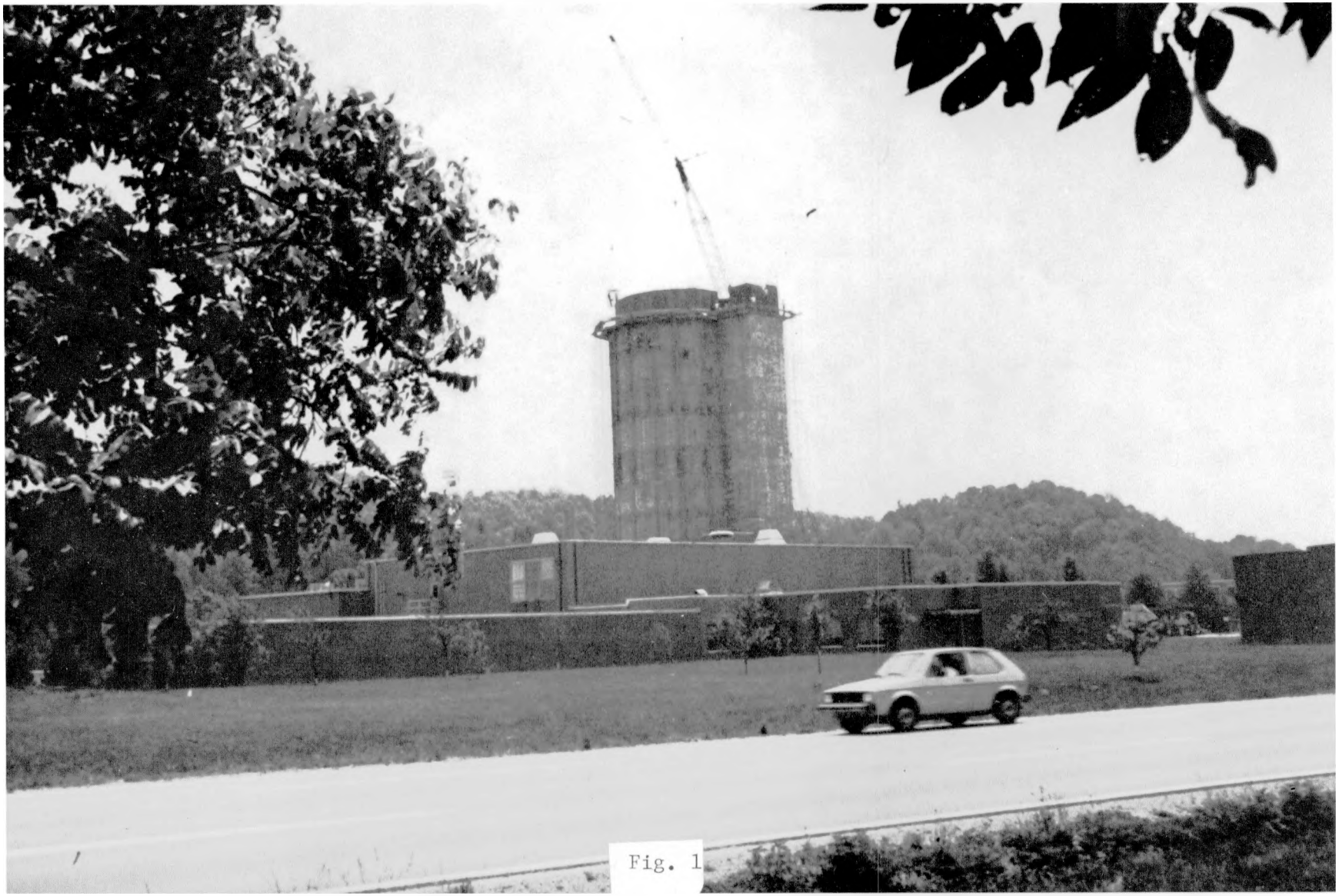


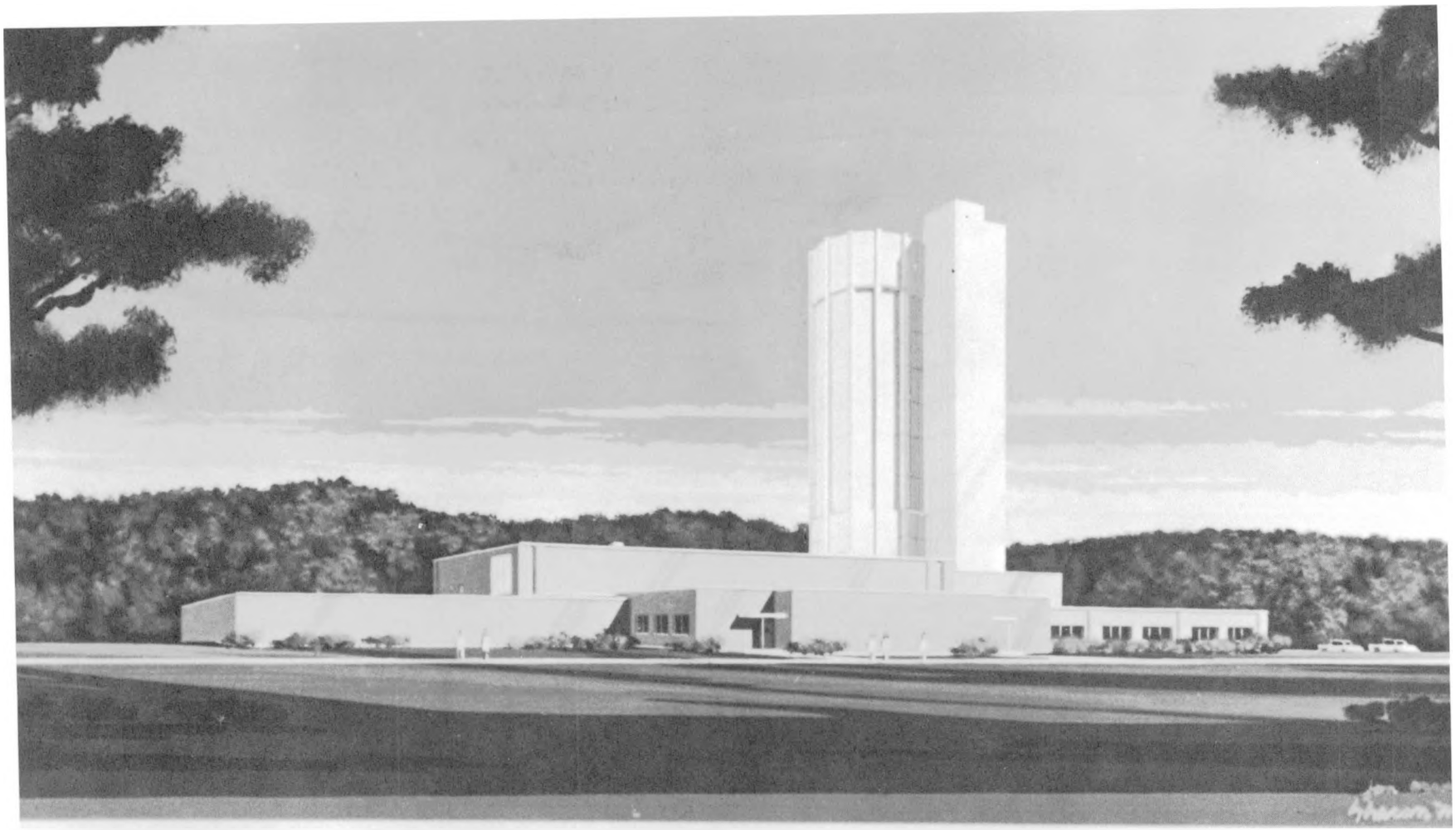
Fig. 1

Fig. 2

Fig. 3

Fig. 4

Fig. 5



HEAVY ION LABORATORY

Fig. 6

COMMENTS ON COULOMB EXPLOSION EXPERIENCE AT BROOKHAVEN

Peter Thieberger
Brookhaven National Laboratory

I wanted to show a few results we obtained which have to do with coulomb explosion. I am sorry I missed Roy's talk but I saw some of his very nice results. We don't have results which are as detailed as his. However, we found about one and half or two years ago the practical solution for avoiding the coulomb explosion by means of gas prestripping. We have been using this technique ever since. The improvements we obtained are rather large. In Fig. 1, I have chosen three examples, which are of practical importance, boron, magnesium, and iron. The source output in nanoamps, which you see here, are our best numbers for the elemental negative ions which we have obtained at Brookhaven and I believe they are not too different from intensities which have been seen by Roy and elsewhere. These are three cases where you have to go to molecules to get any reasonable intensities. The easiest molecules are the oxides, BO, MgO, and FeO. Here you see the intensities that we obtained at Brookhaven for the molecular beams. In some cases the hyride beams are a possible choice and in those cases the coulomb explosion effect is not as important. When we tried to use the BO beams about two years ago, we noticed a drastic reduction in the beam transmission through the analyzing system compared to other beams. It was realized that this might have something to do with a coulomb explosion effect encasing the energy spread and angular spread, as Roy explained. Indeed, by adding some gas in the stripper canal, which is located before the foil stripper most of the lost transmission could be restored.

In Fig. 1 you see the ratio of gas plus foil intensities over the analyzed intensities we have obtained when only using a foil. The improvement we obtained for beryllium was 9.4 and the improvement goes down to about 1.8 for iron. The analyzing slits were set at an energy resolution of about 5×10^{-4} during these measurements and the terminal voltage was about 12 MV. In the next column in Fig. 1, I have used the simple-minded coulomb-explosion formulas, neglecting any such strange effects such as screening within the solid, weight effects, or velocity due to molecular motions. We obtain energy dispersions which are rather large compared to the aperture of our analyzing system, therefore, explaining why the transmission is so poor when we don't avoid the coulomb-explosion effect. The same is true for the maximum angles which are of the same order of magnitude as the multiple scattering introduced by the carbon foils. It seems, from this, that probably the angular effect is not as important as the energy effect.

Discussion:

Lund: Did you see any effect on the foil lifetime? It was my impression, I think, from talking to you or Bob Lindgren, that there was a change in foil lifetime by adding the gas.

Thieberger: Yes, I think there is such an effect. We have observed it but it is not completely consistent. In many cases we have seen increasing foil lifetime and the explanation of that may be that the carbon on deposition on the foil in use by the beam is produced by having this volume of gas in front of the foil.

MAJOR IMPROVEMENTS AT BROOKHAVEN SINCE THE BEGINNING

Peter Thieberger
Brookhaven National Laboratory

Figure 1 lists all the major improvements we have introduced since the beginning, in chronological order. In 1973, we took the aluminum tubes out of our second machine and installed the 8in. stainless steel acceleration tubes. We started to use SF_6 insulating gas; we started to use foil stripping; we installed magnetic suppression and apertures in the dead sections; and we installed the ion pump in the terminal. In a few minutes I am going to show you the corresponding improvements in energies and voltages, but let me just go down this list very quickly. In 1974, we started to use dual foil stripping by installing a second foil stripper in the first dead section following the terminal. Also, in 1974, we started to implement the separated voltage divider to decouple the tube from the column, also we started using a down charge system for our belts. At the end of 1974, a Middleton source was put in use, extending the number of beams and their reliability in a very substantial way. We replaced the radiation sources with cesium sources, which is not only a convenience, but I think it is also an improvement, because that way the intensity of these sources is now essentially constant and we don't have to worry about decaying sources and keeping sufficient activity in the tank. In 1975, we installed external protective ball valves to protect ourselves against possible loss of SF_6 through the vacuum system. These are fast-acting automatically interlocked valves which, fortunately, have not been used. They have been used in order to separate the vacuum system from the diffusion pumps, which is an operational convenience. At this time, we already had the blue HVEC resistors in the machine and they were showing some deterioration, some value change with time and voltage, and we installed additional spark gap protection on the resistors eliminating this problem. We doubled the number of foils in the terminal from 120 to 240 by installing a second stripper in the terminal. We adopted the Rochester method of monitoring the conditioning process in the tubes by installing photomultiplier tubes, viewing each section of the acceleration tube, and recording the output of those tubes on strip-chart recorders. We continued the implementation of the separated voltage divider scheme to 50% of the machine, and in May 1976, we reworked the column connections in MP-6, which we had already done in MP-7, before thus eliminating all the corrosion problems that we had at all the spring-loaded connections and spring connections in the machine. That is another reason for going to the independent voltage divider for the tube, it eliminates all the spring connections between the tube and the column. We also implemented the same ball-valve system in MP-6, which we had installed in MP-7. In September 1976, we installed the terminal sputter source, which Mike described, and we went to the open belt guide configuration, temporarily improving the situation with a failing belt at that time. In April 1977, the Pelletron installation took place, which Bob Lingren described, and we completed implementation of the separated-voltage divider for MP-7. We also improved the spark gap protection on the acceleration tube and we installed magnetic suppression on all the tube sections. This is going to be the subject of my talk this afternoon.

