## General Discussion A

CHAIRMAN JUDD: We will now take time for a general discussion of the subjects presented thus far, after which we will have a brief intermission.

BLOSSER: I have a question which I want to address to Dr. Richardson. First of all, the formula you give for the $\nu_z$ is quite similar to the normal smooth approximation formula of Symon and company in appearance. Do the two formulas turn out to be the same in practical application?

RICHARDSON: They differ somewhat at the large radii. Our equation becomes more accurate as the field approaches a step function, that is, it would be quite precise for a step function. At large radii, F, which is unity for the step function and 0.5 for a sinusoidal function, in our design is 0.9; so, we feel it is more accurate to use an equation such as I gave.

BLOSSER: Even if you use the right flutter?

RICHARDSON: If you wish to put in a careful analysis, then I am sure that they will turn out to be essentially the same; but I meant that for ease of calculation I prefer this because it is easier to apply in the region where you are more concerned about the precise balancing of these two terms. I am sure that, if they are both applied properly, they will both give essentially the same results.

BLOSSER: This leads me then to the second question. I am surprised that you consider these formulas to be sufficiently accurate in this 50 to 100 Mev energy range; I am wondering if any computer comparisons have been made to verify this?

RICHARDSON: We have made no computer comparisons.

BLOSSER: On what do you base the statement of accuracy?

RICHARDSON: That is an interesting question [laughter] and I wish I could give you an answer. Perhaps it can be brought out in this meeting. But analyses have been made at Harwell (and I believe also at Oak Ridge) in which the difference between the computational method and the analytical method was smaller than 10%.

BLOSSER: I think the Oak Ridge work shows several examples where the Symon formula isn't actually good enough for $\nu_z$ in this energy range.

CHAIRMAN JUDD: I would like to comment myself that I took the trouble to examine Dr. Richardson's formulas to see analytically how they reduced to the smooth approximation. To make that reduction it is necessary to assume both that the number of sectors approaches infinity and that the flutter approaches zero. In those limits the formulas become identical from the analytical point of view; so it is quite possible that for a moderately squarish wave and for a small number of sectors there would be a considerably smaller difference between digital computer work and Richardson's formulas than between digital computer work and the simple form of the smooth approximation without correction terms. I suspect that the question of the precise amount of disagreement will have to be settled by computers, since no one can guess the third significant figures.

RICHARDSON: The only point I was making was that in this energy region these two terms do not have to be accurately known because it is enough to get sufficient focusing to overbalance any possible discrepancy.

CHAIRMAN JUDD: I would like to add a point about the cost of providing a larger magnet gap. Even in the Oak Ridge cyclotron, with its great conservatism (and we all have our bets "hedged," with space for extra coils and a lot of space for beam and dee structure) the magnet cost is still only about 20% of the cost of the whole construction. The operating costs every year are the cost of the magnet. The design costs of the magnet are really comparable to the whole cost of the steel and copper, etc. In 1933, when everybody did all the work himself and could not get a bit of outside money, it was important to try to be clever with cutting corners; but perhaps today one ought to think twice about it.

LIND: I would like to speak for the advantages, at least with a small machine, of going to a lower field and making a bigger magnet. I think the cost of the magnet is much less than the difficulties of design and the other difficulties that you run into in trying to push the field too high.

BOYER: I would like to make two comments. One is somewhat in the other direction, particularly on this comment of making the magnet gap large. So far as we have been able to determine, the gap that you actually use is no more than a fraction of an inch. I would be very much inclined to design for half the functional gap in the future. The other comment is that by using poleface windings one can certainly reduce this discrepancy between the central gap and the edge gap and, thereby, avoid a lot of worries about focusing in the central region.

COHEN: There would at least be space for the poleface winding.

BOYER: The need for this is much less than for the other space.

GORDON: I want to make a comment on Dr. Richardson's worries about the field at the very center. I think you will find that in cases such as his, where the gap is narrow and the flutter rises rather rapidly, the average field appropriate for isochronism actually falls off at the beginning. Furthermore, I believe you will find that you do not have the difficulty of having to start with the $\nu_r < 1$ and following through $\nu_r = 1$, because again with the rapidly rising flutter there are important contributions arising from the flutter gradient, which will over-compensate the slight falloff of the average field at the center.

CHAIRMAN JUDD: Isn't your point that you have such a small gap that the flutter rises fairly fast and you have so few turns in this region that no one cares what happens in great detail?

GORDON: I still don't think the problem exists because of the fact that the field should fall off.

RICHARDSON: Actually, the second curve on my slide did show the central field. It was quite flat. It did not actually fall off in this case. I think where N = 3 it would fall off, but for N = 4 it did not fall off; it was just sort of flat. It does make the problem much simpler. I would also like to comment on this question of high field versus low field, something which I did not mention; that is, we started this program with an already existing magnet, a 45-ton magnet with a pole diameter of about 45 in., and we wished to get as much energy as we could. I think that if we get 50 Mev, we will be doing pretty well. I would also mention that the total cost of the program--I mean the budget cost of the actual equipment is $60,000.

