# INFORMAL DISCUSSION OF SPARKING PHENOMENA

SWENSON: We are fortunate to have a representative in this room of practically every proton linear accelerator in the world (Brookhaven, Argonne, CERN, Rutherford, Minnesota, New and Old Bevatron Injector, and the original Alvarez Linac). I have a short list of general questions which I would like to pose to a representative of each of these linacs. These questions were chosen to reveal the type of information which you as a group have and to stimulate a discussion on sparking phenomena. The questions that I have go something like this. Is sparking a problem in your linac? If so, where are the sparks? Why are they there rather than at other places?

HUBBARD: If we start talking about "why", we'll be here for a week.

SWENSON: I would also like to ask you if you have evidence that the sparking is beam dependent, dependent on the vacuum pressure, or dependent on surface contamination. Another question might be for what gradient would you build your next linac? And of course aside from answers to these questions, I would like to have any comments that you feel are pertinent.

Perhaps I should try to define the purpose of this session. The economics of linac design favors pushing the cavity excitation up to some level which is considered safe from the standpoint of sparking. There is very little reliable information on what the safe limit is. And there is no popular description of the nature of the sparking process which is observed in proton linacs. That is, there is no model of the process which explains why the sparks are where they are. Another important point is, how reliable must this linac be? That is, can we stand some sparking and can we allow some time for conditioning after the tank has been opened to air? I know there are some in this room who feel that the future linacs should be very conservative in their electric gradients and if this is the case, perhaps it is not necessary to understand the sparking phenomena.

VAN STEENBERGEN: That last comment I did not get. Why should future linacs be conservative in electric gradients?

HUBBARD: I will write three numbers on the board if you like. In 1948 the Lawrence Radiation Laboratory built a 200 Mc per second linac with a gradient of 0.9 MV/ft. In 1952 we built another with a gradient of 0.6 MV/ft, and in 1961 another linac with a gradient of 0.5 MV/ft.

LAMB: In 19-- something, it will probably be 0.4. It makes a great deal of difference what the application is. For an injector for a machine that costs x times the cost of any linac you are going to put on it, you can certainly afford to run it at half that gradient if you don't get spark one.

FEATHERSTONE: Do you worry about spark one?

LAMB: After it is baked in, you do, I think. You just don't want any sparks.

WHEELER: I think we tend to agree with that.

VAN STEENBERGEN: But isn't that demanding a bit too much. I mean, in all high voltage experiences, one normally expects some conditioning of a particular device under consideration, resulting in usually significant improved performance with regards to maximum electric fields, etc.

LIVDAHL: In January, we were open to air for one week. We did lots of things. Granted, we did not remove drift tubes, but after a week we pumped down late one afternoon. We came in the next morning, and in 30 minutes we had operating gradient. I do not think we had seen more than probably 200 sparks in that time and within an hour we were 10% over gradient, which is where we like to operate.

BLEWETT: Was the tank open to room air, dry nitrogen, or what?

LIVDAHL: It accidently got let up on kleenex and alcohol.

FEATHERSTONE: I think it is fair to say that often when a tank of the Minnesota linac has been down to air and has been pumped overnight, we can establish beam in half a day. It is another story, though, when we have gotten the drift tubes well coated with oil.

VAN STEENBERGEN: From that point of view, I feel again that the gradients could be made higher. It should be a rare occasion that one has to open the tank to air. In that case, conditioning periods of even a day or so would be acceptable.

HUBBARD: But if you are at a point where it requires a lot of conditioning, you are at a point where it will spark occasionally, even after you have done it.

SWENSON: Let me try the first question. Is sparking a problem in your linac?

BLEWETT: (On the BNL linac.) We have some color photographs of the first few drift tubes in our linac. Horrible as the drift tubes look, they still feel smooth to the touch.

VAN STEENBERGEN: Our experience is that in normal operation sparking is not a problem. These photographs look horrible, but we feel that this happens during the first half week after you have had the tank open.