I have shown in Fig. 2 the characteristics of the 3-stage facility at this moment. The assorted lines indicate the most abundant charge-state energies for one machine at 13 MV with one stripper, 30 MV with two strippers, and our present

3-stage configuration of minus 8 on the injector machine, and plus 14 on the second machine with two strippers. The dotted line indicates energies corresponding to about 5 particle nanoamp intensities assuming injection intensities of one microamp. These other lines indicate coulomb-barrier energies for different combinations of beam target Z's. You see with our highest energies we just barely over come the coulomb barrier with calcium beams on transuranic elements. That's opening the possibility, perhaps, of looking for island-of-stability elements. Another way to look at machine performance is to plot the percentage of operating time as a function of terminal voltage as shown in Fig. 3. Here we see what we had with the small stainless steel tubes and a belt. I don't have a curve for the aluminum tubes, but for the aluminum tubes the peak was between 9 and 10 MV. Then we can see what happens when we put in the large stainless steel tube, still with a belt, and then we had a peak between 12 and 13 MV. And finally, we have our present situation where we have a peak between 13 and 14 MV and a maximum voltage of 14.1 MV. What I have done, in Fig. 4, is plot as a function of time, the maximum energy of three important things, oxygen, chlorine, and iodine, as a function of years. Here we have the old aluminum tube, this is when we put in the small stainless steel tubes and foil stripping and that's why we have this big jump here. This is where the large stainless steel tubes went in with a belt and this is the Pelletron installation and magnetic suppression in the tubes.

Discussion:

Lund: Another interesting way of looking at that information, Peter, would be if in your first slide, you would include the cost of items as you went down the list, then the last slide could be dollars vs MeV.

B. N. L. Tandem Facility Improvements

7/73	8" stainless steel acceleration	MP7
	Terminal isolation ball valves	MP7
	35% SF6 insulating gas	MP7
	Terminal foil stripper	MP7
	Magnetic suppression in dead-sections	MP6, MP7
	Ion pump in terminal	MP7
4/74	Dual foil stripping	MP7
5/74	Separated voltage dividers on sections 4 and 5	MP7
6/74	Down charge system	MP6, MP7
7/74	Middleton sputter source in use	MP6, MP7
9/74	^{137}Cs radiation sources	MP6, MP7
9/74	47% insulating gas	MP7
4/75	External protective ball valves	MP7
3/76	Column connections improved	MP7
4/76	14" stainless steel acceleration tubes	MP7
	Improved resistor spark-gap protection	MP7
	Second foil changer in terminal	MP6, MP7
	Use of P. M. tubes for conditioning	MP7
	Separated voltage dividers on tubes 3, 4, 5 and 6	MP7
5/76	Column connections improved	MP6
	8" stainless steel acceleration tubes	MP6
	External protective ball valves	MP6
9/76	Terminal sputter source installation	MP6
	Open belt guide configuration	MP6, MP7
4/77	Pelletron installation	MP7
	Separated voltage dividers on all tube sections	MP7
	Improved tube spark-gap protection	MP7
	Magnetic suppression on all tube sections	MP7

Fig. 1

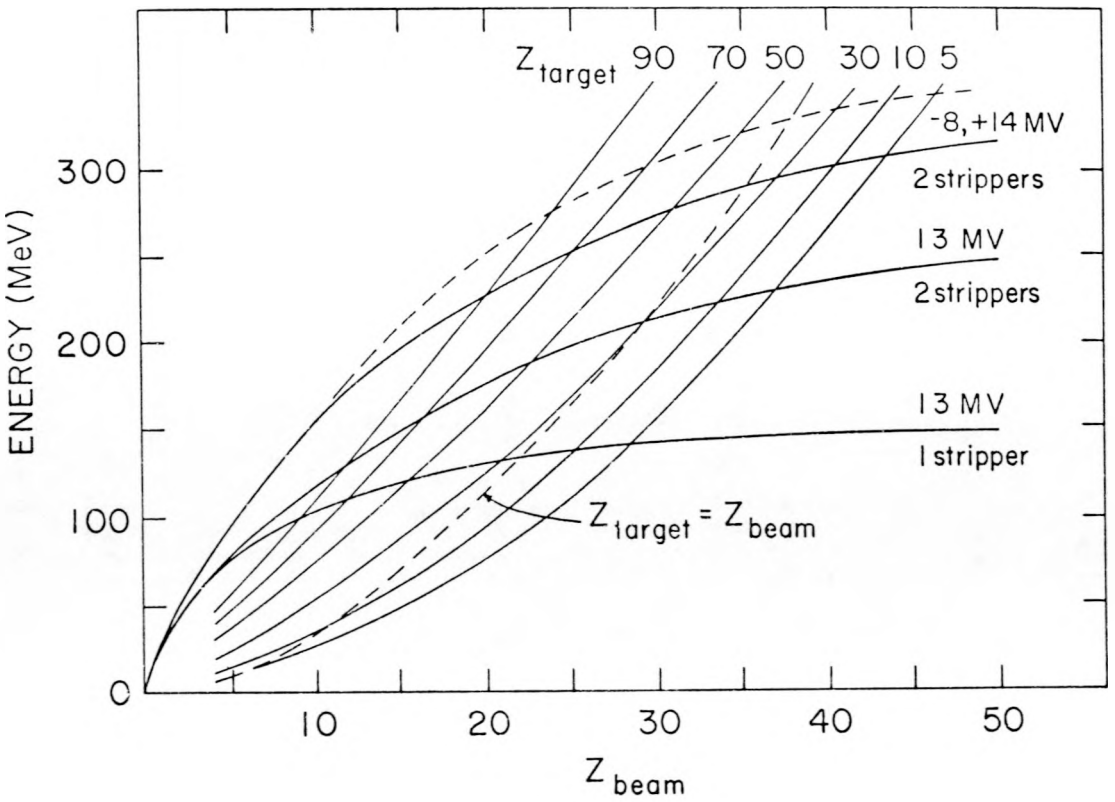


Fig. 2

Fig. 3

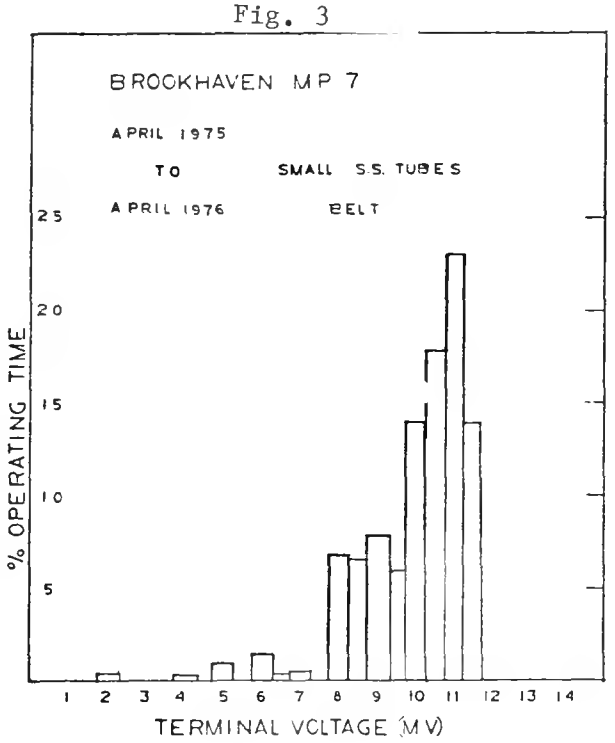
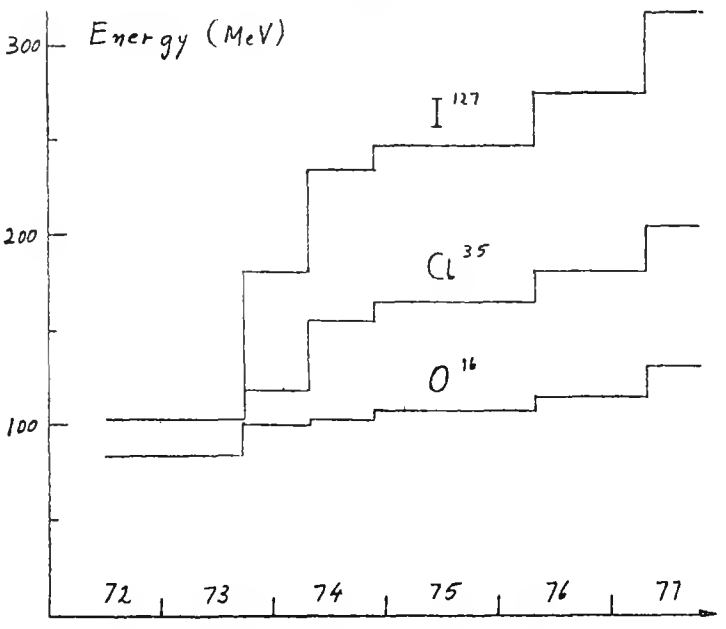


Fig. 4



STONY BROOK BOOSTER PROJECT UPDATE

Eugene Schultz
SUNY at Stony Brook

I think I will take this opportunity to tell you how the Stony Brook operation is coming along. The booster project is progressing nicely right now. The four-cavity accelerator, which is one segment of the future booster, should be at Stony Brook within the next 6 to 8 weeks for beam testing. That should prove the validity of the system as an accelerator and then we can begin firm purchasing of components for an upgrade and the building of the booster itself. The upgrading of the tandem is pretty straightforward. We will go to new accelerator tubes, either the spiral or the stainless steel inclined-field, an elevated ion source of 350 to 500 kilovolts, probably harmonic bunching, new pumping, terminal pumping and the 100-foil stripper, as I mentioned before, has been installed already. We will add some more SF_6. We currently run at about 56 or 53 pounds of pure SF_6 and we will try for 80 or 85 pounds. Also we are planning some sort of a charge-chain installation probably a Pelletron due to the timing. I think that about covers our plans.

McKay: Why are you going to a Pelletron? I am not saying I disagree, there are times when I think it's good and other days when I don't. I would like to know your thinking behind it.

Schultz: Personally, my thinking is, basically, stability, and I am totally disgusted with belts.

Woods: Are you going to hard vacuum throughout the machine?

Schultz: To a minor extent. The pumping will be 1500 ℓ/sec turbomolecular pumps at the low-and high-energy end and titanium sublimation at the terminal. The rest of the transport pumping will pretty much stay the same. We run in the low 10^{-7} range right now.

Wegner: Why did you chose turbos on the machine instead of cryo pumps?

Schultz: I have them. This was something that was ordered for a probable purpose on surplus capital equipment funds some 2-1/2 years ago, and they fit the situation nicely with no added cost right at the moment.

Middleton: Are you going to put just two chains in the machine?

Schultz: Yes, and double-value resistors.

Middleton: There is no plan then to use the corona tube?

Schultz: No.

Middleton: Do you think that is adequate? I mean the current I believe you can get on the chains is about 85 microamps so that gives you a maximum of 170 microamps.

Schultz: I don't think that is quite true right at the moment. The Argonne machine is running an effective practical 160 microamps. The reason for the lower current I think is well known. Their pulley size is somewhat larger than need be and there is arcing between the hoops and the chain at a given chain potential. I am told that, with a minor redesign,we can get the chain further from the hoops and induce more current on the chain. We have been told we are guaranteed 200 microamps with a probable limit of 250.

Norton : With FN's we will probably go to a 12in. pulley which will pull it way down away from the hoops.

Wegner: Yes,the Argonne machine also has pressurized corona which I believe is perhaps a fifth or a tenth of your column load in terms of resistors. Are you going to corona as well?

Schultz: No, I am going to double-value resistors. My calculations tell me that at 9 million volts I should have an excess of 40 microamps.

Wegner: Lots of luck.

Norton: The University of Wisconsin EN, which has chains, also uses double-value resistors and it's been going well for several years now.

Schultz: Yes, I have been talking to them at some length. They are quite pleased with their resistor and Pellet chain combination.

