RF SUPERCONDUCTIVITY MEASUREMENTS

J. M. Dickson
Rutherford High Energy Laboratory

The feasibility of designing a proton linear accelerator with superconducting cavities has been studied by A. P. Banford at the Rutherford Laboratory. The work has been mostly on the measurement of the surface resistivity of metals at 4.2°K, since it was realized that it was vital to this project that a workable means of producing large areas of superconducting surfaces had to be devised, and that nearly all the other problems were easy by comparison with this one with the notable exception of the problem of beam loading. The ideas formulated in the first papers on this subject were based on results obtained with small samples of pure metals. The rf measurement work has had two aims (1) to measure the surface resistivity of superconducting electroplated metals and of superconducting alloys and (2) to attempt to build a small superconducting resonant cavity with low surface resistance.

The rf measurements consisted of the measurement of the Q of resonant devices at 4.2°K and comparison of the results with Q of a dimensionally similar resonant device of copper measured at room temperature. The ratio of the two Q's gives the "improvement factor", which is the factor by which the rf power loss of a resonator would be reduced relative to a similar copper resonator. The approach has been to try to use plating or other techniques which would be applicable to large areas and not to try to relate the results to the physical state of the surface. Most of the work has been with simple $\lambda/4$ resonant lines in the form of a hairpin, weakly coupled to a pulsed rf source and a detector. The Q was estimated from the decay rate of the detected pulse. The result for several surfaces are as follows:

1. Lead, extrapolated onto copper with a standard commercial fluoborate solution, has given improvement factors up to 15,000 (the theoretical limit at 400 Mc/s is 40,000). Solid "Specpure" lead wire gave slightly worse results.

2. Solid niobium wire, after electropolishing, yielded improvement factors of up to 13,000.

3. Niobium deposited from $NbCl_5$ vapor, or electroplated from a molten salt gave results similar to (2) after electropolishing.

4. Inhomogeneous superconductors, such as Pb-Bi eutectic and Nb_3 Sn gave very poor improvement factors ($\sim$50). This result can be explained by the filamentary nature of such materials, which is of course not a disadvantage when dc superconductivity is required.

5. Homogeneous alloys, such as Nb-Zr and Mo-Re gave improvement factors of about 2000. This can be explained simply by the fact that their resistivities fall rapidly below the critical temperature, but change very slowly above it, in contrast to the behavior of elementary metals.

Thus there seemed to be grounds for believing that large surfaces of lead or niobium could be prepared with suitably low surface resistivities. Some measurements on dielectrics at 4.2°K showed that PTFE (teflon) was the least lossy material tested and that it had a tan δ of about 2.5×10^{-6}.

The type of resonant cavity chosen was a half-wave coaxial line, short circuited at each end and split perpendicular to the axis at the central current node. All the cavities were made by electroforming in copper and subsequent electroplating with lead. It was intended that these cavities should be used at high power and that current carrying joints would be tested, but all tests were in fact made at low power. Ten sets of tests were made but the best improvement factor measured was 2000, while all the others were less than 1000. Variations in plating techniques, ambient magnetic field, thermal contact and liquid helium level were all tried to attempt to eliminate the cause of the poor results. An improvement factor of 2000 would mean that a 50 MeV proton linac would have an rf dissipation of 1 kW. A 4.2°K refrigerator for such a machine would cost about £400,000.

To account for the poor results one can postulate a residual resistivity to represent the unexpected extra losses, which can occur due to losses at joints, radiation through holes, etc. It can also include losses due to uncoated areas and parts of the surface held in the normal state magnetically or (unlikely) thermally. These areas would have an rf loss at 400 Mc/s about 4000 times larger than the ideal superconducting surface and if they totaled 1/200th of the resonantor surface, the improvement factor would be reduced from the ideal 40,000 to the 2000 found experimentally. This degree of imperfection might also be expected to apply to the hairpin measurements, but it does not appear to do so. However, only the best results with hairpins have been quoted and some poor results ($\sim$4000) have been obtained with electroplated lead hairpins. Perhaps there is a statistical factor, which favors small surface areas.

These results can be compared with those obtained by other laboratories. At CERN Sussini using 260 Mc/s capacity loaded $\lambda/4$ coaxial resonators has never obtained improvement factors greater than 2000 to 3000 with lead and niobium. At Newcastle University Armitage using open ended 300 and 600 Mc/s $\lambda/2$ coaxial resonators has not exceeded an improvement factor of 40 yet with lead. At MIT Maxwell obtained an improvement factor of 50 with a 400 Mc/s coaxial resonator. At Stanford University Wilson obtained improvement factors of 2000 at 2856 Mc/s with a cavity which had a theoretical improvement factor of 3500. However, it must be noted that the normal (anomalous) surface resistivity scales as $f^{2/3}$ and the superconducting surface resistivity scales as f^2, so superconductivity of the whole surface is 14 times less important at this higher frequency. If $1/450^{th}$ of the surface was in the normal state, then the difference between 3500 and 2000 can be accounted for. Thus Wilson has been somewhat more successful in producing a homogeneous surface.