BLEWETT: We feel it is important that the new injector operate at a 30-pulse per second repetition rate to speed up conditioning.

LIVDAHL: Our conditioning is done at 10 pulses per second which is higher than the current possiblities at Brookhaven and at Berkeley.

VAN STEENBERGEN: Let me make it clear that we do not see any deterioration of the sparking situation with time. We have never opened the linac to clean or treat the drift tube surfaces because of sparking.

LIVDAHL: (On the ANL linac.) I do not consider sparking a problem in the Argonne linac. I have seen no spark marks on any drift tube beyond the 20th gap.

BLEWETT: I think we can go even farther and say that we have seen no sparks beyond the 5th gap.

TAYLOR: (On the CERN linac.) I would say that sparking in our linac is a fault condition, that is, we open up the linac and find, for example, a faulty contact between the drift tube stem and the liner. When the linac is working properly, we just do not lose pulses because of sparking. We might have trouble for a day after being up to air for a week or so. The sparking damage that we see is concentrated on the first ten drift tubes.

DICKSON: (On the Harwell linac.) Sparking is not a problem now in the Harwell linac. A couple of years ago it used to be a problem in Tank one. We do not have direct evidence, but we think it was due to water vapor in the tank. I think I told some of you that we found some of the copper in the drift tubes to be porous to water. Since we have cured the porosity problem, we have a very short run-in time at 50 cps, at 1% duty cycle, and the linac seems to operate properly just for years with no problem. We see a few sparks on the first ten drift tubes but they do not bother us.

FEATHERSTONE: (On the Minnesota linac.) Sparking has never really been a problem on the Minnesota linac. The first cavity is a low gradient cavity, like the old Bevatron injector, and it just never sparks. We do see evidence of sparks on the drift tubes of the second and third tanks. There is no apparent concentration of the sparks in any particular place along the length of the linac. They seem to appear at random.

SWENSON: I might say at this point in regard to the Minnesota linac that I calculated the fields in the vicinity of the drift tubes at three different places, that is, at both ends and at the middle, of their second and third tanks. There are some fairly small radii of curvature on the corners of the drift tubes at the high energy end of Tanks 2 and 3, and I got some electric fields on the metallic surfaces of 28 and 26 MV/m, respectively. I can present a table which compares some actual calculated quantities for the Minnesota linac with similar quantities in some of our latest linac designs.

| | Units | Minnesota Linac | MURA Design Run 30547 | MURA Design Run 30525 |
|---|---|---|---|---|
| Energy | (MeV) | 39 | 39 | 195 |
| $E_{ave}$ over cell | (MV/m) | 3.0 | 2.8 | 2.3 |
| $E_{ave}$ over gap | (MV/m) | 12.0 | 9.8 | 5.1 |
| $E_{max}$ on axis | (MV/m) | 11.7 | 8.1 | 6.8 |
| $E_{max}$ on metallic surface | (MV/m) | 28.3 | 15.1 | 13.4 |

By all the criteria listed in the left-hand column, the MURA designs are conservative in comparison to the Minnesota linac. However, it might be an error to assume that the critical criteria is listed in the table. Additional information is available on these calculations in a MURA Technical Note TN-467.