STATUS OF LADDERTRON DEVELOPMENT AT HVE

J. Shaw
High Voltage Engineering Corporation

I don't really have a long talk planned, we are just in the early stages of our actual machine testing with the Laddertron. We have one Laddertron installed in the MP and we have about 100 hours on it now. Performance-wise we have gotten the machine up to about 15 MV without a radiation source and that is not a very stable place to be in an MP. I think it is most impressive, having gotten to about 9.5 MV half-column suggesting the gradient in the machine is adequate for 19 MV. Current capability, as of now, is a little less than we want. We have about 430-440 microamps available from the one chain. We are making some changes this week which we hope will give us more. It would be nice to have more. In any case, we are committed to installing two of these chains in the Legnaro TU machine, so our primary impetus, right now, is to make a design suitable for MP's or TU's, that is for large machines. With that design straightened out, we plan to take the chain out back on the floor and do the mechanical design changes required for FN and EN installations. We really don't have much done on that yet. We don't anticipate much of anything except mechanical changes, such as the idler diameter in the dead section will have to be smaller than that which we are using now. We have about 10-inch contact diameter idlers which are about a 900-RPM-bearing requirement, which is very modest. For the FN that will have to go up. I guess I really don't have much more to say right now.

Discussion:

Schultz: I assume that the preliminary plans for the FN and EN installations would be to put the Laddertron in the same channel that the belt goes through.

Shaw: Correct. I don't think we have any choice, as a matter of fact.

Thieberger: I have two questions. How much power can you deliver with one chain to the terminal, and what is the cost going to be to convert to a Laddertron system in an MP?

Shaw: By power delivered to the terminal, you mean to belt drive an alternator? I don't think we are sure of the limit, I am reasonably certain we could drive a couple of kilowatts, but I am not certain how much more. That basically has to do with how tight you want to run your chain. As far as the cost I am not sure that I can answer that today either. Since we are not really through the test program we are not exactly certain what we need in the way of power-supply systems. I guess I have to admit to knowing that it is high.

Wegner: What kind of ripple did you see on the capacity pickup unit when you were running at, say 9 MV, with the Laddertron on the MP test program?

Shaw: Basically the ladder ripple starts off at about 150 volts and gets up to perhaps 400 volts toward 10, 11, 12, or 13 MV, but superimposed on that we have

a belt frequency ripple, a slow ripple that gets up to 3 or 4 kilovolts peak to peak, at the moment. Part of that is due to the MP, it is so old and dirty we really haven't gotten it clean yet. I don't think that is all of it, and I really don't know why we should have such a lovely belt frequency.

Wegner: Are these some kind of standing wave oscillations, not unlike the horizontal oscillations in the Pelletron, that cause a different class of ripple than other components?

Shaw: I don't have mid-column position sensing, but I have some capacitive sensors at the base and I can look in the windows. I don't really see that kind of motion. It seems to run fairly clean in the vertical mode. We are not completely satisfied with lateral-mode oscillations. The very first chain we put together, we reasoned that our tolerances and statistics would obviously mean that you could just put the parts together and have a chain. Well, we can't. The statistics pile up on you and you will get one side being a little bit longer than the other and so you will get whipped. At rated speed, which is 12 m/sec, we had something like a quarter-of-an-inch peak-to-peak lateral motion which is unacceptable to me. So we rebuilt the chain in about 1-meter-length pieces and that knocked the lateral run down to a little less than half of that. I still don't like that and I think we are going to have to rebuild the chain once again in even smaller increments. I think that's a detail we can work out.

McKay: Do you have any thoughts on how the current carrying capability will be reduced when you go to an FN configuration? I understand you will have shorter rungs on the ladder. Do you know much about where the charge sits on the ladder and how much that will reduce it?

Shaw: No.

Berners: What is your time schedule for the EN-FN Laddertron?

Shaw: After completing the Laddertron tests in the MP, we really want to get a power shaft design in there, probably by January or February. We can't really be much later than that. Hopefully, we can get that tested out in a month or two. Everyone always says those things. I don't see how we could even get to it until March or April. I don't think that should take too long with the exception that we would like to put some time on the configuration just for mechanical reasons. I guess I should have said that we preceded our machine installation with about 2400 hours of test-rig time just to see where the wear was. We want to do the same thing for the FN.

Larson: I would like to respond to an earlier question about where the charge goes on the ladder. Daresbury obviously ought to speak about this but my understanding, from simple electrostatics, is that the rung is the place where you capacity-couple when you are putting charge on, but the charge will go to the outer extremes of the rung, the bulges, as soon as it gets away from the inductor.

Shaw: I am not sure, but it seems to me, if that were true, that one Laddertron would only carry two chains worth of charge.

Larson: I think that there is some truth in that statement. You carry somewhat more because of the rung but not a lot more. I believe the rung helps a great deal in the inductive charging process.

Shaw: I thought you were talking about the free-run limit.

Schultz: Isn't it my understanding also that you have more surface to see the inductor, so therefore you are able to put more on?

Shaw: Yes, that is true.

Wegner: Do you find any slippage in your Laddertron, for one question; do you find very much negative charging from friction effects, is the second question; do you oil, is the third question; and as a fourth question, does NEC plan any modifications in the Pelletron to stop the slippage which causes our machine to go into a yo-yo configuration now and then, jumping up and down by a couple of megavolts?

Shaw: I can't answer the last one! We don't see slippage. We are running, at least statically, eleven hundred pounds total tension, or 550 pounds per run. We don't oil, we see about 50-kilovolts of positive self-charge in air and we see about 200 kilovolts of positive self-charge in 90 pounds of SF_6.

Wegner: You don't see negative self-charge?

Shaw: No negative self-charge.

Billquist: Obviously, this is not really directed at Laddertrons, but just a bit of information related to oiling and selfcharging. In our present conversion project, we found it necessary to hunt hard for places to put things through the end of the tank and we removed our oilers. In the three years we have had the Pelletrons in, we have never once seen oiling change the operating condition of the machine.

Hurley: A comment on the oiling of the Pelletron: we must oil above 10 MV. The self-charge gets quite severe if we haven't oiled, say, for a 2-day period. We haven't noticed the yo-yo effect that was commented on by H. Wegner, but we do notice a bit of instability if the regular daily routine is to oil at the beginning of the day shift and if someone doesn't do that, then it becomes obvious in terminal voltage instability. Of course it also becomes obvious in the slightly higher charging voltage and charging currents. It also shows up in the daily reading sheets.

Thieberger: I would like to make a brief comment about the yo-yo effect. I think it may have something to do with the bearing problem we have in the idler wheel, when the amount of tension you have to put on the chain is increased by the fact that the wheels are not turning as free as they should, then the amount of slippage increases and the chains essentially stop. We hope to solve this problem by changing all the bearings in the system in one or two weeks from now and, hopefully, this effect is going to go away.

Larson: My understanding from Daresbury is, they use a very special pulley material with some graphite cloth in there. Is HVE using that and do you attribute that material to your lack of need to oil?

Shaw: Can't think of any other reason.

Schultz: I have a quick question before we break for lunch. The Oak Ridge people, last year, were wondering about a nondestructive pulse measurement technique and I wonder if they have made any progress toward it? Evidently not.

Session IX, Chairman, P. Thieberger

IMPROVED MP ACCELERATION TUBE PERFORMANCE

P. Thieberger
Brookhaven National Laboratory

The maximum electrostatic field that can be reliably sustained by acceleration tubes has been the main limiting design parameter ever since electrostatic accelerators were first developed. After the first multi-electrode acceleration tube was introduced[1]), most improvement efforts have been directed towards suppressing vacuum breakdowns or discharges caused by complicated regenerative processes involving ions, electrons, and photons[2,3]). The accumulation of charges on the vacuum side of the insulators is also thought to play a role in these processes[3]). Several different techniques have been developed to reduce the number of parasitic low energy charged particles capable of being accelerated and of generating secondary particles by interaction with the electrodes or with the residual gas. The initially low energy of these particles allows them to be easily deflected in relatively weak magnetic fields or transverse electrostatic field components, without unduly affecting the accelerated beam. These methods were first proposed by Van de Graaff[4,5]) and the introduction of his inclined field tubes constituted a major breakthrough in accelerator technology. Magnetic suppression is also mentioned in his patent[5]) as a means of providing improved operation at the injection end of the tube where the low beam energy prevents the use of inclined electrostatic fields. Magnetic suppression in straight tubes is described in detail by Howe[2]).

Other techniques used to reduce the number of accelerated secondary particles involve backbiasing of tube electrodes[3]) or the use of restrictive apertures either between sections of inclined field tubes[6,7,8]) or in special

straight tubes[9]) in which low energy particles experience strong lens effects. Such apertures are designed to transmit the beam while minimizing the solid angles for acceleration of secondary particles. The generation of secondary particles at these apertures has to be minimized by either magnetic suppression[6,7,8]) or by careful choice of special materials and extremely clean vacuum conditions[9]).

Various combinations of two or more of these techniques have been used in the past. In particular, magnetically suppressed apertures[6,7]) and backbiasing have been in use in conjunction with inclined field acceleration tubes at the Brookhaven Tandem facility. Magnetic suppression in each inclined field region along the entire accelerator, has now been implemented for the first time. The result has been a dramatic improvement in acceleration tube performance. This development originated as a solution to abnormal conditioning characteristics of one of the acceleration tube sections.

The light generated during small discharges in the acceleration tube is monitored by photomultiplier tubes[10,11]). Normally the frequency and intensity of the light pulses increases when the voltage gradient in the tube is increased and then gradually decays over periods of minutes or tens of minutes. Continuous recording of the light pulses from each of the eight acceleration tube sections serves as an indication of how fast to voltage condition the accelerator without generating an excessive number of massive vacuum discharges (tube sparks). Above about 12 MV terminal voltage, one of the acceleration tube sections closest to the terminal showed persistent light flashes especially in the absence of beam and it was difficult to condition the accelerator to 13 MV. The length of these light pulses was between 1 and 3 milliseconds. By means of a 3 x 3" NaI(Tℓ) scintillation

detector it was shown that they were in coincidence with bursts of x rays extending in energy up to about 2.5 MeV corresponding to electrons accelerated for almost the entire length of this tube section which was operating at 3 MV. Such energetic electrons were unexpected since the inclined field configuration is such that low energy electrons originating anywhere in the system should be stopped by an electrode before gaining more than about 300 or 400 keV. Possible explanations for these high energy electrons must have to do with forces that deviate them from their normal trajectories. One possibility may be small angle collisions in the residual gas. Other possible explanations may be related to instantaneous perturbations of the electrostatic field either due to disturbances of the overall voltage distribution along the tube, due to space charge or surface charge effects, or due to radio frequency fields. In any case, additional electron suppression was strongly indicated. Magnets were installed in the middle of each acceleration tube region with either upward or downward components of the inclined electrostatic field. There are four such regions in each 72" tube section and six magnets (three above and three below the tube) were used in each region. The magnets are of the same type as normally used on the straight sections of the first acceleration tube[12]). They produce a horizontal field perpendicular to the beam direction oriented so as to deflect electrons vertically in the same direction as the deflection produced by the inclined field. The maximum value of the magnetic field on the accelerator tube axis is about 10 gauss and the field integral along the beam direction is about 350 gauss-cm leading to negligible beam displacements[2]). The installation of these magnets eliminated the abnormal conditioning characteristics of the tube section

which then became the best tube section in the accelerator in terms of low light levels during conditioning and 13 MV operation of the accelerator improved considerably.

Recently all the acceleration tube sections were provided with similar magnets and this has contributed importantly to performance levels unprecedented for model MP tandem accelerators or, for that matter, for any tandem accelerators now in operation. The accelerator was conditioned for the first time from 13 to 14.1 MV in about three hours, which is several times faster than the previous conditioning rate for 12 to 13 MV. The operation at 14 MV is more stable than it was before at 12 or 13 and the amount of light generated is considerably less. Tube sparks under normal vacuum conditions have at present been completely eliminated. Several experiments have been successfully completed utilizing terminal voltages between 13.8 and 14 MV. At the moment, the accelerator performance is not limited by the acceleration tube. Several times larger magnetic fields could be used in the future should higher gradients call for further improvements of acceleration tube performance.

The expert cooperation of Robert Lindgren and the entire Brookhaven Tandem operations crew are greatfully acknowledged.

