Factors Affecting Beam Quality

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At Oak Ridge our interest in this type of cyclotron started with a higher energy cyclotron which was to serve essentially the same function as a linear accelerator for injection into a large synchrotron. This notion of using a cyclotron as an injector aroused so much merriment on the part of the average synchrotron man that more or less in self-defense I found that I had to acquire some sort of understanding of what the factors were in an ordinary cyclotron that makes this idea ridiculous.

I felt that if I could achieve some sort of understanding of this then it would be a simple matter for my engineering friends--assuming that they are on the ball--to figure out what had to be done to make the thing work. I am not sure about the second part of this, but for the first part I will try to give you the story, so far as I know it.

The first point I won't elaborate on very much. I think probably it is obvious to those of you who have thought about it. The quality of the cyclotron beam is not bad at the beginning; it just gets bad during acceleration. Possibly you can make a more efficient ion source than a cyclotron ion source from the point of view of the phase density, but a little thought convinced me, and I will take it for granted as being fairly obvious, that there is nothing inherently diffuse about the phase distribution put out by a cyclotron source. The problem is to keep the phase figures which define the radial and axial phase-space distributions from becoming badly twisted up, as Ken Green was remarking yesterday. In fact, he gave part of my talk for me.

It is inevitable that during the very first part of the acceleration, that is, during the time the ions are coming out at essentially zero velocity from the surface of whatever it is they come out of--the surface of the arc--during the time they are accelerating in their passage through the first accelerating electrode, it is quite clear that space charge forces must be important. This is true with any type of acceleration. There is no getting around it. Also it is a very complicated region to calculate, and cyclotron geometry is not suited to calculation anyway.

To get around this difficulty I have adopted the following policy. I decide on an aperture of reasonable radial and axial size through which all the orbits can be extrapolated backward, and then go forward a turn or so before any detailed calculations are attempted. A current is assumed and we can then define a size of radial and axial phase space as well as a phase density which we wish the machine to process in some way. The definiation of these phase regions must be completed by an assumption as to angular distribution. A simple and conservative assumption is that all phases of oscillation are equally likely, with a maximum amplitude as set by the extent to the aperture.

I think that the following numbers are not unrealistic. I will assume that all of the orbits can be extrapolated back into an aperture, 2 cm axially and 0.4 cm radially, and let me assume that the total number of orbits which go back through it are equivalent to a time-average current of a milliampere. That is the rough range in which it seems reasonable to work.

I want to make some numerical estimates and I need some numbers to do it with. It may be asked why I wish to assume a beam of such large axial extent. The answer

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is that the beam does not shrink very markedly during acceleration, or at least it is advisable for it not to be allowed to shrink, and we wish at some point to extract the beam and put it into a synchrotron. One has generally some trouble otherwise filling the vertical aperture of the synchrotron. These considerations, of course, would not necessarily hold for the use of a beam for nuclear physics, and one might want to make the beam more nearly square, as I believe Dr. Boyer would like to do. However, I have found no reason why one cannot accelerate a beam of large axial extent, and the higher it is the more current one gets.

There are a number of effects here that are troublesome. I cannot calculate any of them with high accuracy, but all of them can be calculated well enough to see what is happening without much trouble. Let me first indicate the importance of the ordinary transverse space-charge repulsion in the cyclotron. Under the conditions I have indicated, we assume a pulse in the cyclotron, extracted from the source and out coasting around between accelerations. What I would like to do is to estimate the effect of the transverse repulsion. I am not going to give details because I don't have the time, but it is a simple procedure. The experts know that these estimates are easy to make, although a real calculation is impossible.

For this estimate, we write the radial equation of motion, in this form:

$$\frac{d^2x}{d\theta^2} + x = \lambda x \tag{1}$$

where x is the radial deviation from the central ray. The term x is essentially the focusing from the centrifugal force, and the term λx represents the transverse electrostatic repulsion of the sausage of charge.

I will assume that the particles get just the dee-to-ground voltage in the first kick, and I will ignore the complications of what happens during the acceleration and calculate what the space-charge repulsion (in terms of the coefficient λ) will be on the first half-turn after the initial acceleration. The answer is for the conditions I gave and for an energy gain per turn of 100 kv (25 kv dee-to-ground):

$$\lambda = \frac{5 \times 10^{-3}\ I}{\phi h w \sqrt{\delta E}} \tag{2}$$

The current, I, is in milliamperes, 2ϕ is the phase angle in radians subtended by the sausage; w is the radial extent and h the axial extent, in cm; and δE is the energy gain in Mev/turn. For the conditions previously described, and taking $2\phi = 30^{\circ}$, λ turns out to be about 0.06.

