ROUND TABLE DISCUSSION ON SPACE CHARGE AND RELATED EFFECTS

Participants:		
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	K. R. Crandall	Los Alamos Scientific Laboratory
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	T. Nishikawa	University of Tokyo
	J. Haimson	Massachusetts Institute of Technology
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L. SMITH: In the process of organizing this conference, the program committee recognized two important areas in which various workers have reached various conclusions starting from various assumptions, namely, in beam dynamics calculations and in properties of rf structures. The decision was made to try to clarify the situation by use of a round table format in place of the usual succession of seemingly unrelated papers. Rf structures will be discussed tomorrow; today we will deal with theoretical work on phenomena which determine emittance and energy spread of linac beams. Space charge effects will probably dominate the discussion, but questions of aberrations, alignment, and tank flatness, for example, are certainly part of the picture.

We will proceed as follows - first, each person at the table will speak for a few minutes in order to introduce himself and explain his particular interest in this subject. There will follow a discussion among the panel members, after which the audience will be invited to participate with questions or comments.

I call on Lapostolle for his opening remarks.

LAPOSTOLLE: Experimental work at CERN on this question of space charge effects was started a few years ago. The results were reported by C. Taylor at the Los Alamos conference but the work has been extended since. The main results I can summarize in the following way: first, if one injects a flat ribbon beam into the linac at low intensity, one also gets a somewhat flat beam at the output. If the intensity is increased to say 40 milliamps, the beam becomes progressively round at the output. Second, when trying to optimize the machine in order to minimize the emittance at the output, whatever you do, even if you inject a smaller beam at the input, or whatever you change, you cannot go below a certain emittance at the output. Of course, with bad adjustments the emittance may go up to large values. This is completely in agreement with what Rena Chasman[1] explained at the beginning of this session. This minimum emittance is a function of intensity. The measurements indicate a dependance on intensity to the power of 1/3 or 1/2.

Another point: if one looks at the longitudinal emittance, or rather, energy spread, one finds that there is a close relation between the change in energy spread and the change in transverse emittance when the intensity is increased. This led me to try to think in terms of a kind of thermodynamical model; this has no physical sense, it is just a model. I introduce three temperatures corresponding to the transverse and longitudinal coordinates of motions and assume that there is an exchange of heat between them through the medium of space charge. I don't know how; that's just the model. If I am doing that, I can explain the rounding of the beam, the kind of bell shape or gaussian distribution one always measures, and the fact that the energy spread changes in the same way as the transverse emittances. If I go a bit further and recall that in thermodynamics one introduces the concept of entropy, I must remember that entropy is constant only as long as heat transfers are made at equal temperatures and are therefore reversible. Constant entropy is analogous to constant emittance; that is Liouville's Theorem. But in usual thermodynamical processes heat transfers may also occur at unequal temperatures; then entropy goes up. I may wonder whether something similar may not occur with the beam. When the beam is wriggling, due to strong focusing forces, transverse temperatures are oscillating. If I then assume that there is a slight heat exchange between them, (that would be a nonconstant entropy phenomenon) the emittance would blow up. One can then explain most of the results and even another one, which has been observed on the CERN linac. There we use ++-- focusing, which entails a rather big wriggling factor. We tried to use +-+- which gives a smaller wriggling factor. (This, however, cannot be done over an extended period of time because it requires excessively high quadrupole gradients.) To our surprise we found that the transverse emittances were reduced and the energy spread, too. Emittance and energy spread values were lower than we had ever obtained before, for the same intensity.

NISHIKAWA: I think the analysis of space charge depends on the model, especially on the assumed charge distribution in the bunches and it is necessary to get a self-consistent method for this model. If we use the self-consistent method, then the results will not be so dependent on the first assumption of the charge distribution. One of the most promising ways to get the self-consistent space charge effect is to start from the reduced Boltzmann equation, just as has been done by Nielsen and Sessler for the circular machines. There is some difficulty in extending the Nielsen-Sessler method to the six dimensional phase space analysis. However, if we assume some simple distribution in the stationary state, then we shall get boundary equations for the six dimensional phase space. If we take the self-consistent potential for the assumed distribution, then we can estimate the space charge effects on the boundary motion in stationary states. Then for the non-stationary states, I think, the perturbation analysis will be useful. Assuming the charge distribution and the external restoring forces as the summation of the stationary values and the time-dependent perturbed values, we then get the effects of the perturbations on the boundary motion. In this way we find some resonant effects which would occur, particularly when the plasma frequencies, of protons in the bunches, approach the betatron oscillation frequencies. Such conditions will be satisfied for about a 100 mA beam at low energies. Any perturbation such as: longitudinal-transverse coupling, or transverse x-y coupling will grow during the acceleration. It will be enhanced through a collective motion of the assembly of charge particles, compared with the case without space charge. Details of the analysis, I made along this line, have been submitted to these proceedings.[2]