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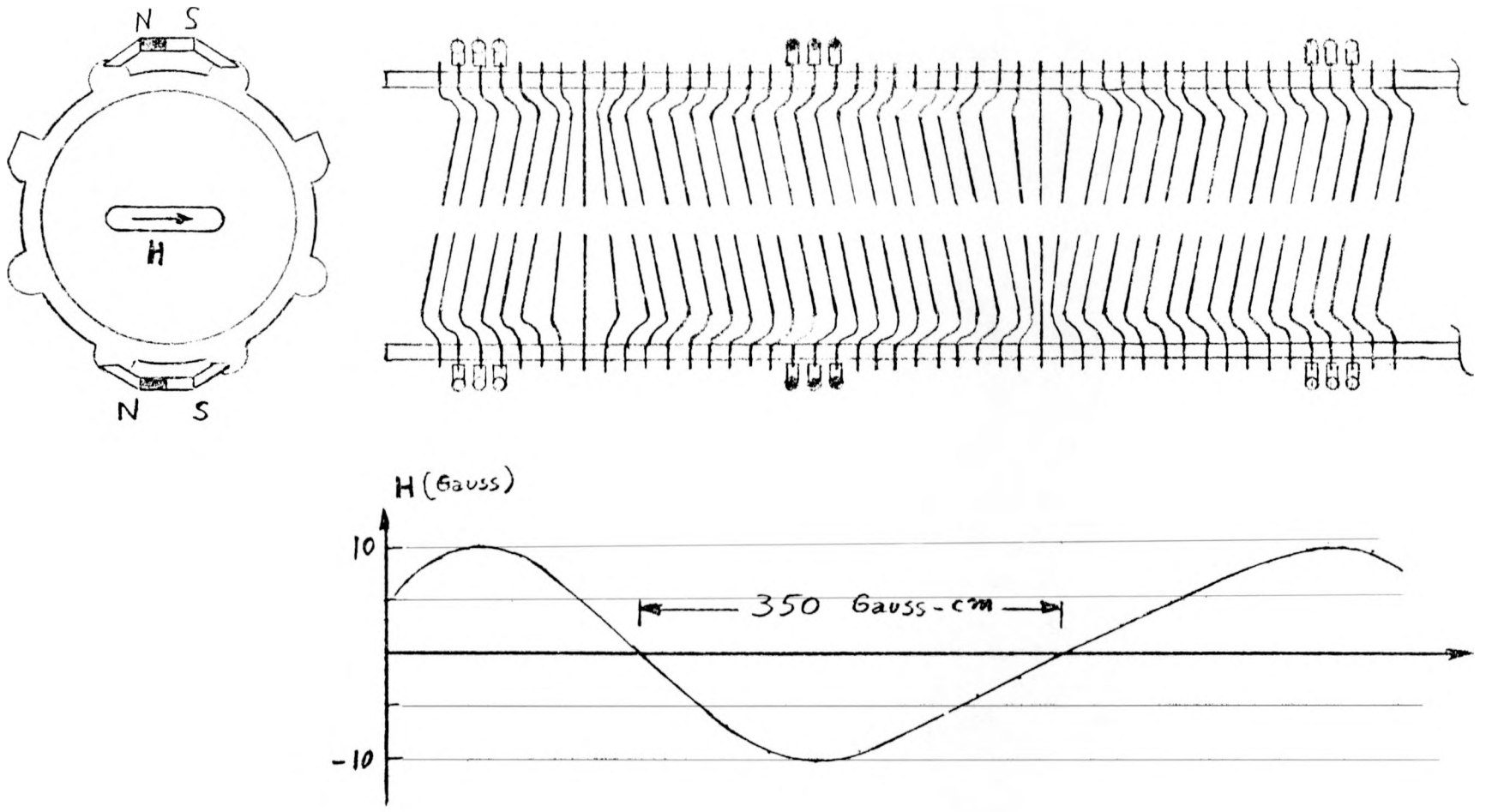

Fig. 1

Discussion:

Larson: I am going to usurp the prerogative of the appointed session chairman and point out to people, here, that the HVEC tubes are made of magnetic materials, and so you must be a little bit careful in deciding what the magnetic field configuration is going to be. If you measure the fields of free magnets you will get a different distribution when you put them on the tubes. Yes, Harvey's point is well taken that the end flanges are magnetic and they influence the field for the magnets placed near the end.

Liebert: Did you look at the x-ray distribution after you put the magnets on? I was curious what the end point was.

Thieberger: No, I did not. We measured the field configuration of the magnets mounted on the tube.

Chapman: If anybody with an FN intends to follow this same technique, one word of caution, which probably doesn't apply to the MP because I imagine that the spring configuration is different. We, upon one occasion , almost had a disastrous accident with one of our glass sections by having additional magnets in and the spring that broke, instead of falling out like a well-behaved spring does, stuck to the magnet leaving a sharp pointed wire just fizzing, and almost ate its way through the glass section, so anybody who does this in an FN needs to be a little cautious.

Wegner: I think there should be some caution also in terms of an FN. The tubes are smaller in diameter and these magnets certainly cause more electron dumping in a local area. It is possible that the intensity could be high enough to cause local glass damage because of the added intensity. The 14in. tube is bigger in diameter and it is possible that electrons are more absorbed by electrodes because of the larger size. It is something to think about; things don't necessarily scale.

Thieberger: On the other hand, you also reduce the total number of secondary electrons, so it's not clear that it's going to be worse for the glass in an FN.

Wegner: The geometrical effects could be more important an unknown way, so you should be cautious if you do it.

Schultz: Harvey's point is well taken and most of us who have FN's keep a close eye on the 20th plane where the electrons drill through the glass.

Hurley: We have put the magnets on the tubes in a manner quite similar to what Peter was decribing. We have definitely cut down our ratio of column sparks to tank sparks, so we can certainly say it made an improvement.

Larson: May I ask, have you done anything about the spark gap protection like Brookhaven has done?

Hurley: We haven't changed the spark gaps yet.

McKay: I was going to say something about the 20th plane, but I guess that's well known. The comment about springs stretched around the magnets on an FN is a good point. We replaced those with short lengths of wire, bolted onto the

column and onto the tube. You get a very, very small clearance in there otherwise. Now, if someone can just tell us how to keep from burning through the 20th plane, we will be in great shape.

1977 SNEAP Conference, Los Alamos, N.M., September 26-28, 1977

THOUGHTS ON ACCELERATOR TUBES

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ABSTRACT

A brief, subjective review is given of mechanisms that may be limiting electrostatic accelerator tubes to present levels of performance. Suggestions are made for attacking these limitations with the purpose of stimulating the thinking of designers and users of electrostatic accelerators.

*Consultant

†Operated by Union Carbide Corporation for the Energy Research and Development Administration.

I. Introduction

At the recent Strasbourg Conference, Hyder[1] commented that electrostatic accelerators remain limited by tube performance and that, "Further improvements in tube design would therefore benefit existing accelerators as well as enhancing the performance or reducing the cost of new machines." In his conference summary, Wegner[2] observed that the HVEC model MP accelerator has been improved from about 10MV to 13MV over the four-year interval between successive conferences and he speculated that further improvements to about 16MV in another 4 years and ultimately 20MV in 8 years were not unreasonable goals for the MP. In each instance, Wegner associated higher voltages with new tube designs, an "X-tube" to reach 16MV and "XX-tube" for 20MV.

This paper represents a personal and imperfect view of what is limiting the performance of present accelerator tubes and entertains possible directions to explore in developing higher performance tubes such as advocated by Wegner. My purpose is to kindle the imagination and stimulate discussion rather than educate.

II. The Environment

While tube weaknesses may inhibit present accelerator performance, a harsh high-voltage environment is detrimental to best tube performance. Each year at SNEAP we hear of improvements made to voltage generators that permit operation at higher voltages with the same tubes. Such improvements include:

1) Secure electrical contacts.

2) Separate tube grading.

3) Chain type charging.

4) Transient protection.

Each accelerator facility has favorite tactics to make the environment surrounding the tube as "quiet" as possible. Independently of other considerations, belt charging causes unfavorable electric disturbances that apparently can be avoided by using chains. Recent work at Brookhaven[3] to be reported at this meeting suggests that careful attention to external spark-gap protection may significantly reduce tube damage from interior breakdowns. In future, operation at substantially higher voltages probably will require intershields to better grade the terminal-to-tank potential difference.

III. Electric Breakdown in Vacuum

Electric conduction in vacuum systems is maintained by electrons and by ions that originate from various neutral sources including:

1) Residual gas.

2) Microscopic particles or "clumps".

3) Absorbed surface contaminants.

4) Substrate surface materials.

During breakdown, surfaces probably provide all the material necessary to sustain conduction.

Breakdown processes in vacuum may be categorized roughly as:

1) Avalanche (primarily electrons).

2) Regeneration (primarily ions).

3) Plasma-arc (gaseous conduction).

Avalanche encompasses uni-directional, multi-step processes having substantial overall gain (electron-multiplier tubes typify this process). Avalanches can be stable and self-limiting or, they may generate sudden current overloads that destabilize a system and contribute to plasma-arc breakdown. Electron avalanches that caused large x-ray fluxes in early tube designs have been brought under control by a variety of electrostatic and magnetic suppression techniques.

Regeneration encompasses feedback processes in which both positive and negative charges participate. The dominant mechanism for this process, exchange of positive and negative ions, was proposed at Los Alamos in 1951 by McKibben and Boyer.[4] Hyder[1] emphasized that sputter ion source development is contributing new knowledge on this subject which reveals that surface conditions are very influential and that high yields of sputtered negative ions are attainable. Gains greater than unity for a full regeneration cycle lead to exponential current growth with time. This is catastrophic unless changing surface conditions reduce sputter rates permitting a self-limiting "microdischarge".

Plasma-arc encompasses electric conduction processes in any region of momentarily high vapor pressure. Once a plasma or glow discharge is initiated, it can be nurtured by materials evaporated or sputtered from nearby surfaces. When confined to a single electrode gap, the arc may extinguish because of insufficient electrical energy stored in local capacitance. Plasma-arc probably is the last phase of any major electric breakdown in vacuum.

IV. Total Voltage Verses Gradient

Clean, conditioned electrodes in good vacuum sustain gradients across single gaps far in excess of gradients attained in large electrostatic accelerators. For small (1-mm) gaps gradients approach 100MV/m and for gaps comparable to the HVEC electrode separation (25-mm) maximum gradients of 20MV/m are still an order of magnitude larger than present tubes can sustain. As the gap is increased, breakdown voltages rise but, as indicated, gradients decline. Does this constrain long accelerator tubes to low gradients?

In 1952 at Los Alamos, Cranberg[5] surveyed the available spectrum of voltage breakdown versus distance and proposed a mechanism to encompass all data:

1) Accelerated microparticles or "clumps" initiate plasma-arc-type breakdown on impact.
2) Onset of breakdown depends on localized energy densities which in turn depend on the product of total voltage and surface field.

In brief, Cranberg argued that the charge of a departing microparticle depends on surface field while the energy of the impacting particle depends on the product of charge and overall voltage. If the surface field varies as V/d, where V is the overall voltage and d the separation distance, then particle energy rises as V^2/d; this exceeds a threshold for initiating breakdown when $V > Kd^{\frac{1}{2}}$. A constant value for K of approximately $2.5MV/m^{\frac{1}{2}}$ fit much of the data available in 1952 over some $4\frac{1}{2}$ decades in d. This is truly remarkable because microparticles probably do not contribute to breakdown over this entire range and possibly to none of it.

Microparticles have been observed; they probably do contribute to breakdown across small gaps at high gradients through the process outlined by Cranberg. Other contributory phenomena such as microscopic melting and explosive evaporation initiated by field emission tend also to depend on total voltage and surface fields.[6] Strong metals having high melting temperatures, such as titanium, resist surface damage and withstand highest voltages. Fields of 20-100MV/m are probably sufficient to wrench adhered particles and protuberances directly from a surface[6] but it is not evident why lightly bound particles should be continuously available to initiate breakdown and limit the voltage in long structures. Some other mechanism, perhaps positive-negative ion regeneration, probably causes the $d^{\frac{1}{2}}$ distance relationship in long structures.

Hyder[1] pointed out that accelerators now operate significantly above the Cranberg trend line of $V \simeq 2.5MV/m^{\frac{1}{2}}$ and show evidence of following a new linear trend (eg. NEC accelerators are designed for operation of V = 2.4MV/m across the insulators, independently of length). This new trend presumably results from field modulation techniques (either axial or transverse) that restrict nearly all charged particles originating anywhere within the tube to a few decimeters travel before intercepting some surface. Breaking the end-to-end communicating path by appropriate field modulation permits separate portions of the tube to operate with autonomy. This appears to be a necessary but not necessarily sufficient condition for eventual linear dependence of voltage on distance. Other factors remain important. NEC, for example, relies on high-vacuum technique to minimize ion production from gas ionization and surface sputtering. Recent problems encountered with NEC tubes thought to be

contaminated internally by plastic packing materials[7] suggest that the contaminant enhances ion sputter rates leading to regenerative positive-negative ion exchange and premature breakdown. Support for this interpretation comes from work by Langsdorf[8] at Argonne who finds physical evidence for sputtered "hot spots" in agreement with calculations on NEC tubes based on similar design principles but having different geometry.[9]

V. Insulators

Insulators in vacuum degrade breakdown voltages by an order of magnitude or more. The cathode corner where the insulator joins the cathode electrode is critical. A popular explanation for breakdown is that field-emission electrons from the cathode corner cause (usually) positive charging of the insulator surface through secondary emission. This distorts and enhances the field leading to increased electron emission, further field enhancement, increased emission, etc., etc. Breakdown follows.