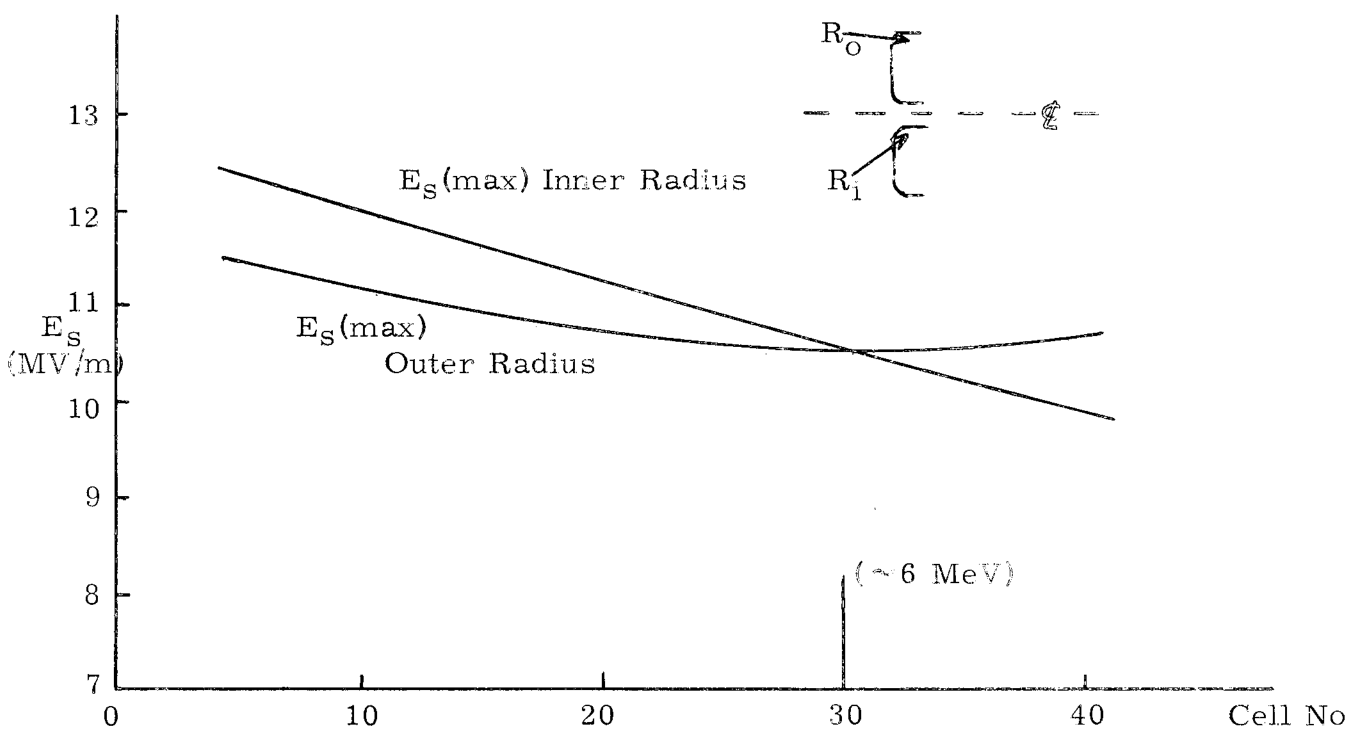
FEATHERSTONE: Incidentally, the location of the sparks that we do see does not coincide with the location of the peak fields on the drift tube surface. We tend to get a concentration near the bore radius.

REMARK: That is probably where the field is the highest.

CARNE: May I throw some wood on the fire at this point? It was mentioned in the PLA Progress Report for 1963* that we have done a redesign of our Tank 1, and we were quite concerned about breakdown. We have

*PLA Progress Report 1963, pp. 8-10, NIRL/R/60, January 1964.

done a whole series of electrolytic tank measurements to determine the electric field gradients across the drift tube surface. If you plot the field distribution along the drift tubes surface, you get a peak in the distribution where the outer radius of curvature meets the flat of the drift tube; you get a uniform distribution across the drift tube flat and another peak in the distribution near the radius of curvature of the bore hole. If we now plot surface fields against drift tube number (we have 41 + 2 half drift tubes in our machine) as in the sketch below, we find for the inner radius a surface electric field which goes from about 13 million volts per meter down to about 10. The field for the outer radius is initially less than for the inner radius, crossing over at about 6 or 7 MeV. So for the first 6 MeV it is the field near the inner radius of curvature that is critical. This may throw some light on the fact that the sparking seems to be concentrated in the first few MeV and that the sparks seem to be concentrated on the flat drift tube faces, or near the radius of curvature on the bore.