L. SMITH: Since Mrs. Chasman presented her work earlier this afternoon, we can pass directly to Ken Crandall.

CRANDALL: About a year and a half ago I became involved in trying to compute the effects of space charge on our buncher system at Los Alamos. That is, we tried to take space charge into account in determining the optimum parameters for the system, and evolved a program called MRA which Dr. Lapostolle[3] referred to this morning. I won't try to describe it in detail but the basic idea is to assume circular symmetry and to simulate a section of the beam $\beta\lambda$ long, by many rings. The initial position of these rings are prescribed by specifying an initial charge distribution (normally a uniform charge density.) The rings are followed through the buncher and the lens to the linac. At each step the position of the rings give an approximation for the charge distribution. From this approximate charge distribution, the radial and longitudinal electric fields are computed at the mesh points of a two-dimensional lattice in r-z space. The force on each particle is obtained from the electric field. The program is fairly efficient. Using a CDC6600, roughly 2000 particles can be followed per second per step. We think that this procedure probably simulates our buncher system fairly well. This procedure has also been incorporated into PARMILLA by Don Swenson and Jim Stovall. PARMILLA is the beam dynamics program that follows the particles through the drift tube linac, and there, of course, we do not have circular symmetry, but still we feel that this procedure does give us an order of magnitude correction for space charge. More recently a different space charge routine has been included in PARMILLA which does particle-to-particle interactions and doesn't assume circular symmetry. The result of the two methods have been compared and they agree surprisingly well (except in the amount of computer time that they take).

L. SMITH: Dr. Ohnuma, who was scheduled to be a member of this panel, was unable to come to the conference; Dr. Haimson has volunteered to take his place.

HAIMSON: It feels a little awkward to be an "electron" amongst a group of "protons," but I think our space charge problems are similar. I would like to briefly discuss the problem of selecting an initial-bunch model, as close to actual conditions as possible, before embarking on a complex program. Early approaches to this problem can be found in high current electron beam analyses by workers in the klystron field such as Rowe, Tien, Webber, etc. The disc models used in their approach inspired much of the earlier work done on injection systems for electron linacs. In more recent years, however, phase orbit analyses using time domain techniques and three dimensional models, comprising a multiplicity of rings or annuli, have been adopted. This is especially important in the study of the more critical drift space region prior to acceleration where the particles are low in energy. Since we are not involved with many of the transverse forces, such as produced by the quadrupole focusing elements in proton linac injectors, we can expect that a reasonable semblance of rotational symmetry will be maintained and the problem is somewhat simpler.

The use of a multi-annular model not only allows radial variation to be considered, but the effects of non-linearities due to irregular bunch geometry and nonuniform charge distribution can be taken into account. Extensive beam measurements on a variety of injectors, using profilometer scanning and rf chopping techniques, have enabled the initial bunch conditions to be determined. For example, Figure 1 shows a bunch model we have chosen for assigning initial conditions to a multi-annular mesh relating to a biased-chopper prebuncher injection system. This spheroidal model is about as close as we could match to actual conditions, and the nonuniform radial and longitudinal charge distributions result in corresponding non-linear field distributions as shown on the Figure 2. It is interesting to compare them against the straight dashed lines of a uniform charge density spheroid. An important aspect of operating at high injection voltages, as discussed in yesterday's paper[4] on high duty cycle machines, is that with a moderate amount of chopping a relatively low prebuncher voltage can dominate the space charge forces and even with some non-linearity we have an opportunity to keep the bunch organized and prevent cross-overs prior to entering the accelerating structure.

In regard to the longitudinal direction, one might ask how do you measure rf bunch density. The Figure 3 shows the impression on a tungsten screen of a 120 kV, 3 ampere beam being rotated at S-band frequencies before rf chopping and the successive stages of increased chopping to produce a small bunch. I've reported previously[5] on the results on this work, but briefly stated, it is a method of displaying the longitudinal phase space of an rf bunch; and in combination with beam cross section measurements it gives us an opportunity at least to get an approximate check on the density distributions.