Fortunately, breakdown does not always follow. The postulated positive feedback process has limits that allow insulators to function in vacuum at reduced voltages. Moreover, insulators can charge positively, negatively, or perhaps in mixed patterns depending on local geometry. It is not clear whether such field disturbances lead directly to breakdown or instead enhance the prospects for other mechanisms.[10] Perhaps heating by field emission and electron bombardment, found appropriate for narrow metal gaps,[6] is a breakdown mechanism applicable in this case also. Field enhancement caused by insulator charging could increase field

emission at a given applied voltage and thus lower the voltage at which melting and vaporizations commence. The insulator provides virtually unlimited material bounty temptingly distributed from cathode to anode. If vaporization leading to plasma-arc breakdown can occur on clean, bare metal electrodes, surely conditions along an insulator are more favorable by an order of magnitude.

The present linear trend of $V \propto d$ shows that "total voltage" effects are at least partly under control. This suggests (but does not prove) that individual insulators now limit accelerator tubes. To rectify this, several approaches might be taken:

1) Improve present insulator designs.

2) Develop new insulator technology.

3) Use more of present insulator types.

Improvements in insulator designs encompass testing of materials, shapes, thicknesses, shielding, coatings, etc. Undoubtedly more can be done to improve present designs. It is important to distinguish between an insulator's ability to withstand high DC voltages and its resistance to damage during transients. Both are essential but the first is intrinsic to the insulator, whereas the second is a function of the total environment.

New technology includes research into why insulators degrade vacuum insulation. This may lead to radical departures from present materials or designs.

Insulators of present capability would suffice if greater lengths of insulator could be introduced per unit length of accelerator. This mostly represents an exercise in engineering enthusiasm. The enclosed sketches are offered to stimulate more thought in this direction.

Figure 1 shows schematically present-day axial field modulation utilizing about 70% insulated length along a tube section (on left) and the possibility of extending the insulator towards 100% fill (on right). Similar designs provide high gradients within open-air injectors such as at LAMPF. It is interesting to speculate whether changes such as this could lead to 20MV operation of the NEC model 14UD accelerators.

Figure 2 shows an "accordian" shape that permits more than 100% insulator fill. The mechanical properties, including tension loading of some insulators, leave much to be desired.

Figure 3 shows a spherical structure that maintains compression loading yet also accommodates more than 100% insulator fill. Here the complicated shapes of individual pieces are troublesome.

Figure 4 shows a cross section through one standard "pitch" that is further subdivided. Because insulators do not evidence a linear $V \propto d$ relationship, some advantage may be gained by further reduction of individual insulator thicknesses; this should be pursued for each material until no further advantage is obtained.

If, for example, an insulator exhibited $V \propto d^{\frac{1}{2}}$ behavior then the theoretical optimum occurs when each insulator section is equal in thickness to the electrode thickness (assumed constant). This is extreme, but there may be substantial advantage to subdividing present insulators into about 3-mm thicknesses using about 0.5-mm intermediate electrodes. Self-grading of sharp-edged intermediate electrodes might prove practical.

VI. Conclusions

The historic problem of a non-linear "total voltage" limitation for electrostatic accelerators has been overcome by modulation of the acceleration fields. Electromagnetic decoupling between tube sections also helps. A byproduct of this development is nearly complete suppression of electron avalanche loading that plagued early tube designs with high x-ray production. New, larger accelerators and higher gradients in existing accelerators will test the efficiency of present field modulation techniques. Particle trajectories are calculable and more exacting designs of field patterns should be investigated.

Surface conditions in tubes remain important. Choice of materials and vacuum system influence production of ions and microparticles. Constant conditioning through controlled ion bombardment of electrodes may promote surface cleanliness and suppress runaway regenerative processes fed by surface contaminants. Pumping, as nearly continuously along the tube as possible, will become an integral part of high-gradient tube design. A merger of inclined field designs with bakable, high-vacuum joints and insulators is overdue.

The insulator probably is now limiting tube gradients. Present insulators could be used more effectively. For example, HVEC probably should reduce pitch at least a factor of two, NEC should explore designs that use more length of insulator per tube section. Comparative insulator testing requires relatively low voltage ($\sim$ 1MV) surge generators. The Daresbury program sets a good example except that no benchmark of

comparison is available when new insulators are used in a new tube in a new accelerator. Testing is also needed of new insulators in old tubes and perhaps old insulators in new tubes. Without actually understanding insulators, it should be possible within a few years to realize factors of two or three in improvement and give Wegner his "XX-tube" for the MP.

All this is expensive. Costs of new performance from old machines have to be compared to costs of new machines. An MP or 14UD accelerator operating at 20MV is an exciting prospect, certainly worthy of intensive development effort over the next few years.

Acknowledgements

Joe Bair shared his collection of articles on this subject and provided many stimulating discussions. Dick Hyder has been especially generous in explaining his experiences designing and testing various accelerator tubes. Dick Woods posed this challenge and deserves to share any consequent fame or ignominy heaped on the author.

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Discussion:

McKay: Your accordian tube could be made stable, I think, by taking the small diameter ring, and putting at the same level, a still bigger diameter ring. This means extending the electrode structure out and it would all be in compression. You would have a double ring at the same place.

Larson: I will let you come up with the design, but I am happy that I stimulated another idea.

McKibben: The clump mechanism always seems to me to imply that there is a spark down the center of the tube, and if such a spark occurred you would expect to see a big x-ray burst. Has anybody ever seen a big x-ray burst with a spark in an accelerating tube. I don't think that anybody has.

Thieberger: We have seen at Brookhaven, x-ray bursts, I don't know exactly what you mean by large x-ray bursts.

McKibben: You could calculate the stored energy and know how much energy is being dumped and put this into terms of x-ray intensity and always it's not nearly enough.

Larson: It's not clear to me that all that much energy might eventually go into x-rays. It might start down the center of the tube and then become a gas discharge and the energies would be reduced.

McKibben: The electrons are the chief carriers and your pressure does not rise high enough to stop that mobility.

Norton: With the contaminated tubes we have seen large x-ray bursts. This occurred in Japan on the 12UD and in the initial stages when this problem first arose; we had a counter which just read out on a strip chart recorder, and I have no units, and as we moved up in voltage, the tube flashed with large bursts of x-rays. They were very common.

Larson: Let me make another comment with regard to this drawing. I think we have a situation in this type of tube where the regenerative process between positive and negative ions is going on, or is incipient at any rate, and, now, if you coat this surface with a material with the right kind of sputtering properties, you see it happen. If you clean that surface and provide a material that has a low coefficient of sputtering then it's an exponentially decaying, instead of an exponentially rising, phenomenon. By the way, I didn't mentioned this, but I am quite convinced that one can stimulate this process with beam. Let us suppose that the coefficient is $1-\varepsilon$ for the total return path. Now you start hitting this with 10^{10} particles per second out of the beam. Well, that might lead to a rather large regenerative process going on along the paths indicated there. I think that you could put beam into the tube and see things happen that don't happen when you have beam, I know people have seen it. Let me make a comment about another phenomenon; you are aware that under some circumstances you can put some gas in the tube and go to higher voltages. How do I explain that? One possible explanation is that the gas molecules that you are putting in have a different sputtering property from whatever is on that surface otherwise, so the gas damps things a little bit by putting a new surface coating on. I think you would be better off making the electrode out of the

material that you chose for its good properties, and then keeping it clean, so you are not working with various layers of contamination, but you are working from the substrate which you should be able to control better.

Thieberger: Another explanation for that is possible stripping of negative ions, thereby reducing the feedback coefficient sufficiently, to get rid of the discharge.

Larson: I lost something in the mechanism that you are proposing here.

Thieberger: If a negative ion, which is starting to get accelerated, sees an appreciable pressure, it is going to get neutralized or become positive.

Larson: I guess there is enough gas around to neutralize it, with some probability and you are getting close to the gaseous-discharge situation but I may be wrong about where you stand.

Thieberger: I don't think so, because as soon as vacuum conditions become worse, you start losing your transmission of the negative ions at the low-energy end of the machine.

Larson: You are quite right. That would explain the damping process by a different mechanism which is not a surface phenomenon.

McKibben: If you happen to have a low gradient on one end, you can have an asymmetric tube, and you will find out, that in the condition where the negative ions are traveling at the low gradient, that tube is most sensitive to pressure, more than where you turn it around. I think evidence is very much in favor of the mechanism just mentioned.

Wegner: In terms of the real world, acceleration tube sets for accelerators are made by private industry because there isn't any setup in government research laboratories to even attempt to do this. Even though it is reasonably obvious that ultra-high vacuum techniques, inclined fields, and aperturing techniques should be married, possibly, in some different varieties, it would appear to me, because of the nature of the development program not being in government but being in private industry and privately patented, that the marriage of these techniques is just a swan song that we can sit around and talk about but will never happen.

Larson: Well, if you want to see your x and double-x tubes come through, then somebody has got to do something. I agree with you that we have a problem that private industry has to be supported by contracts in order to be able to sponsor this kind of development work themselves and the government is not supporting it directly. It's hard to think of buying multimillion dollar sets of tubes for an accelerator but I think that's what they are worth. If you can double the voltage of your accelerator what's it worth to you. Think about it a little bit.

Bair: I would just like to stress one point which Dan has made. Many years ago, say 30 years ago, people, like Trump and others at MIT, made insulating vacuum gaps which ran without conditioning at about 50 to 60 kilovolts per inch. Those are about the gradients that we now run in the best accelerators. At that time, these gradients were so far above the gradients that you could

sustain over a long tube section that really not much work has gone into the insulators since then. We are in a different situation now and I think work on the insulators would be of value.

Larson: I stressed the insulator but I think that Joe's going to say a little bit more on the subject. We have to consider what the submodule is that determines the voltage gradient capability and it may be a subsection of the tube. It may be a good number of insulators working together or interacting with everything that's going on in the tube. It may not be just the single pitch.

Lindgren: In response to Joe McKibben's question, I can remember, on the research machine, the single-ended machine at Brookhaven, that on tank sparks we got very large bursts of x rays. That was using the Clarence Turner tubes.

Larson: Do you mean large in the sense that an appreciable amount of energy went into x rays?

Lindgren: I can't say that. We always used a monitor on that machine to tune the beam through it and we saw the x-ray bursts on that monitor.

Larson: If I am correct, Joe, you are asking, where does the energy go? Let me also make a comment about what may be happening at Brookhaven with this additional magnetic suppression added to the tube. It's conceivable that this is doing something locally at the insulator. Any magnetic field tends to reduce the electron mobility, I believe, it's electrons moving around on that insulator that is part of the hypothesis of breakdown of insulators. It may very well be that putting some magnetic field on there is just immobiliziing the electrons or reducing the mobility and helping. Certainly, we should consider the marriage of some weak magnetic fields with the electrostatic suppression technique. There is no reason why you have to have a magnetically suppressed tube or electrostatically suppressed tube and not combine them in some way. If the field will do something for you by all means use it!