This means that the focus is deferred a little past 180° (but not very much), and after one more acceleration this effect is correspondingly smaller. The general conclusion, which I am confident of, is that the ordinary simple space-charge effect, the transverse repulsion, is unimportant in the formation of a cyclotron beam and in its behavior throughout the cyclotron.

Having arrived at this point, there seemed to be no further problem until it occurred to me that another space-charge effect, namely the longitudinal space-charge repulsion, is relatively unimportant in synchrocyclotrons and in synchrotrons only because of phase stability. One thing about a f-f cyclotron which is directly connected with its high intensity is the fact that it has <u>no</u> phase stability. There is a question, therefore, as to what happens to the longitudinal space-charge effect in the cyclotron. By this I mean the tendency of a sausage of charge to push itself apart in the azimuthal direction.

At first sight it might be thought that charge would lose its azimuthal bunching, in which case acceleration would cease. We know that this does not happen and hence it can't be important. There is a theorem (unfortunately not so well known as it might be) that the electric field of the machine cannot affect the orbital frequency. The orbital frequency is further assumed independent of energy. There are some extra complications if isochronism does not obtain, but all designers are planning to achieve a high degree of isochronism. With orbital frequency independent of energy, the leading particles of a pulse will gain energy more rapidly than the trailing particles, and they will gain radius more rapidly. The pulses will, therefore, not be extended azimuthally for long. They will begin to "tip" more and more extremely as they work out through the machine. Clearly, this is an effect that will not seriously interfere with the operation as it is usually observed in an ordinary cyclotron. However, it is an effect which I believe to be crucial in the question of how one should go about forming a cyclotron beam suitable for extraction and injection.

Let me give you an indication of the effective voltage discrepancy between leading and trailing particles. The question of interest is the comparison of this voltage with the r-f voltage to find the rate of "tipping."

I don't have a very good general formula, the derivation being elementary but messy. Two limits are fairly easy to estimate. Assume that the pulses are merged into a radially continuous distribution of charge; this is not bad after a few turns, and it quickly gets to be very good. In one simple case the axial extent is small compared with the length of the pulse; in the other the axial extent is large compared with the length of the pulse. Image forces from the conducting surfaces above and below the beam will reduce the effect, but probably not by an order of magnitude, and they have been ignored.

What I did first was apply this to the first half-revolution. Under those conditions I found that δV, with my standard conditions, is 84 volts per turn difference between front and back. This is not at all serious at first sight, since the total gain per turn is over 1,000 times larger. It should be examined a little more closely, but I believe this is not bad, and if this were maintained throughout the machine, there would not be much worry.

Unfortunately, when we go to the other case of axial extent small compared with the pulse length, at the same time the pulses are coming closer and closer together as the radius gain per turn becomes smaller and the charge density is therefore building up somewhat. I worked out the result for $\beta = 0.3$ (which is around 50 Mev) for protons on a machine with 10,000 gauss central field and beam dimensions as previously assumed. Under these conditions δV turned out to be roughly 2 kilovolts. This is on the assumption that the voltage gain per turn maximum is about 100 Kv, and so in this case we have about 1/50th of the voltage as the size of the discrepancy from front to back.

Let's ask what happens to a pulse as it works its way out of the machine. I am not going to worry about the question of axial focusing. In the big machine it turns out that you can get magnetic focusing essentially from the source outward. In a little machine, such as we are talking about here, I think it is clear enough that some ingenious designer will find a way to put in grids to get electrostatic axial focusing. At any rate, I am not going to worry about axial focusing, even though it is important that such focusing be present. It is the question of extracting the beam that we are concerned with, and for this purpose one must have (as I think the previous speaker and others have emphasized) a fairly small range of orbit centers for this to be possible efficiently. It is, therefore, essentially the radial behavior of the pulse that I am worried about.

If we look at a particular pulse coming out, we can, for simplicity, draw an orbit circle as a straight line and the radial lines which bound the pulses at a given instant can be drawn as parallel lines. There is a constant phase subtended, and the pulse remains between the two parallel lines. A pulse starts with radius independent of azimuth (Pulse 1 in Fig. 189). If we can in some way insure that the pulses end up still looking like this (Pulse 2), then in principle it should be possible to arrange an extraction mechanism into which these pulses can enter without loss. This is on the assumption that the extraction mechanism involves a stationary septum. Even though we do not have turn separation in the usual sense, under conditions which will be explained, we can hope in principle to get the turns separated by building up radial amplitudes in such a way that one turn will pass just inside the septum and the next turn will pass just outside the septum and be extracted.

