On The Future of Accelerator Research at A.E.R.E.

The purpose of this note is to provide the basis for a discussion on the future of the 'Accelerator Research' programme. We begin with a historical survey.

It is over 2½ years since an experimental section was set up to study possible 'future machines' by means of 'table-top' models. No machine has been completed, and it seems unlikely that we shall have a useful working electron model in the near future. This is highly unsatisfactory, and the present note seeks to analyse the reasons, to point out some questions which must be answered, and to make suggestions for the future.

The section was set up towards the end of 1955. At that time the accelerator group was contemplating the construction of a 2-3 GeV C.W. cyclotron. The theory of such machines was at that time little understood, and there was clearly an urgent need to press on with a model to study various types of resonance. There were a bewildering number of possible resonances, and the original idea was to make a versatile machine, in which different ridge systems could be tried - in fact poles were to be changeable like 'gramophone records'. Betatron acceleration was to be used for acceleration, since this is simpler than using an r.f. system. This model was in some ways comparable with the 'Mark 1 FFAG' model then under construction by M.U.R.A. at Michigan. Certain differences however made the project more difficult; the full implication of these was not appreciated at the time, and it was decided to start without thinking too much about the difficulties - too much consideration of these would have taken time, and perhaps even frightened us off the project altogether! We resolved therefore to takle them as they came up. Our first aim was to use a smooth pole, just to get the model working and study the $\nu_r = 2$ resonance as an exercise.

Eighteen months after starting the model was till not running even with the smooth pole.

Technical difficulties caused both by lack of foresight and experience and also by the inherent difficulty of the project had bogged us down. These were associated mainly with the very difficult vacuum problem (non existent in the M.U.R.A. model which has separate sectors, and the magnets outside the vac. chamber) and with the

construction of a suitable gun. We needed a three point seal in order to get the thing together; the small magnet gap meant that the whole magnet must be in the vacuum, and the necessity for small betatron oscillations meant that the vacuum needed to be very good to avoid gas scattering. It is not suggested that these difficulties are fundamental just that they were not tackled with sufficient foresight or sufficient experienced staff. Further more the cost and time to make a single model spiral pole for the 'conversion' project was rather discouraging.

By this time also the theory of spiral ridges had been sorted out in a remarkably elegant way by Walkinshaw, and also the large cyclotron which inspired the project had been dropped in favour of a conventional synchrotron. The cyclotron conversion project remained however as a possible machine to be built employing spiral ridges. After a visit from R. Livingston, who described his model cyclotron at Oak Ridge, the work here was reconsidered. In view of our clearer view of the theory it seemed more sensible to change the direction of our work towards a more specific project and to build a scale model of the 'converted' Harwell cyclotron. It was clear that if our results were to influence the decision as to whether the cyclotron should be converted or not (which would be taken in a year or less) that we should have to get a move on. Once more we rushed into things with gay abandon, deciding to use the poles which had been made (costing some £1,000 or more) for the scale magnet poles for the conversion and to correct the fields with concentric coils. We did not pause to consider the problem of tolerances or field synthesis at all thoroughly and furthermore we decided to use the existing vacuum chamber with an inner chamber to ensure the required vacuum whereas a complete redesign was really desirable. Again we were too optimistic, but we needed to be if the model was to be working in time (early 1958). Providing stabilized currents for all the coils has been a major effort, but more difficult has been the design of the magnetic field. This is considerably more complicated than Livingston's, and we have learned the hard way why he says a) that iron is no good, b) that a sufficiently accurate field survey is extremely hard to make, and c) that extreme precision is necessary in winding all coils.

The present situation is that we are about to assemble the model, and hope to look for a beam of some sort as soon as this is done. The field is not as accurate

as it should be however, accessibility is poor because of the rather clumsy mechanical design and the r.f. system has not yet been properly tested. Even if we get some sort of beam it is doubtful what we shall learn of real value.

In this programme the same old mistakes have been made. Not nearly enough detailed calculation and planning at the beginning; unrealistic target dates; not enough experienced staff able to engineer the job properly and do things in the most economical way, far too much 'string and sealing wax' throughout. We have suffered also from lack of space and perhaps a lack of the feeling that the work is important and that people outside the section are keenly interested in and anxious to have our results.

This cannot go on; the work must be organised on a much sounder basis. We must <u>either</u> expand our effort (by introducing experienced men) and our space (a special building devoted to the work is desirable, with a large hall where models can be erected, with plenty of space around); <u>or</u> concentrate on less ambitious projects. If we do this we must be prepared to shut the model down after its preliminary tests even if its performance is poor (as it probably will be).

If the effort is expanded we must decide quite clearly what we want to build, make an extensive and realistic design study in full collaboration with the theoretical section, and then proceed on a project basis with an adequate number of experienced scientific staff and of engineers to carry the project through. A large section of this sort might build simultaneously a 10 MeV ridged cyclotron (the Hangar 7 model is not really big enough to answer all we need to know), a high precision 'air cored' cyclotron like Livingston's but with spiral ridges, and support some experiments of a fundamental nature on plasma beams or space charge. At least thirty people would be needed, including four or five good P.S.O.s. Such a team should be recruited during the next year. The question must be asked: is this a justifiable programme? If so, it must be pressed forward.

If such a programme for any reason proves impractical, we must consider whether to continue with our present programme in the present slow and unsatisfactory way, proceeding from our iron pole to an air-cored pole, with a possible redesign of the whole apparatus to be closer to the Oak Ridge design; or whether to re-deploy the effort in other directions. If the latter, then the following programme might be considered.

1. Following on our experience in trying to set up the field in the model, we could do a thorough investigation of the problem of shimming and field synthesis in S/R machines. This involves theory and computer work, as well as an experimental programme (the large magnet used by Smith for ridge measurement could be used), and should be done in close conjunction with the theoretical section.

2. Stepping up of the relatively simple problem of Q_v and Q_R measurements by pinhole techniques in a static field, now going at half speed.

After the H.7 team has finished work on the 4 MeV machine (1 - 2 years) the combined groups, slightly strengthened in some respects could consider the design and construction of a 10 MeV cyclotron to be started say in two years from now, (or an alternative accelerator if anything else turns up).

This programme abandons study of resonances in a tightly spiralled machine. Would anyone build a machine now with such parameters? If not, this may not be a great loss.

13th August, 1958. J. D. Lawson

Copies to: Dr. Pickavance
Mr. Mullett
Mr. Walkinshaw
Mr. Snowden
Mr. Lawson