AXIAL MODULATION

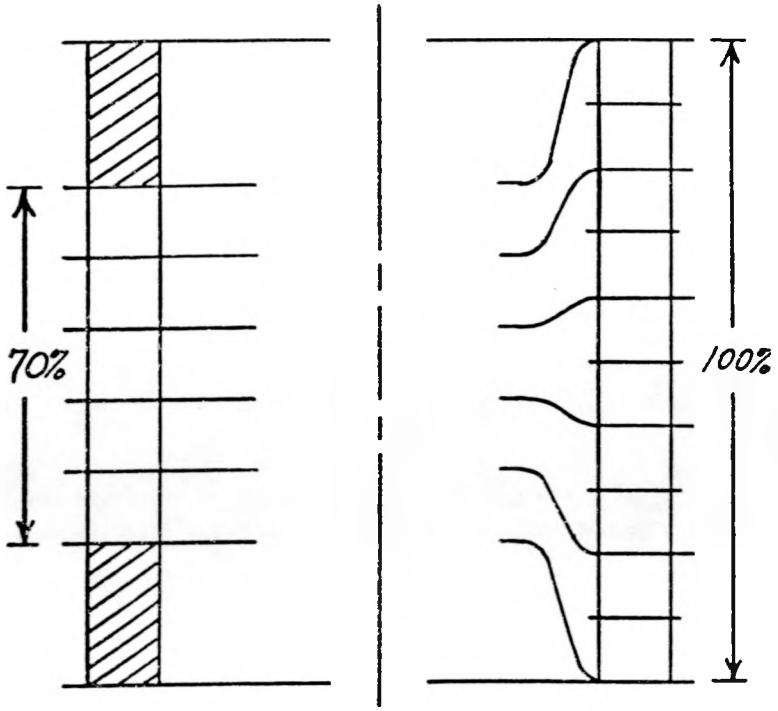


FIG. 1

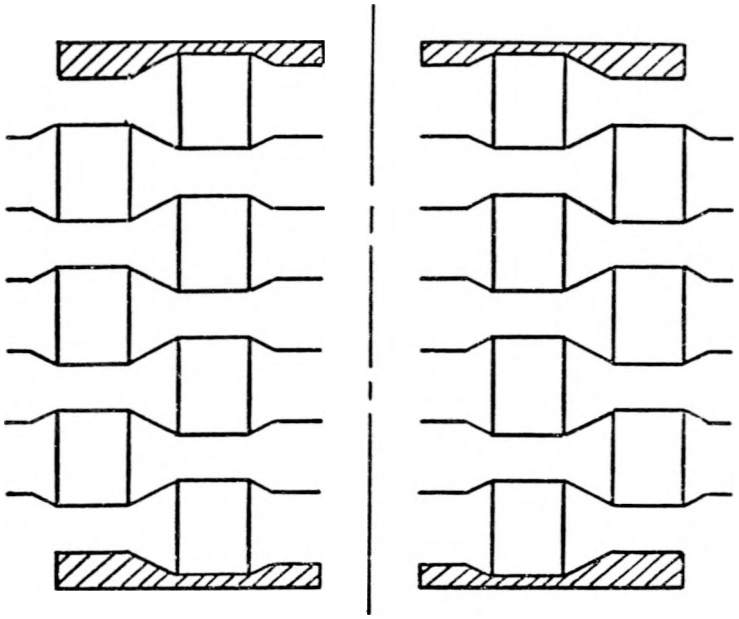
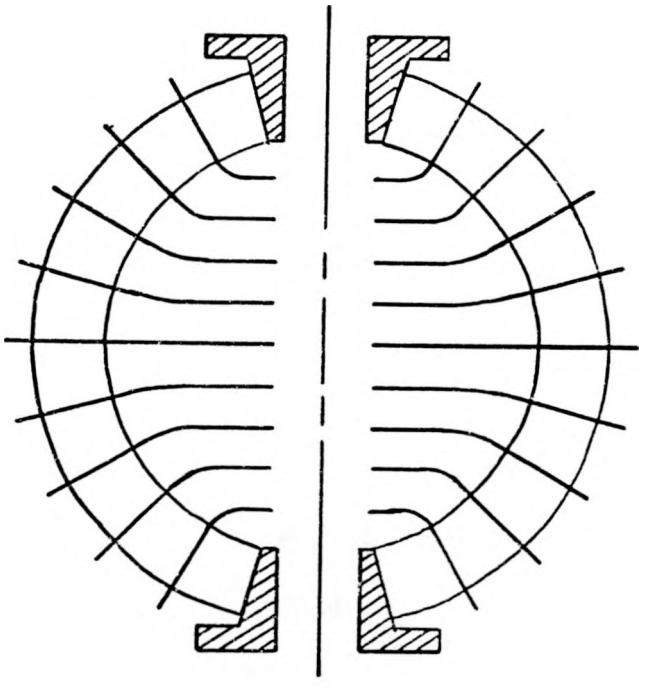
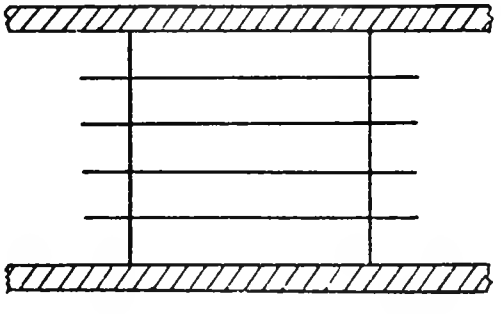


Fig. 2 ACCORDIAN?



TUBERCLE?

FIG. 3



MICROGRADING?

FIG. 4

EXPERIENCE WITH ACCELERATING TUBES

Joe L. McKibben
Los Alamos Scientific Laboratory

When I went to Wisconsin in 1935, Ray Herb had a tube in the long tank that he was building. It is clearly not the kind of tubes his company is now purveying. It was put together with red sealing wax and then black sealing wax was put over that. Under tutelage, I built the first tube with a rubber dam for sealing and spun electrodes. See Fig. 1. In this tube I had trouble, and I put in tilt rings, and so I had a sort of bastard inclined field tube that went to about 1/2-million volts per foot or 2.7 MV on 5-1/2 feet. Well, that was just in the way of an introduction.

When I came to Los Alamos, I had the job of building the vertical Van de Graaff, which most of you have seen. I did a fair amount of work on tube development at Wisconsin. I guess I was impressed by the insulator problem which I want to get into today. We have done a good deal of work on insulators, and breakdown in insulators. You can, as I will show you shortly, put an insulator between electrodes about a centimeter apart and get a voltage across that gap which is very close to the breakdown of the gap without an insulator. This is in contradiction to what you have said. We did a fair amount of testing, but at that time I did not know about the multiplicative processes. Ray Herb and his workers at Wisconsin did not know anything about these multiplicative processes, ion regeneration, micro discharge, ion exchange, or whatever you call it. It is a very troublesome problem. I disagree with Hyder's comment, that it's a dissipation of the material which causes the thing to go off. I think that it is just simply that the voltage drops. This is one of the things that someone should clear up, I think. In the case of Alex Langsdorf, he had enough current capability there that he really turns it on and burns holes into it, and I guess that's a new game there, but I think, for the normal type of Van de Graaff, the voltage drops a little bit, the avalanche stops, and then the voltage starts piling up and it goes again. It does not cause a spark in itself, something else has to go along with it. That's what I want to get into. Cranberg, of course, was an associate of mine and I never did believe his clump hypothesis. The explanation was too simple. I built a tube in 1958 under the direction of a committee that met around here and that was my worst tube. I think that maybe the clump mechanism ideas had something to do with it, I am not sure. In the tube shown in Figure 2, we have used a lot of flat electrodes put together with Mycroy insulators with a technique we developed as a result of test work. In a sense, this came from visiting Trump's laboratory, where they were doing something similar. They eventually went to shaped fields and I have gone from shaped electrodes to flat electrodes. We had a test generator and we did a lot of work where a section of this tube was put in this machine. (Figure 3.) We put a plate across there and we saw that there was a loading effect. It seems to me, it had to be an exchange mechanism which should be sensitive and the effect of pressure on it showed that we were breaking up the negative ions. We could insulate the top plate and stop it, so from that I went on into developing the back-bias system. Earlier, as shown in Figure 3, we didn't have diaphragms when we started using this, and we started seeing things happening in this tube which were bad. We started introducing diaphragms to break down the effects but we

were having loading problems, as indicated by lots of current and x rays. Then I developed the back-bids system in which the voltage is back connected. With that you could stop the microdischarge, ion regeneration, or whatever you call it, completely. But, unfortunately, the tube would spark at a voltage that was much too low. That's what has held this vertical Van de Graaff down for some time. Understanding the basis of that sparking is a problem that has concerned me for some time. That tube was used in the early 50's, then we put in the tube which was designed in the committee and I will not discuss that now. We rebuilt this tube in 1960 with the apertures shown in Figure 4. This region was changed just a little bit and better shaped diaphragms were put in, and these were put on in every section.

What is the problem I think now that there is something simple that we have been ignoring. In connection with this question I asked a little bit ago, you could see sparking x rays. In the test generator, I had an ion chamber and you could see occasional, pretty fair sized x-ray bursts, which tended to be slow at a millisecond or two, then it would break down without x-ray production, and that was a very rapid discharge. In the rapid discharge, clearly, the sparks were going along the insulators, or at least between the electrodes, and I think that has been a big problem with accelerating tubes. We get fancy about all the mechanisms but, basically, sparks propagate along the insulator, from insulator to insulator, or maybe between electrodes to electrodes; it's hard to tell for sure. They don't make x rays and it's not any of the usual theories. You don't need a clump theory. It's what is happening and why does that happen? In this same test generator in 1959, I built a test section, shown in Figure 5. The plate had a smaller hole than shown here. That tube was an amazing thing; it went to 1.1 million volts, if I remember correctly, and this was only 12 insulators long, or about six inches. There was no loading nor x-ray production—it just went right up. In the design, I shaped this purposely because if there is an ion exchange process we want to cause the ions to go to the center and let them escape, get rid of them by trapping them out. This apparently worked because we were getting 1.1 MV, which corresponds to an average voltage of pretty close to 90 kilovolts per insulator. We did quite a bit of work on test insulators and it was never published, but should have been I guess. They were 3/8-inch long, roughly 1 centimeter, and they were about 2 inches in diameter. We mounted them in a vacuum system and applied voltage. In many cases you can get 150 kV across that. If you try removing the insulator and try voltage across the same gap you may not get to more than about 200 kV. So it is possible to get high voltages across insulators.

Larson: How did you treat the corner effect?

I was aware of the negative corner effect because it showed up in Cockcroft-Walton at Wisconsin while I was there. We developed a technique to join the electrodes and the insulators using vinyl acetate and, again, I guess I copied that from Trump. Then I found out that it is important to wash all the fillet away to get rid of all the excess vinyl acetate so you have a good clean joint. We ran test after test; there were improvements we tried like tapering them. It seemed to condition more rapidly, but on that type of test it didn't seem important. At the time I found that glass-bonded mica was one of the better materials. This seems to be in variance with a lot of other people's experience, and I don't know why. In one case we did bring in a field-emitting point. I drilled a few holes, polished just right around the edge of the point,

and that performed so terribly we never got around to doing the experiment. I am sure if you bring in a lot of electrons, you are in trouble.

I think the mechanism that we are over looking is due to electrons causing charging of the insulator. The manner in which it causes the charging, I am not able to put together from the various experiences that I have seen. There is some evidence that soft photons created in the vacuum cause trouble. This fits the Brookhaven experience, where magnets were put in to get rid of the high-energy electrons and they obtained much better performance. One of the things that I did was an experiment on the ions that are involved in loading. I wanted this tube section to load so I put in this little curved electrode. I was able to get a nice loading current of ions. We magnetically analyzed that with the apparatus shown in Figure 3. Unfortunately, now the tube had a much greater tendency to spark, but was much worse with negative ions--something about having these corners in here emitting electrons and causing sparks. You would also see them for positive ions but the probability was much less. Now let me go back to Figure 2 . When we have back-biasing on this, one has the condition that electrons, originating from here, can go up and cross over and strike the other electrode. There is a trajectory which crosses over, so any electrons (or clumps) would go up and strike the electrode, which seems to me has been the explanation for that. Later on, in some other work, we reduced this back-bias voltage to where it does not stop the x-ray production electron or the ion exchange process and, in that case, we found that we could draw rather large currents and it didn't spark. You could condition away and it did not spark. In that condition, electrons are no longer able to reach these and they must go on up. I can go through several examples. There is a tube at LAMPF on the 750-kilovolt Crockcroft-Walton injector which uses back-biasing. They have a problem where they can get the voltage up normally, but after they have been in the tube, it has a strong tendency to spark with the ion beam. They have to do a lot of running to get rid of that. The explanation, I believe, is that the ion beam strikes the back-bias electrode, which releases electrons and, because of the back-biasing, those electrons cross over and hit other electrodes and are somehow causing breakdown sparks. You gain by this mechanism, but in effect, you have lost by the fact that these electrons are crossing over and charging the insulator. I think that is why the inclined field tube has not gone to much higher gradients. You are going about 600 kilovolts per foot or so. Some of this is summarized in the report LA-5376-MS, which I wrote four years ago. Incidentally, I am exhausted of copies so don't write to me for a copy; you will have to buy it.