The conditions for this to be possible so far are quite simple in principle. I should say incidentally that they are certainly not completely original with me; H. Snyder, for example, thought this through fairly carefully for himself some years ago. Let's suppose we have an acceleration of 0.5 Mev-turn. If we look at the particles that have made 100 turns, it is very unlikely that these turns will be separate at 50 Mev, as you well know. They are, however, separable in a useful sense under the following conditions. If we look at the energy spectrum and at the spectrum of particles that have made 101 turns, the distributions may overlap (Fig. 190a) or they may be well separated as in Figure 190b. If these two energy spectra are well separated, then a simple argument convinces me (and I hope it will convince you) that although the beam may not seem to be composed of separated turns at this radius, it is still composed of particles that can be sorted into turns by a mechanism that is effectively energy-sensitive, like a regenerative bump to build up radial orbit amplitudes.

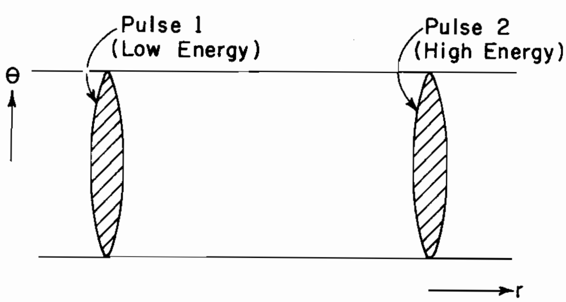

Fig. 189. Pulse preserved. (Idealized).

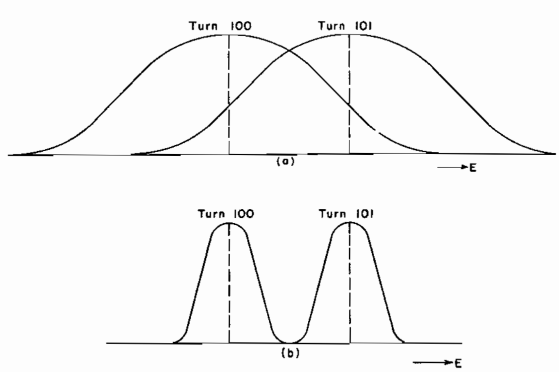

Fig. 190. Effect of energy spectrum on turn separation.

It is this type of spectrum which we have to maintain. Such a spectrum is insured if the pulses retain the form shown (Fig. 189); in other words, if we

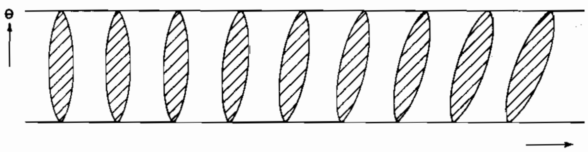

Fig. 191. Effect of longitudinal space charge repulsion.

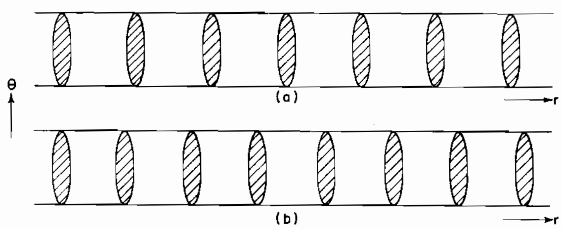

Fig. 192. Effect of flucuating r-f voltage, (a) at peak voltage, (b) at lower r-f voltage.

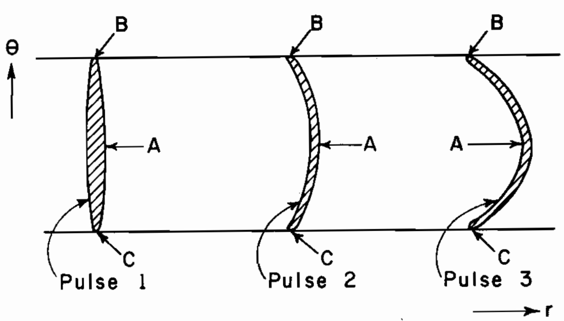

Fig. 193. Effect of voltage variation with phase.

can somehow arrange for all the particles coming out from the source in one pulse to remain together radially to a sufficient accuracy. An energy (radius) spread, of about 10% of the energy gain per turn, is probably adequate.

The difficulties are three-fold. One is the space-charge difficulty for which we gave some numbers. This will cause the pulses to tip progressively as they go along (Fig. 191). It is clear that this will spoil the efficiency of the extraction, providing the tipping is such as to allow no radial gap between pulses.

Another effect which is just as bad as the "flickering" of successive pulses which occurs because of the fact that the r-f voltage is changing with time. At a given instant the train of pulses may appear as in Figure 192a. A little later the r-f voltage has shifted slightly and the train of pulses will appear as in Figure 192b. In one instance we will be getting good extraction while an instant later we will not; this "jitter" is just as destructive as the tipping of the pulses which comes from the space charge.