Plot of Maximum Surface E-Field Against Cell Number

HUBBARD: (On the new Bevatron injector.) It's not serious. I do not have any detailed information, but they do not lose any pulses in the machine. They do see evidence of sparks on the first few drift tubes but it does not interfere with the operation. They had trouble on the original Alvarez machine with sparking. Of course, this was with a poorly baffled oil pumping system. With regard to location of sparks, they were worse at the low energy end.

BLEWETT: I think what bothers everybody is that nobody seems to understand the mechanism.

SWENSON: That was my next question. Why are the sparks where they are rather than at some other place?

BLEWETT: Could I make some general comments? First of all, the story you get from the electrolytic tank is not the whole story. The rf magnetic fields are certainly playing a part in determining the electron trajectory. We found that whereas an electron may start off in the direction of the electric field, the magnetic field may very well turn it around. We know that electrons can get all the way across the gap. I want to propose, now that we have all the drift tube fields worked out on computers, that we ought to trace the trajectory of some ions and electrons across the gap and see what final energies and phases they have for a variety of starting phases.

HUBBARD: Are you going to insist that they get there in a half cycle?

BLEWETT: No, I think you should run the calculation for several cycles.

(At this point in the session about two-thirds of the people left to return to the regular afternoon session of the conference.)

CARNE: There is a theorem that says a nonuniform stationary rf field will have a dc force acting on any charge particle, directed toward the minimum in the field. We actually tried to use this force at one time to drive the electrons and ions towards the center of a resonant cavity. We were of course driving the electrons there much more easily and they were in fact sucking the ions in. If you look at the situation that you have across the face of the drift tube, you find, as I said earlier, a nonuniform distribution of field, with a minimum in the region of the face of the drift tube. So there can be this dc force acting on ions which will tend to drive the ions to the region of the flat face of the drift tube. If ions are indeed involved in the sparking process, this could explain the occurrence of sparks on the drift tube faces.

SWENSON: That would enhance the sparking on a flat face, would it not?

CARNE: Yes, certainly. Now the other point to observe is that as you go to higher energy drift tubes, the peak field near the bore radius decreases and the ions either tend to collect on the axis of the linac or tend to slide along the drift tube surface to the stem. If anyone has time to do the details of this calculation on a computer, I will certainly send him the details of the derivation.

PREIST: That is a very reasonable theory.

BLEWETT: It would be very interesting to take two cases, one at high energy and one at low energy and trace trajectories through the cell. The calculation ought to be run for several cycles, including both the electric and magnetic fields in the calculation.

FEATHERSTONE: I would like to point out another aspect of sparking, a question which has not been mentioned but may be significant in some cases. That is, when does sparking tend to clean up, as is usually observed, rather than produce progressive damage, as has been observed in a few notable accelerators? I have calculated that the energy stored in the Brookhaven tank is sufficient to vaporize several milligrams of copper, which is perhaps three orders of magnitude larger than any sparking damage that has been observed. So apparently only part of the energy in the cavity gets into the sparks.

BLEWETT: Yes, if you are watching the probes when the spark occurs, you can see the field drop to zero in the low energy end, and it is still there in the high energy end. With respect to the question made by Featherstone concerning the stored energy, there has been a criterion established by the beam separator people. They believe, if you store more than 50 joules in an electrostatic field, that the sparks will begin to be destructive.

HUBBARD: Yes, that is certainly a rough number, but the 50 joules is somewhere in the region where you might get damage from sparking.

BLEWETT: I think in the case of the Brookhaven linac, where we have about 100 joules, that the sparks detune the cavity and the power is dumped back into the amplifier.