Going to Figure 5, where you have a section with 2 MV/ft, how can you combine the goodness of that there section into a long tube of practical design? This I have wondered about several times. I think I have a solution which is shown in Figure 6. I drew this up a couple of years ago, after being to Europe and visiting Daresbury. I must admit I did it at home and its been languishing on my desk for these past two years. Dan was kind enough to give me a copy of what he was going to talk about, and that's what stimulated me to bring this out. What I have to do here is incorporate the good features, and I actually drew it up with insulators because I was hoping to stimulate P-9 to do something like this, but then there is the problem of a government place doing research on tubes. I drew this up using some Mycroy insulators that were on hand and perhaps contouring them a little bit. This would be back-biased. It wouldn't take very much but you should have an inverse voltage here and there. This is made squatty because what I want is anything coming off here, I want to go over

this way. We don't want anything going from here to here. If we can, we must eliminate all the electrons (I assume that it's electrons, it might be clumps) from coming over here, hitting and causing whatever it is that seems to trigger the breakdown. Then maybe we will have a high-gradient tube. This is one attempt, and I have another version here. It needs somebody who can set up to do trajectory calculations to see what some of these trajectories are doing. It may well be that some of these shapes that I have are not right. What we are trying to do is dump the ion exchange in here, and providing this opening gives us a considerable pumping speed. I think that this tube, even without hard seals, may perform quite well. In fact, I guess I have some reservations about hard seals. I would like to see results, and I believe you can't do as well with hard seals as I believe that you can with this, because the continuity of your electrodes is better preserved through the junction.

Discussion:

Larson: Let me simply say with regard to the clump hypothesis that the greatest weakness that I find in that theory is how do you get clumps that are easier and easier to draw off the surface, as you make the tubes longer and longer, because you must have a source of clumps of the right extractability, if I may use such a phrase, as a source all the time. You would think they would clean off, so I think that clumps may exist for very high gradients and very narrow gaps, and we are not talking about that situation here. I just don't know where the clumps come from over the longer tubes.

McKibben: The second reason, and I didn't mention that, is, suppose the clump does hit it; you can't really start a spark because the space charge just blows the thing up. You can't start a spark down through the center of the tube because the space charge would blow it up. It might trigger something through the electrodes, and that you can't throw out.

Larson: I think that it is sufficient to just breakdown at one pitch or very few pitches to trigger the whole thing off.

McKibben: That, unfortunately, is true, I think.

Middleton: It is a possible solution and I believe that this was aired by Dick Hyder in Strasbourg, to have a conductive coating on the insulator on the inside. I think perhaps Lionel Fell should be able to comment on that. Haven't you tried putting on some kind of chromic-oxide coating on the inside?

Fell: Answer not recorded.

McKibben: I have had the pleasure of trying two different resistive coatings during my time and one was at Wisconsin. One called Rescon, which was a carbon coating, was actually used on the pressure system at the University of Pennsylvania machine. That was terrible! Then when the GE machine was built, and Clarence Turner ended up working on it when it went to Brookhaven, they got Katherine Blodgett to put a lead glass on it. I think maybe she had something there except I wasn't ready for it and probably didn't give it a fair test. The mechanism that I just proposed should be helped a great deal by a resistive coating.

Middleton: If I understand you correctly, Joe, you seem to think the problem with the insulator is accumulation of electrons on the face. You are making efforts here to direct electrons away. It would seem to me that the alternative approach would be to drain the electrons off the source.

McKibben: Right.

Larson: There is a certain amount of confusion because this is a rather complicated subject, I would say. You are talking about a conductive coating, the chromium trioxide, which I believe is the material of the coating, is not conducting. The purpose of that coating is to change the secondary electron emission coefficient, and thus change how the charging process or the avalanche process, if you will, might take place. A conducting coating I think would be very desirable to drain off the charge because there is evidence that the charges can last for hours. Either the conducting coating or the bulk conducting insulator might keep the insulator from charging appreciably and, of course, that's desired. These other coatings, which affect the secondary emission properties, are intended to change the polarity of the charge. That is, if you have a high secondary emission probability, you get a positive charge, and the consequences of that are thought to be to stress the cathode corner more because of this positive charge on the insulator. If you reverse the situation and actually accumulate electrons, which you can't do on most materials, hopefully, you would have the opposite effect. Shaping can affect things as well as changing the secondary emission coefficient. It's a complicated thing and the problem is that all this complexity doesn't seem to explain the simple fact that breakdown occurs.

McKeown: When we run three-stage, we have a glass tube in the negative ion injector of MP-7, which is ordinarily filled with air when we run two-stage to keep the 150 kV from breaking down across that tube. I did coat the inside of that tube by sputtering with iron electrodes. I had an iron coating with something like 100 megohms per inch and that stopped the breakdown of that tube. Coating does something there.

McKibben: I guess Trump has worked a good deal with lead glass and he found that was never a particularly good solution, not lead glass but a conducting glass. In that case, I suppose that can be explained by the lack of homogeneity in a glass. It would be very difficult to get a conducting glass material that is satisfactorily homogeneous, that would give you a better distribution than none at all. That can also be a problem with a coating and also can deteriorate with all that heat, etc.

Adams: In recent years, since they developed the metal oxide resistors, it has been my contention that one should go to thick coatings rather than going to a thin coating, as Mike has done, because these thin coatings are very unstable. Many things such as heat can change the characteristics and you can run into a problem. These metal oxide coatings are thick coatings and you cook the inside of the glass. You also have a built-in resistor because you can adjust the resistance of the coating by the ratio of the oxide and the metal you put into this thing. You have a built-in resistor chain and drain for your electrons.

Larson: I think we have to be a little bit careful about the consequences of the mechanism for these various coatings. What Mike was talking about is a very low-gradient situation and simply doing a little better distribution of grading

there, along an otherwise ungraded tube, with a little over a 100 kV I think is the change that improved the situation. I believe that we are talking about quite a different situation where the gradient is an order of magnitude or more higher. You are dealing with new phenomena under those circumstances. It is not just a question of linearizing the grading anymore.

Wegner: You might consider the analogy to the channeltron. The channeltron is made of a highly insulating kind of glass matrix, and the inside of the little tubes are coated with a slightly conducting kind of glass, but it's still glass, not like a conductor in any sense of the word. There you, of course, take advantage of the gradient along the glass and the secondary electron production of that surface. Those things, of course, are exactly what you don't want in an insulator and an acceleration tube because they give you a factor of 10^6 gain per electron. So you can imagine that if you are setting up that kind of condition that you find inside a channeltron, you are just asking for breakdown along the insulators. I would suggest that there are two problems; one is, possibly, conductivity to get away from charge build-up locally which causes high strain, but the second thing to really worry about, is secondary electron production.

Broadhurst: I would just like to reinforce Harvey's comments. A lot of the mass spectroscopists use detectors which are flat plates of glass coated with metal oxide, because this makes an excellent linear channel multiplier; therefore, one has to be careful if you put on a conductive coating that it is a very poor secondary emitter at the same time.

Larson: Keep in mind that what we want is a linear gradient across this surface. Any nonlinearity is intrinsically rather bad unless you can arrange that you are shielding the cathode corner, in the same process that is reducing the gradient at that corner. I think the analogy with channel plates and channeltrons is very good. You have that and what you want to do is control the situation as best you can. You can't get away from it. In my perspective, the fact that a surface must exist from anode to cathode is the problem. The trick now is to find ways to get more voltage across that surface, and grading it uniformly with a partially conducting surface may be a very good thing to do. I don't think you should draw the wrong analogy from the channeltron. Also, as I mentioned in my talk, the avalanche process does not necessarily lead to breakdown as long as you can support the current that it drains. The process you have to worry about is the one that builds by regeneration, by transfer back and forth, but if it's unidirectional, it's just a question of how much current flows from the cathode, how much multiplication, and whether you can support that current.

Liebert: Has anyone tried to put in the kind of standard little shims on the electrodes to reduce the field intensification at the corners where the insulator comes in?

McKibben: I tried this in connection with these tests that we did years ago, and every time I tried one of those I got less voltage. I would like to make one small summary. It seems to me, it is the observation of facts through experiments, rather than theory that counts, and we tend to get off into a theoretical discussion. But I think it does indicate that we should give more attention to a back-bias system and recognize that the inclined field tube has a weakness in this respect. Therefore some work should be done somewhere, but I will never do it because I am about to retire.