The third important effect is this. Let's suppose that these particles A (Fig. 193) pass through the acceleration gap at the peak of the voltage wave. Particles B and particles C must pass through with less voltage. This relation is preserved throughout the history of the pulse (pulses 1, 2, 3). This means that the center of each pulse will bulge outward with respect to the edges. Therefore, as they come out, the pulses will have acquired a shape like Pulse 3. Again, this is clearly destructive of efficient extraction.

These three troubles have to be solved by some trick. The trick is actually not very complicated, but there is, as yet, no real test of its effectiveness. Clearly we do not want to have too many turns in the machine. The more turns there are the more vulnerable the pulses will be to these deformations. This is one solid argument for anybody who says that one of the great virtues of the Thomas machine is the reduction of threshold voltage. If one tries to reduce the voltage, I do not think the optical quality of the beam can be maintained. The internal beam, of course, will not necessarily be hurt badly in this way. If we have a large number of turns, then a very small flickering of the voltage on the dees can cause a serious displacement of the turns back and forth. As one increases the number of turns, keeping the phase angle subtended the same, then the "bulging" effect has more time to build up, so that for a given subtended phase, increasing the dee voltage will straighten out the pulses. Similarly with the space charge effect, the amount of tipping is clearly determined by the ratio of the voltage discrepancy between front and back (arising from the space charge) to the total voltage, and to the number of turns. It is also unfortunate that as the voltage is decreased, the charge density gets larger and there is an additional adverse effect. The space-charge effect can be compensated by shifting the phase of the pulse with respect to the r-f wave so that the front of the pulse goes through the gap at a lower voltage than the rear of the pulse. This can be done by suitably adjusting the r-f and the isochronous condition. It is a very complicated business to calculate and I think it will have to be done empirically, but provision should be made for it.

If the acceleration is to be accomplished in 100 turns, the voltage must be controlled perhaps to 1 part in a thousand. If the range of phase used is restricted so that cos ϕ, which is essentially $1-(\phi^2/2)$, does not deviate by more than about 1 part in a thousand from the peak value, then a total phase angle of probably not much more than 10^o can be used.

I think I have given the three necessary conditions. It should be possible to obtain these conditions and we can then expect to be able to achieve extremely good extraction with a very high quality beam. It may be worth adding that these problems become so severe in a 1,000-turn large machine as to make very attractive the introduction of a third-harmonic accelerating voltage.

SCHMIDT: Your numbers are off very much.

WELTON: The machine I used is idealized for illustrative purposes. For example, I used a high current, about 1 ma. In fact, it is very sensitive to current.

BOYER: I wonder if you have looked into what effect the regenerative system might have in itoning out this tipping. It certainly will have some effect.

WELTON: I have thought about it. Essentially there will be some particles capable of extraction; others that are not. The percentage that are capable of extraction, I am afraid, is influenced by the tipping that has occurred up to the time they have entered the extraction range. I think the answer to your question is that the regenerator cannot help.

SYMON: It seems to me that if you have an extraction mechanism that will select a certain energy and then extract it, then the only thing you gain by being able to separate the pulses is that you don't lose anything.

WELTON: That is correct.

SYMON: So in principle even if they were not separated, you might get a good quality beam.

WELTON: I should have said on the question of beam quality that the extractor acts as a very fine aperturing system. The extracting beam can easily be of very high quality. One unfortunately needs intensity also.

GORDON: I might add to Welton's conditions this maintaining of isochronism to a high degree of precision. I think you can see quite readily that if the particles slip off the peak of the r-f wave, then the difference in the front and back voltage again becomes quite aggravated.

PETERSON: You said that at the small radius the successive turns will overlap in space, but you said they do not necessarily overlap in energy. If they are all at the center of the machine, these two conditions are competitive, aren't they?

WELTON: No, I think not. I don't think there is any inconsistency.

PETERSON: That means the centers are not all at the center of the machine.

WELTON: Yes. A finite range of betatron amplitude is always assumed.

TENG: I would like to continue Dr. Symon's argument. Suppose you have finally made the beam separation between turns, let's say, 10 times the thickness of the septum; then, theoretically you get 90% of the beam. Then it is not necessary that you have the turns separated.

WELTON: No, not at all.

TENG: Also I think it does monochromatize the energy of the beam. In other words, if you have tilted pulses coming along, it will chop them off at a certain energy and make the tail of the pulse wait until it gets accelerated to that energy and then take it out.

WELTON: I would rather argue that privately with you, Lee. A minimum statement would be that the use of programmed turns, as described, makes it possible in principle to retain the original phase density and use all the intensity. Other methods may allow this, but each must be considered on its merits.