PREIST: Have you come across this report by Little and Whitney at NRL?* People have suspected for years that what caused sparking to

*NRL Report #9544, dated 5/20/63, by R. P. Little and W. T. Whitney, entitled "Studies of the Inhibition of Electrical Breakdown in Vacuum." Astia Document #408298.

start in the dc case (that was all he was talking about) was the phenomenon of whiskers on the metallic surface. And he did a rather elegant experiment in which he saw the whiskers grow and actually measured them, and he computed the increase in the electric gradient that they caused and he found that they had a factor of 100 times the average gradient, and they found that no matter how well you polished the surfaces, that the whiskers were always there.

There is another paper, of which you might be aware, published in the Journal of Applied Physics in June of this year,* on gaps with dc fields in a vacuum. Their approach is to cover the cathode surface with a thin film of dielectric and they had remarkable success in two respects. One is that the ultimate breakdown voltage after conditioning is increased by a factor of two and the other is that (and they consider this most important) the pre-breakdown currents are decreased by three or four orders of magnitude, when you put this stuff on. They used some epoxies and fluorides and $TiO_2$. They point out you have to have a thin layer, and you have to have some electrical conduction. The whisker-growth idea may explain some of the area effects which were discussed in the main conference room. Of course, the bigger the area the more chance there is for whisker growth.

BLEWETT: This is probably quite true with dc, but I would be quite surprised at whisker growth in an rf field especially since the duty cycle is quite low.

CARNE: How large are the whiskers?

PREIST: I think they are typically a micron in diameter and some hundred microns long.

FEATHERSTONE: Is the presence of electric fields essential for the crystal growth or is it a chemical process?

BLEWETT: It must be an electrical process. I think the primary argument against this as an explanation of sparking in the rf case is that conditioning of the tank tends to get rid of sparking, whereas whisker growth should lend itself to a continuous process.

We have seen another funny effect. It was after a process of cleaning the drift tubes, in which we were using some sort of abrasive. We

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*"Vacuum Insulation of High Voltages Utilizing Dielectric Coated Electrodes", by L. Jedynak, June 1964, JAP, p. 1727.

must have imbedded some sort of insulating particles in the drift tube walls. These did not result in any sort of disruptive breakdown, but if you peered into the cavity, you could see dozens of little stars on the drift tube faces at the lower energy end on every pulse.

SWENSON: Are there any other models which can help shed light on the sparking phenomena?

CARNE: Well, it seems that probably the rf pulse length is relevant. It would certainly be nice if someone would do some high power model studies (which I know you are going to do at MURA) at the actual pulse lengths for which you intend to run the machine.

HUBBARD: Isn't pulse length important because it affects the duty factor? Our experience suggests that you can convert from one duty factor to another just by taking the sparking rate proportional to the duty factor.

SWENSON: Have you seen anyevidence for beam dependent sparking?

LIVDAHL: We have seen beam dependent sparking at Argonne but only in the case where the operator had steered the injected beam off to one side so that a majority of the beam piles into one drift tube, but in situations where the beam is properly steered into the tank, I can't say that anybody has ever seen it.

BLEWETT: We have not seen beam dependent sparking in our linac.

SWENSON: Lamb reported that he had seen beam dependent sparking, but I guess that was on the MTA.

HUBBARD: Yes, that is true, but they had very high currents, a quarter of an ampere at times.

PREIST: Now, I would like to mention one more thing. Something that has not been important yet on 200 Mc machines, but may become important on the machines you are talking about. The effect I speak of is multipactoring on dielectric surfaces. I hear people talking about a megawatt or so that has to be put through some kind of window. We've found, and we've done a lot of work on this, that we do get a single surface multipactor in a strong enough field, where the E field is parallel to the dielectric surface; you can work out the critical field for this process. You find a minimum field strength at which it can occur, of about 1 V/cm/Mc/sec. The mechanism is that the electron leaves the surface with some initial velocity. It will then be carried parallel to the surface by the electric field, and if there is a restoring force, it will come back, and if it comes back in the right place and with enough energy, you will find that the process can continue. Well, this has been found to occur at three frequencies: about 600 Mc, about 3000 Mc, and 9000 Mc. It can be eliminated completely by suitable coatings applied to the dielectric surface.