CHAIRMAN JUDD: We cannot get into a debate on accounting procedures here. [Laughter]

SNOWDEN: I have one comment about a report by Lawson on the gap, Harwell report A/R 2557. Lawson has arrived at a good working rule in deciding on the gap to use; he has it that the gap times the number of ridges dived by $R_\infty$ (that is the point at which the radius will become infinity) should be less than or equal to 0.3. In other words, if you use a large gap then you are going to run into a lot of trouble. Taking up Dr. Cohen's point on the gap size then, if you are only putting up the gap because you are using high dee voltage, then you are presumably going to lose out on the phase slip. So you may very well finish up with a better design with low dee voltage and a small gap.

CHAIRMAN JUDD: Is it clear how to optimize these two conflicting considerations?

SNOWDEN: Yes, the phase slip increases very rapidly.

CHAIRMAN JUDD: What is the basis of the gap determination?

SNOWDEN: If you are going to have marginal current and you have a magnet design, you have to have some compromise by making the field fall off. The phase slip introduced by that is a minimum.

COHEN: Working with a three-sector machine, we don't have any phase slip.

CHAIRMAN JUDD: It isn't clear to me that this always argues for the smaller gap consideration mentioned.

STAHELIN: There is a different approach; that is, one can try to compute the maximum beam for a given total magnet gap and for a given available dee voltage and a given spark gap. It turns out that one gets the maximum beam if the dee height is approximately the same as the height of the spiral ridges. If one tries to provide more focusing that means thicker ridges against higher beams, and so the optimum lies where the spiral ridge height and the dee gap is about the same.

SCHMIDT: I wish to ask Dr. Richardson one question. Why can't you design for essentially zero threshold dee voltage.

RICHARDSON: Well, you can design for zero threshold [laughter], but I'll guarantee you cannot achieve it.

SCHMIDT: Where do you lose out?

RICHARDSON: Well, the question is how much time are you willing to spend on tailoring your magnetic field? We felt that something like 0.25% on the mean field was something one could get quite easily and then from there on one would use coils. Then the question was how many coils would you need to be able to tailor it the rest of the way. Incidentally, from experience I am sure the best way to tune up the cyclotron is to have at your disposal a series of concentric coils and to work the beam out from the center just by satisfying the resonance condition. It turns out then for our case 8 coils are adequate, and we can work the beam out, we hope, in an orderly manner.

SCHMIDT: I wish to make a general comment, which may show my lack of understanding a little. It seems to me that we worry about the middle of the cyclotrons; yet this problem is completely solved for the reason that there are a great many conventional cyclotrons, some of which work quite well.

CHAIRMAN JUDD: I believe the question is, "What is the middle?" As soon as you have advanced in radius to where you have an appreciable flutter, then the machine no longer behaves like a conventional cyclotron. This happens at a radius of the order of one gap height, which varies from a radius of 3 to 10 in., perhaps. We are discussing machines that are several times that big; so already when you are one-third of the way out you are well out of the conventional cyclotron region from the point of view of the focusing forces. So I think it is a quantitative question how much of the center of the machine you choose to regard as an ordinary cyclotron.

SCHMIDT: On the other hand, you can tolerate a large phase slip certainly in the middle.

COHEN: The answer is that in an ordinary cyclotron the particles are just ready to slip all the way out of phase when they get to the outside radius. In other words, you have used all the phase shifting you can. This is no condition to inject into the rest of the cyclotron. So what you say is not really true. You cannot have an ordinary cyclotron essentially injecting into a spiral-ridge cyclotron because the available phase slip at the outside of the ordinary cyclotron is gone.

SCHMIDT: But there is a great deal of beam at that phase.

COHEN: But at just another inch of radius, you would have nothing.

GREEN: I might be anticipating the analytical people but there is one concept which is extremely useful for this kind of discussion, and that is the concept of admittance. An accelerator of any sort will admit a certain area of particles in phase space. This applies to a synchrotron. It is a concept which is quite vital in a synchrotron because the admittance area may be extremely small; if you don't hit it, you get nothing. But it is also quite important in a cyclotron because one begins with a certain area in phase space. This area can be shrunk during acceleration inversely proportional to the root of momentum with some small corrections, and it governs the quantity of particles from an ion source. Now the final goodness of the beam which might emerge from this cyclotron is dependent on this admittance. If the ion source will emit a high density in phase space which can be accepted by the cyclotron, then a small emittance will be found at the output of the cyclotron which makes the beam much more usable than a large fire hose spray.

The intermediate consideration is that during the process of acceleration one has admitted a certain region in phase space. It is necessary that the intermediate region of the cyclotron simply not clip the existing phase space; that is, the dee gap or whatever the restriction is vertically and radially must not impinge on the phase space configuration of the beam which may grow and shrink.

Then, the second consideration is that there is essentially nothing one can do about the area in the phase space which is reduced by the acceleration. It is possible to distort this area in the phase space so that the emerging beam again will occupy a ragged, more irregular phase region and be sprayed and smeared all over the place. So again in the central region of the cyclotron, it is important that one does not take a nice ellipse and wind it up into a long curly worm due to non-linear turns which cause disturbances for large amplitudes. These considerations are not too important if one is merely striking a target which is to be pulled out through the wall and given to a chemist; but for any experiment in which one wants a well defined beam, so as to be able to do rather nice physics, it should be possible to manipulate the emerging beam from the machine with respect to the ratio of size and divergence. One now has nice lens systems for these manipulations. But the product of size and divergence emerging from the cyclotron is what you have to work with, and you cannot do anything about it unless you accelerate to a higher energy.