INTRODUCTORY COMMENTS

D. L. Judd, Chairman

I rather wish I had the verbal facility of the Secretary of the American Physical Society, K. K. Darrow, so that I could make appropriate remarks on the unaccustomed splendor of the surroundings in which this group of physicists finds itself. Lacking this, I believe that we should get down to business, and I will start by making a few introductory remarks about what I believe to be the purpose of this first session on orbit calculations and magnetic field design. I would like to make some brief historical comments intended to bring us up to date on the subject of sector-focused cyclotrons.

Most of us know that the idea was originated by Prof. L. H. Thomas in 1938,[1] in his by now very well known paper which was neglected for a rather long time; in a sense we are all here because of his work. The first construction of a machine using these principles was, to the best of my knowledge, the effort undertaken at the Lawrence Radiation Laboratory in Berkeley beginning in 1949; the work was stimulated by the independent consideration of the same ideas by Prof. E. M. McMillan at Berkeley, and led to the two electron models which were reported some time ago.[2] Related theoretical work was independently initiated in the Soviet Union during the same period.[3]

Interest in Thomas shims also developed during the early 1950's at the Oak Ridge National Laboratory under the leadership of Dr. R. S. Livingston. In 1954 Dr. T. A. Welton and others at Oak Ridge instituted the first moderately large-scale effort on the use of digital computers to study the possibilities of such machines. This work played an important part in the design, construction, and operation of the Oak Ridge electron model cyclotron.[4] In the meantime there was the work of the MURA group which approached the subject through the back door, from the high energy and coming down! Their most important original contribution, from our point of view, was the introduction of the concept of spiralling and the detailed development of the consequences of that idea.[5]

Then at some time during this period people began to think about the possibility of having a variable energy cyclotron, by raising or lowering the magnetic field as the experimenter might require and changing the frequency appropriately. One of the early machines of this sort is the cyclotron at the Livemore site of the Lawrence Radiation Laboratory, and there are others.

The first realization of some benefit from sector focusing to be achieved on a proton cyclotron was at Los Alamos, and we will hear about that, later in this meeting, from Dr. Keith Boyer.

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(1) L. H. Thomas, Phys. Rev. 54, 580-598 (1938).
(2) L. Kelly, et al., Rev. Sci. Inst. 27, 493-503 (1956).
(3) E. M. Moroz and M. S. Rabinovich, Proceedings of CERN Symposium, 1, 547-551, Geneva, 1956.
(4) H. G. Blosser et al., Rev. Sci. Inst. 29, 819-831 (1958).
(5) K. R. Symon et al., Phys. Rev. 103, 1837-1859 (1956).

At this point in the history, as I have tried to outline it, many people began to do many things. It seems fruitless to try to enumerate them further, since most of these people are here and will contribute in the various sessions.

Now I will give an outline of the problems that are to be tackled by orbit calculations; the purpose here is to tell the non-experts (that is, the non-orbit-calculation-experts) what this session is supposed to be about.

The basic difficulty with a cyclotron is, of course, that the magnetic field should decrease with increasing radius to provide focusing, but should increase with radius to compensate for the relativistic mass increase of the particles during acceleration. As L. H. Thomas first showed, this dilemma can be resolved by making the field have azimuthal variations, which provide additional focusing to compensate for the defocusing that would otherwise be produced by a rising average field designed to cope with the increase of mass. We now know that these azimuthal variations may be sinusoidal or square wave; they may be radial or they may spiral. In any case, one must calculate the manner in which they should be shaped to do the job for the particles, the final energy, and the general configuration one has in mind.

The first general problem to be faced is the choice of field shape representation. Does one begin by thinking of his field as a plot with lines on it, dividing high fields from low fields, with the boundaries of these regions being given by formulas? Or does he set up some type of analytic formula containing a lot of parameters to specify a class of magnetic fields? It is sometimes rather difficult to compare the results of different workers because they start from different representations of the field.

Then one must calculate the closed orbits of particles circulating in such a magnetic field in the absence of the r-f voltages. Having found the closed orbits in some manner or other, it is necessary to adjust the field until all of these orbits have the same period so that the particles will not get out of phase. This requirement is referred to as the condition of isochronism or the resonance condition, and if it is not satisfied, one has phase slip, which means that the particles get ahead or behind in phase with respect to the r-f voltage.

Next there is the most simple-minded kind of stability analysis for these orbits, referred to as a linearized stability analysis; the things one tries to calculate are the frequencies of the radial and axial (or vertical) betatron oscillations, measured in units of the circulation frequency. There is not too much unanimity about the terminology for these quantities. (Most of us in this country call them ν_r and ν_z or ν_x and ν_y, but in Europe the letter Q is used, and now MURA has made these numbers more euphonious by referring to them as the "tunes.") The process consists of linearizing the equations of motion in small displacements relative to the closed orbits, and determining the betatron oscillation frequencies from the linearized equations. One next examines the question whether these numbers are integral or half-integral for any of the closed orbits being studied. Such values would lead to resonance difficulties even in the linear approximation. Then, there is the possibility that nonlinear resonance effects may occur. The nonlinear resonances are somewhat subtle and sophisticated phenomena to calculate and to understand in detail; however, some of them are important, and many of our speakers will be discussing them. One of the quantities of interest in some cases is a stability limit. If the amplitudes of oscillation do not exceed this limit, the orbits will be stable, while if they are outside this limit the orbits will be unstable, and it then becomes important to calculate the rate of growth of amplitude. At the very least, these nonlinear resonances are still "bogey-men" for us all, and at their worst they may present serious problems.

Then there is the special set of problems connected with the central region, having to do with the trajectories of particles as they emerge from the ion source and undergo their first few turns in the region where the r-f field is providing phase bunching and in which the electric focusing is large with respect to the magnetic focusing, or at least comparable to it.

In addition to this kind of problem there are also what one might call the dynamic effects, when we continue to include the r-f voltage in our thinking on out to larger radii. One might here consider particularly the effect of passing through a resonance, rather than a static analysis of how particles will behave if they stay at a constant energy in the neighborhood of resonance. Estimates of phase slip in the presence of departures from isochronism provide an additional example of this type of problem.

Then there are the special problems of beam extraction, and finally the question of tolerances. How good a job must one do in meeting various criteria in construction and in operation in order that the machine will do what it should?

From this variety of topics, and the even greater variety in the sessions to follow, it is clear that the speakers at this session face a difficulty (and I believe it is a serious one). There is much diversity in our specialization and our interests, and it will be necessary for the speakers to use good judgment as to how much detail to include in their talks. I hope that the speakers, particularly those who are discussing quite sophisticated and technical subjects, will aim their formal remarks at persons who are halfway down from their level of competence in their subject, and will confine the subtler technical points to private discussion or to the discussion periods. It is not fair, for example, to assume that we all know what a nonlinear subharmonic resonance is, what a tune is, what a Runge-Kutta step is, or what a phase plot is. Some of us have been living with these things for years, while others are meeting these terms for the first time. With this admonition we will now proceed with the program of this session. It is particularly appropriate that our first speaker should be Prof. J. R. Richardson, since he has probably had as long a continuous experience with cyclotrons as anyone attending this conference.