L. SMITH: The final opening presentation will be given by Prof. Gluckstern of the University of Massachusetts.

GLUCKSTERN: I should like to make a few remarks concerning thoughts people have had about coupling. Their calculations may have to be reinterpreted. If one neglects space charge and considers the transverse-longitudinal coupling due only to the rf field, then the transverse equation has the following character:

$$\frac{1}{\beta}\frac{d}{dS}\left(\beta\frac{dX}{dS}\right)+k_t^2 X=\mathcal{E}\left(\phi-\phi_s\right)X\ .$$

The most important coupling term is linear in X and is multiplied by the amplitude of the phase oscillation.

The interpretation of the effect of this coupling upon beam quality was made more or less along the following lines: In the properly normalized transverse phase space one starts with a circular region. In the absence of coupling, particles starting in this region will move in circles and the boundary will remain stationary, assuming the beam is properly matched. One then notes that the longitudinal motion causes a perturbation to this circular motion. This effect is strongest at injection since the coupling coefficient decreases as the velocity increases. The primary effect is a permanent distortion of the original circle in the transverse phase plane.

If one considers a fixed $\delta\phi$ and $\delta\gamma$ in the initial longitudinal motion, one finds that the points on the circular boundary distort to an ellipse. If one considers a starting point with the same amplitude but a different phase of the longitudinal motion, one again gets an elliptical distortion of the same amplitude but with a different orientation of the axis of the ellipse in the transverse phase plane.

The sum of these effects makes the beam appear as if the transverse area has grown. This is the basis for our estimates of the deterioration of beam quality, or growth in apparent area due to this coupling effect. It is important to see, however, that the density distribution in the central part of these overlapping ellipses has remained about the same and only the density distribution near the outer edge has changed. If one looks at the density distribution in some radial direction in transverse phase space, it starts out uniform, and to first order, the effect of the distortion is to produce a "ramp" type modification at the edge of the beam. The end of the ramp formed the basis for the estimate of amplitude increase.

Calculations of area by R. Chasman using the rms measure of the size of the beam showed that the growth was much smaller than expected because some particles move inward while others move out. To a first order, in any kind of rms calculation, these effects should cancel. The importance of this is that: If you take initially uniform distributions you will get apparent beam growth but if you consider distributions with "tails" then one must be sure to do the rms calculation in an appropriate way, taking into consideration just how the tails are cut off. One cannot use simple formulas. The actual observations are probably neither rms or border observations. One must decide how the tail is to be cut off. For example: one can consider the contour enclosing 90% of the beam. The implication is, however, that this effect is not as important as some others in contributing to growth in transverse amplitude.

The other remark I would like to make concerns the use of the Vladimirsky-Kapchinsky formalism in considering problems with beam envelopes, some of which people have discussed during the course of the day. One has the following equation for the transverse beam size:

$$\frac{1}{\beta^2\lambda^2}\frac{d^2a}{dn^2}=\frac{P^2}{a^3}-k_t^2\,a+\frac{\mu}{2ac}\left[1-f\left(\frac{c}{a}\right)\right]$$

One has a term due to the emittance, another due to the linear external focusing force, and perhaps a term due to the space charge. There is a similar equation for the longitudinal beam size. It is remarkable that matching can be done, even in an approximate way by making a" and c" equal to zero because this implies a number of untrue assumptions. The first is that the six dimensional treatment is self-consistent. One cannot have a real positive six dimensional distribution which leads to uniform charge in real space. The second difficulty is that the uniform densities themselves are not realized in practice. These densities do have tails. A third difficulty is that couplings due to space charge or rf longitudinal transverse coupling or x-y couplings in fringing fields of the quadrupoles are not included. It is remarkable that these effects do not seem important when one tries to match by making a" = c" = 0. One makes this assumption, obtains starting values of a and c, then checks by making computer calculations of envelope motions and finds little pulsation. This is remarkable and will probably continue to be exploited in matching.

One is faced with the following parameters: current, initial transverse wave number, longitudinal wave number, beam size in transverse direction, angles in transverse direction, initial phase spread, initial energy spread. There are too many parameters to explore in any consistent way. The fact that one seems to be able to match in the particle motion codes is very helpful in keeping the numerical analysis manageable.