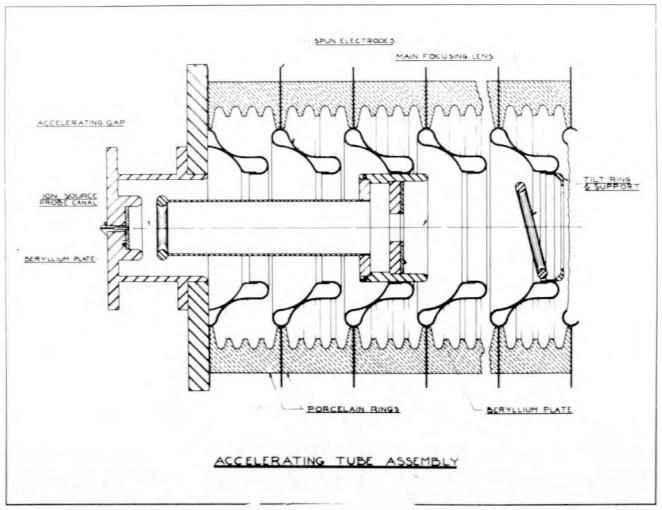


Fig. 1

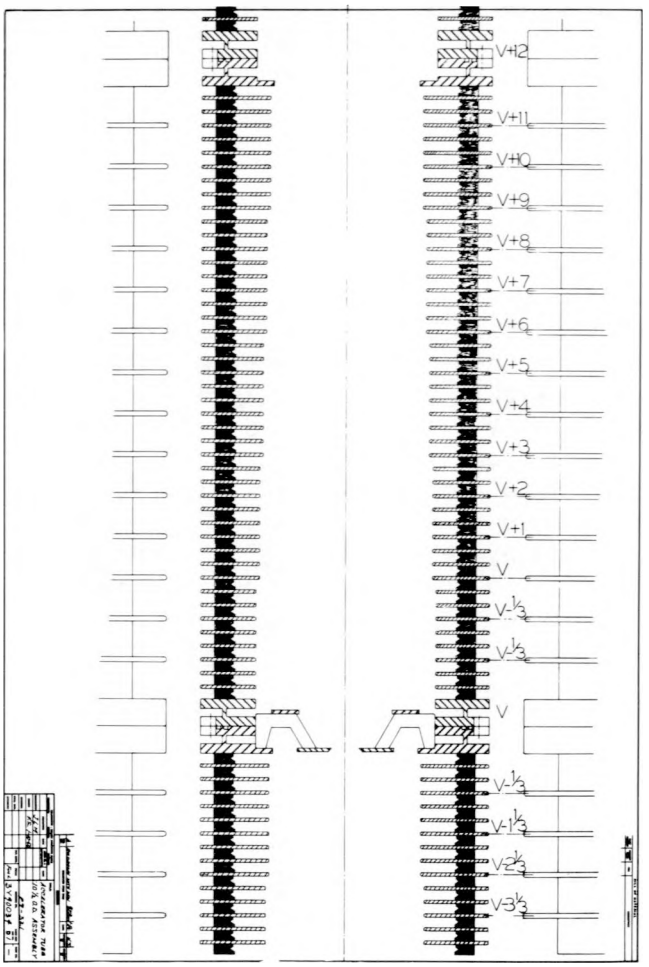


Fig. 2

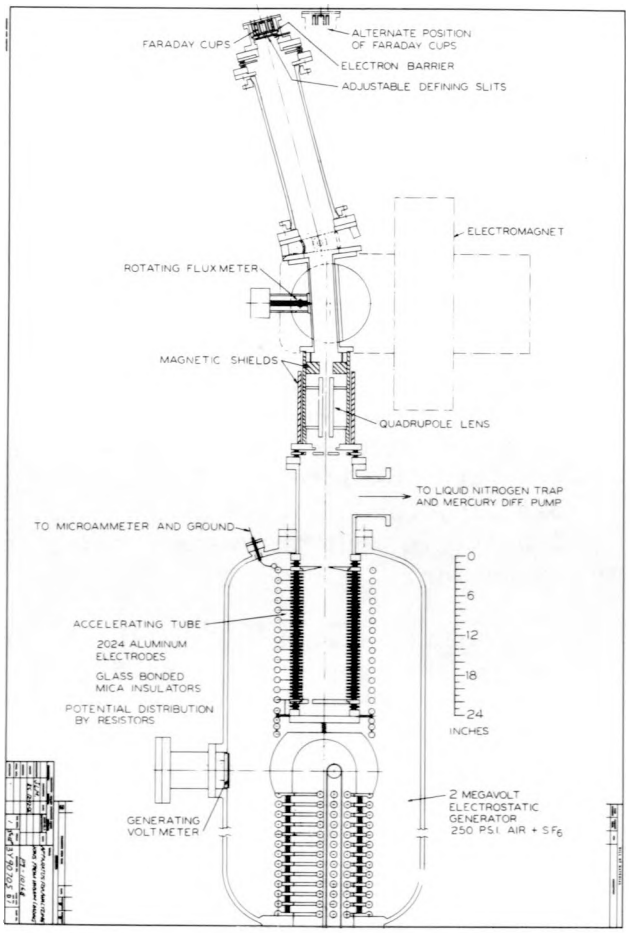


Fig. 3

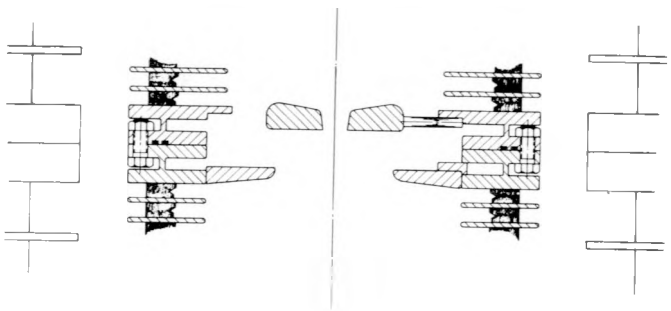


Fig. 4

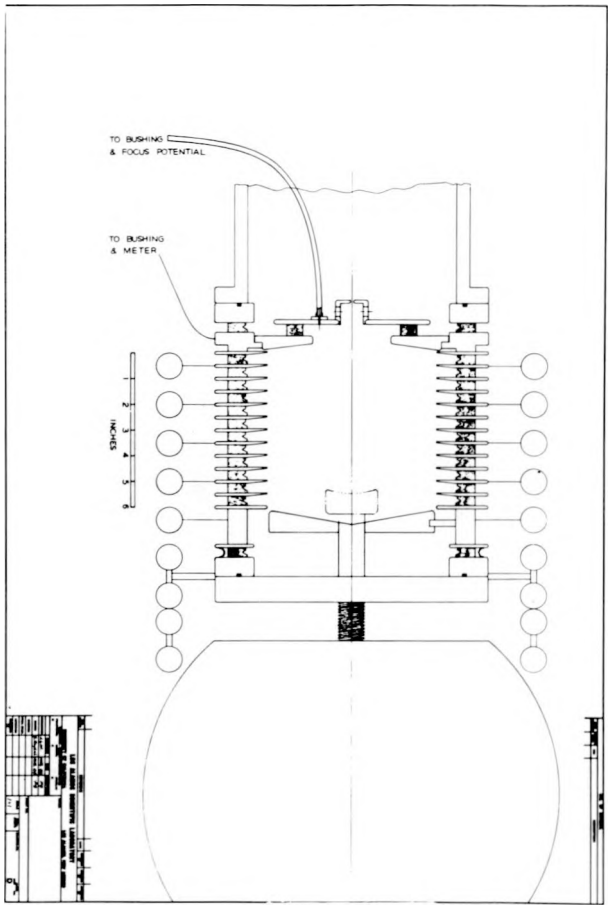


Fig. 5

Session X, Chairman, J. McKay

SNEAP Business Meeting

The single ended machine people still feel slighted at our meetings. Some method of identifying those and ascertaining their problems needs to be found. Perhaps identifying themselves on the SNEAP membership form would help.

An offer to hold the next meeting at Oak Ridge next fall was unanimously accepted.

The financial statement through September 26, 1977 is attached. Also attached is an accounting of the monies taken in and disbursed for this meeting.

SNEAP

Financial Statement for SNEAP, September 26, 1977

November 1976	329.66
1977 SNEAP Membership (29)	287.48
Printing Expenses SNEAP 1976	446.32 DR
Balance in Account Sept. 1977	$170.82

(All funds in Canadian Dollars)

Financial Statement for SNEAP 1977 at LASL

Income

Registration Fees (63)	315.00
Banquet Tickets (70)	490.00
HVEC Donation	100.00
NEC	100.00
TOTAL	$1,005.00

Expenses

Banquet	490.00
Refreshments for Banquet, Open House and Coffee Breaks	227.33
Banquet Hall Rental	25.33
Secretarial Services	100.00
TOTAL	$842.66
Balance	$162.34

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Orsay, 13th September 1977

Secretary of the 1977 SNEAP Conference
(Sept.26-28)

Los Alamos Scientific Laboratory
P.O. Box 1663

LOS ALAMOS NEW MEXICO 87544 U.S.A.

Mr. President,

We regret not to attend the SNEAP conference this year.

Yet, we take the opportunity to give some informations on our MP tandem. Concerning the machine itself we continue to run easily at 13 MV as we said at the conference on Electrostatic Accelerator Technology, last May in Strasbourg. About this, please find enclosed a diagram (fig.1) of experimental time vs terminal voltage.

On the other hand we can give some informations about the production of negative heavy ions beams on the machine.

About 50% of beam time is sheduled for heavy ions experiments. ^{12}C, ^{14}N, ^{16}O and ^{18}O, ^{19}F, ^{32}S and ^{34}S, ^{40}Ca, ^{56}Fe were used for experimental work on the machine.

Before setting up of the 834 Genionex source on the machine, heavy ions beams (except ^{12}C and ^{14}N) for runs in Nuclear Physics, were produced by Penning source or Penning with sputtering (Orsay design (1)(2)).

For calcium, we obtained till 120 nA on L.E. Faraday cup of CaH^-. In spite of the improvement of cathodes cooling, lifetime of the source was 24h but it was easy to exchange the source for another one and so we ensure many experiments 8 days long.

With this source, as well for Ca as for O, the mean transmissions were of 60% without quanching gas (max. 70%).

In 1976 we tested the Genionex source on our test bench.

We work with standard cone shapes (Ø 2mm (0,80") - angle 40°).

On test bench, we produced H, Li, B, BO, C, O, F, MgH_3^-, Si, CaH_3^-, Fe, Ni beams.

For example, for calcium hybride we obtained 600 nA with a lifetime best than 70h, with one cone.

In last January, the Genionex source was set up on the machine in the Penning position.

The source was used in nuclear physics experiments for ^{18}O, ^{16}O, F and ^{13}C. In ^{18}O the consumption is weaker than the Penning source but transmission of the former is a little less : 50%.

Recently, we had the opportunity to make an experiment in MgH_3 according to the Middleton's method.

We sent a spray of NH^3 (partial pressure 4 to 6.10^{-7}torr) and we obtained 300 to 450 nA of MgH_3^- on the low energy faraday cup. The MgH^- intensity was the third of the MgH_3^-.

We accelerated this beam in the tandem at 10 MV terminal voltage, with a 5 $\mu g/cm^2$ carbon foil stripper and we measured the current at the high energy end of the tandem (F.Cup nr 1) and after the slits (2,5 mm total) of the analyzing magnet (F.Cup nr 3) we obtained :

Low energy F.Cup MgH_3^- nA	F.Cup nr 1 µA	Charge state	F.Cup nr 3 nA
300	1	7	95
300	1	8	100
300	1	9	17
300	1	10	0,9

The transmission through the machine was about 45%.

For one cone, after 4 1/2 hours the intensity decreased but was still 100 nA.

Few minutes after the end of this test run we could continue the nuclear physics experimental program with a $^{12}C^-$ beam without any pollution of the ionizer with NH_3.

To conclude, we hope that John McHay have received our 1977 member ship and that we will received the SNEAP report.

We wish a good success for the conference and we remain,

Yours sincerely,

P.BRETONNEAU M.DUMAIL

References :

(1) "Production d'un faisceau de Fe^- par pulvérisation cathodique dans une source Penning" NIM 112 (1973) 607 - M.Dumail et G.Chauland-Lottet.

(2) "Une source de CaH^- pour accélérateurs Tandem" NIM 127 (1975) 305 M.Dumail et J.P.Mouffron.

P.J. : 1 figure.

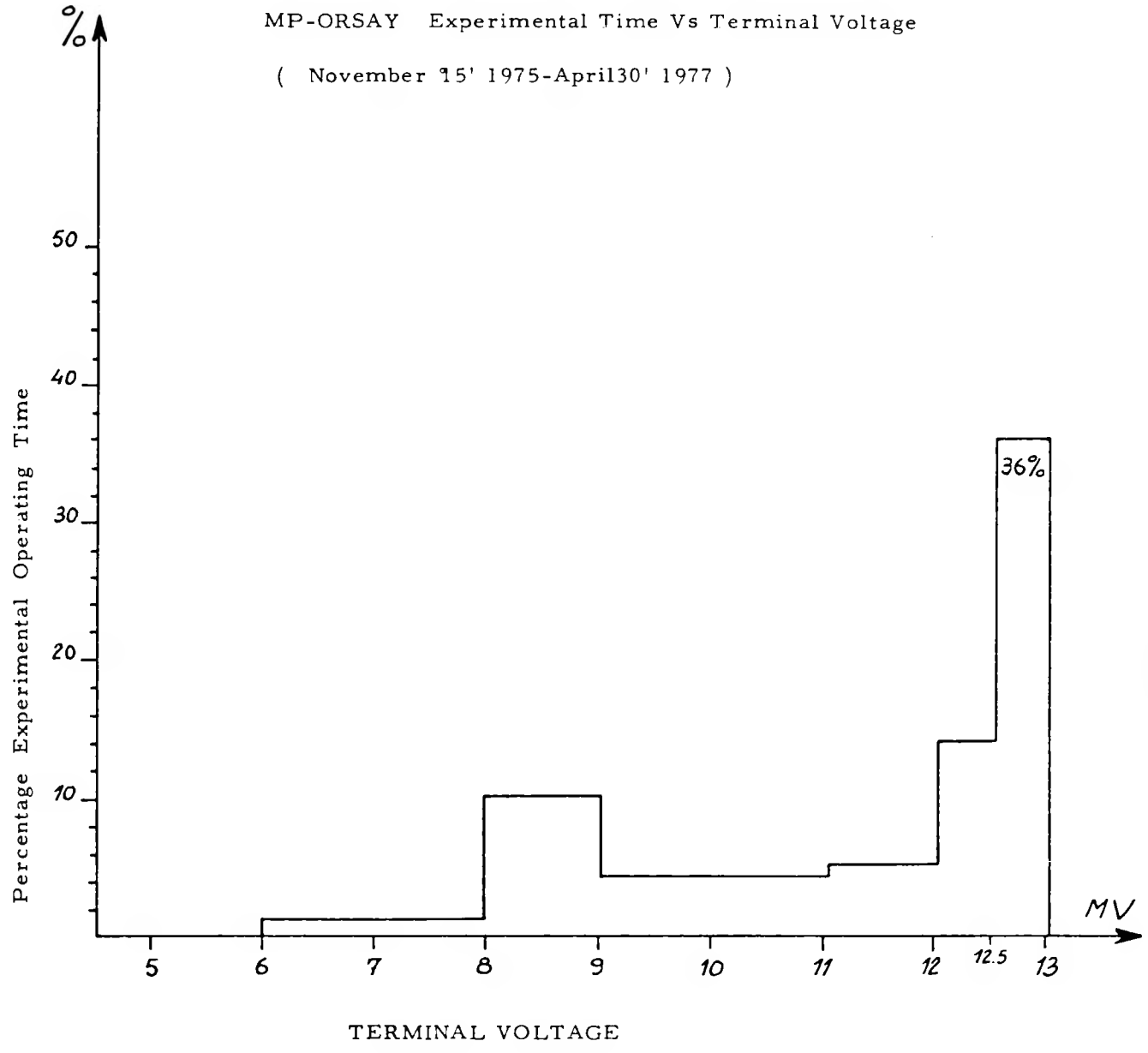
MP-ORSAY Experimental Time Vs Terminal Voltage
(November 15' 1975-April30' 1977)
%
50
40
30
20
10
Percentage Experimental Operating Time
36%
MV
5
6
7
8
9
10
11
12
12.5
13
TERMINAL VOLTAGE

FIG: 1

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à Basse Energie

Dr R. WOODS
Los Alamos Scientific Lab.
LOS ALAMOS NM 87545
U.S.A.

SACLAY, le September 15, 1977

Dear Dr R. Woods,

Unfortunately we have not the possibility to join you for the SNEAP meeting this year.

We would like to thank you very much for your letters about this conference and we hope that it will be very interesting for you.

If you plan to publish something after the SNEAP meeting, we would be very interested by these proceedings.

About our machine, since the replacement of the "blue" resistors by the old "yellow", all things seem to be OK : no sparks, no radiations and good capability to obtain 9 MV.

Sincerely yours.

L. BIANCHI - J. GASTEBOIS

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