Other problems associated with a superconducting linear accelerator which have been studied include the effects of transient heat transfer and the relative merits of a conventional liquid nitrogen jacket and superinsulation. Thermal problems appear to be unimportant, even gross local overheating, whether steady or transient would not give trouble owing to the greatly enhanced diffusivity of metals at low temperatures. The extra cost of superinsulation would be offset by savings on liquid nitrogen after four years.

The rf tests have not been exhaustive and eventual success may yet be achieved by improved techniques and the expenditure of much time and effort. The results have, however, been sufficiently discouraging to inhibit further work on this problem at the Rutherford Laboratory, for the present.

SCHOPPER: I would like to comment on the beam loading problem. Gluckstern mentioned in the talk he gave here that according to his calculations, you get a limit of about 4 mA or several milliamperes, let us say.

DICKSON: Do you mean milliamperes or amperes?

SCHOPPER: Milliamperes instead of your 10 μA.

CARNE: Gluckstern gave 2 A as the limiting current for the new BNL injector for an estimated 1 M Ω/m for the deflecting mode, and, I think, a frequency of 800 Mc/s. In view of the remarks of Walkinshaw, this current is probably of the order of 1 A. Now the limiting current varies inversely to shunt impedance of the deflecting mode, so that at a given frequency the limiting current will simply be reduced in ratio of the shunt impedances of the normal case and the superconducting case. Assuming that the deflecting mode exists at 400 Mc/s, where the improvement factor is 40, 000, the limiting current will be down by the order of 40, 000 from the limiting current of the normal situation, i. e. of the order of a few tens of microamps.

SCHOPPER: The main thing I want to point out is that you have an advantage in a superconducting linac in that you are working with CW which means that your peak current is equal to the average current. And so even 10 μA average current, I think, would not be too bad.

DICKSON: Well, one has the advantage as far as counting techniques are concerned. I think the advantage is probably a doubtful one in the case of CW beam loading. I don't think one is convinced that CW beam loading is any easier to cope with than pulse beam loading.

LEISS: I don't believe I can agree with this beam loading conclusion that you make. The beam loading is at a different frequency and it is not too hard to consider the possibility of a selective filter which keep the Q of that mode low and leaves the Q of the operating mode high. Admittedly with these leakage problems, it is nontrivial, but in principle it is probably no more difficult than many of the other problems involved. And so I just cannot see this limit.

CARNE: This selective loading may remove the deflecting mode, but in, for example, the disc-loaded guide, there are plenty of higher order modes which may cause deflection, and in CW operation there is time for them to build up. I doubt if one can load down all of them.

LEISS: I am sure that is true, but as you go higher and higher, you are almost guaranteed that the limiting currents go up. I agree there are many technical problems of a very formidable nature, but I don't believe that it is as discouraging as that.

DICKSON: Well, you see, the Rutherford Lab is in business for studying accelerators, not studying rf superconductivity as such. We feel that the present state of knowledge of the latter subject is sufficiently discouraging for us to abandon further work on a superconducting proton linac for the present. This is slightly changing the subject.

LEISS: But there are other people who are intensely interested in the ideas of superconducting machines.

DICKSON: Well, let them have a go.

ROWE: You were estimating what part of the surface area was a good surface in your cavities. Am I correct in thinking that what you did was make a copper cavity, say, and then cover it with lead, or were these cavities solid lead, for example?

DICKSON: The half-wave coaxial cavity we are talking about was one which was electroformed in copper over a plexiglass mold, in two halves, and then the mold was extracted and the inside of the copper cavity was electroplated. In all measurements a copper structure of the same physical dimensions was used for comparison and to find the improvement factor.

SCHOPPER: I want to mention another problem; that is the sparking. I think an advantage of a superconducting linac is that you can use high gradients. If you could go to higher gradients and somehow avoid sparking, then you could build a much shorter accelerator, which would be a saving.

DICKSON: O.K., but you put in two "ifs" which I do not think are really justified.

CARNE: You are suggesting that the sparking limit is a function of temperature. Now there may be something in this, but the sparking limit, we would like to say, is 17 MV per meter.

SCHOPPER: This was just my question. I was asking if anybody has any experience with sparking properties at low temperatures.

DICKSON: No, I haven't. There is one thing that can be said about sparking and it is just that you have still got the same stored energy in the cavity whether it is superconducting or not, because you have the same fields. So when you do get a spark, the same power will be dumped into the spark as in a normal machine, which is of the order of 50 Joules per 20 MeV cavity. 50 Joules might make a mark on the superconductor producing a normal area.

PERRY: The problem of sparking at low temperature, I think, is aggravated by the possibility of condensation of gasses on surfaces, which is an unknown quantity, I believe.

DICKSON: We have no data on this, but we recognize that is another problem.