L. SMITH: I have the impression that, at least up to a few months ago, there has been some disagreement between groups doing calculations of space charge effects, both as to results and as to a proper model. I would like to ask Crandall, Gluckstern, Chasman and possibly Lapostolle to clarify the situation.

CHASMAN: We made a few checks. We checked the program used at Brookhaven against the particle orbit code used at Los Alamos, in which ring interactions have been added. We got fairly good agreement.

CRANDALL: We made a similar check. When we put in a particle to particle interaction routine instead of the rings. I think we also agreed.

CHASMAN: I think the difference in the two checks was that: in the first, we compared different orbit codes and both the output routines and input distributions were slightly different. This test was repeated at Los Alamos with the same program, employing two alternative ways of employing space charge forces. Both calculations gave similar results.

L. SMITH: Dr. Lapostolle - has anything along these lines been done at Saclay?

LAPOSTOLLE: No not yet. Something of a similar nature is in preparation at CERN in common with Saclay by M. Martini and M. Promé, but it will be a long time before results are available. Slightly different models will be used, but the same principle will be employed.

L. SMITH: If I understand correctly, your results are in reasonable agreement and not sensitive to the space charge model used. What remains to be done? For example, are image forces important?

CHASMAN: I have not included any image forces in my program but I have made an estimate of their possible magnitude by assuming a uniform ellipsoid and using the formulas from a CERN report of Dr. Lapostolle. For a beam of approximately 0.6 cm radius compared with a bore radius of 1 cm, the forces are not more than 10 to 15% in the beginning of the linac and then decrease as the energy increases.

L. SMITH: Is this true in the case of intense electron beams?

HAIMSON: I am not sure what charge densities you are considering, but in electron devices the beam radius relative to the containing-pipe is usually not very critical until quite high currents are reached. This is an effect that klystron designers must take into account. There are two important aspects: first, the voltage depression occurring with these high current beams results in the outer electrons having a higher conversion of potential to kinetic energy at emergence from the gun anode than the axial particles. The outer portions of the beam tend to shear away from the kernel. Second, there is a reduction of the space charge longitudinal E field itself. We looked at these problems and reported on them in Washington last year.[6] The effect is fairly insignificant for the current levels considered in "physics" machines. For high pulse current machines it is quite significant since the E_z reductions (20 to 40 percent) that result from high ratios of beam to tube diameter can have a pronounced effect on the bunching behavior. I do not think any program we have used up to now provides, at very high currents, an adequate analysis of the spectrum of the beam at the end of the drift tube. Klystron people have also found it very difficult to develop an adequate "large signal" analysis technique. However, we feel that the 3 dimensional model is a substantial improvement over the old disk model because the so called "dishing" or "oil canning" effect of the disks, as well as E_r variations of the cavities, can be taken into account.

NISHIKAWA: Dr. Hirakawa[7] expanded the kernel function in a cylindrical beam duct in terms of series of Bessel functions. He assumed three types of charge distributions in an ellipsoidal bunch. They are:

1. Uniform charge density in the ellipsoid.
2. Statistical distribution.
3. Gaussian distribution.

He also assumed the same second moments around the axis for each of these three cases. Then he computed the space charge potential by means of the MESSYMESH method. The calculations were made for the case of 10^{10} electron charges in a bunch, which corresponds to a 300 mA beam in proton linacs. The typical results of equipotentials due to a uniform charged ellipsoid are shown in figures 4 and 5, where the equipotential lines in a beam duct (right half) are compared with those in the free space (left half). The electrostatic potentials on z-axis and r-axis, which correspond to three different charge distributions, are also shown in figures 6 and 7. As a result, he discovered that the effect of image charges is most noticeable in the axial direction.

L. SMITH: Any comments from panel?

CRANDALL: We can put image effects into our program without sacrificing speed, if we assume circular symmetry. We plan to do that in the near future.

L. SMITH: It seemed from the talks this morning that the subject of bunching, which takes place in the region before the accelerator where the space charge effects are very strong, is well in hand. But are the models realistic? Do we need further refinements in these computations and are there other aspects which have not been considered yet?

LAPOSTOLLE: These programs are quite good, apart from image effects, which have just been discussed. However, it is difficult to get a clear idea of the accuracy of the results, especially since the number of points computed is always limited. In the three dimensional program I described this morning, 500 points were used, which by symmetry represent 2000 points, but still only 500 are computed and it is difficult to approximate the appropriate input distribution: uniform, statistical or gaussian. It is difficult to know what to do. Present computations require only a few minutes of computer time, but greatly increasing the number of points would greatly increase this time.

L. SMITH: There is a practical point here. The results of these calculations depend strongly upon buncher locations and voltages used. Is it safe to say that bunchers can be designed on the basis of what has been calculated so far?

GLUCKSTERN: There is one point which has not yet been discussed at this meeting but must be taken into account. If one has a multiple buncher system, two bunchers for example, then the adjustable parameters are: the two buncher voltages, their phases relative to linac, the separation of the two bunchers, and the distance to the linac. Moreover, the beam current will be different at different times. It becomes necessary to decide which parameters are going to be fixed, and which will be adjustable by "knobs". Very likely the buncher positions will be fixed and voltages and phases made adjustable. How does one carry out a suitable design? For example: does one design for the largest current and then optimize the variable parameters for lower current, or does one design for some intermediate current? I don't think this aspect has been considered sufficiently.

LAPOSTOLLE: I agree completely, but to return to the quality of the computation, I feel that present computations, even if not completely accurate, are adequate and good enough to test if one system is better than another. What is missing rather is the guide line for optimization.

GLUCKSTERN: I think this guide line may differ in different accelerators. In some, it is important to have very small spills; in some, maximum capture; and in others, maximum capture in a small phase angle.

HAIMSON: The electron linac people have considered many of the problems which have been discussed this afternoon. They have studied tandem prebunchers, harmonic prebunchers, chopper prebunchers and tapered phase velocity bunchers. Tapered traveling wave circuits have been used in many electron linacs over the last 20 years, particularly in supervoltage therapy applications. It appears that in these various approaches we have been motivated by the same problems that motivate proton linac designers, namely, the need for more current and higher resolution. Early work demonstrated that correctly designed prebuncher systems could capture about 65% of the output of an electron gun. As the machines become more sophisticated, however, and higher resolutions were required, it became obvious that the "over bunching" technique, to get more current, usually resulted in too large a spread of velocity at injection. This in turn limited the minimum value of final longitudinal phase spread that could be achieved. I am wondering, therefore, if the electron linac approach of a high energy injection, a combined chopper-prebuncher system, has been considered for proton linacs to decrease this emittance growth, of which I have heard so much about today. The initial loss of current, however, might preclude such an approach.

L. SMITH: Before closing this part of the discussion, I would like to ask Dr. Lapostolle to present his thermodynamic argument to the panel for criticism or comments.

LAPOSTOLLE: First, I would like to make an additional comment on bunching. It would be advantageous to increase the energy at which bunching is done when one is working with high currents because then the space charge effects will decrease. There is also an advantage in using short systems so that the space charge effects and especially non-linear space charge effects act for a shorter time. These, however, are just rough suggestions.

In regard to the thermodynamical model, I would like to repeat that it is not founded on any theoretical basis. On the contrary, there are a lot of difficulties to justify it. I have only tried to find a descriptive model, not understanding yet how it works.

I consider that the motion of a bunch of particles in a system such as a linac is defined by three coordinates, one longitudinal, and two transverse and I define for each coordinate, a "temperature" which is proportional to v_z^2, v_x^2, v_y^2, and denote them as t_1, t_2, and t_3. As long as there is no coupling between coordinates, the motion is completely normal. I assume now, however, that there is an exchange of kinetic energy between the various coordinates, due to space charge, non-linear effects, etc. These couplings vary according to some increasing function of beam density. One result of this transfer is that the temperatures tend to become equal. Equal temperatures normally imply a round beam, as I mentioned at the beginning.

To go further, consider the transverse motion of a beam in a strong focusing device in which the transverse velocities and dimensions are varying along the device. The question of entropy becomes important. If the exchanges of energy are done at equal temperature, the system is completely reversible and this implies constant emittance. On the other hand if the two temperatures are always changing and are not equal to each other most of the time, then the heat exchange will not be reversible. This means that the entropy will increase. If one computes this effect, i.e., the change in entropy in the thermodynamical model, one finds that the beam growth, measured by changes in entropy is proportional to a certain function of beam density, i.e., a function of current and emittance. If one considers that heat exchange is proportional to density or to the square of density, one finds that the emittance grows like the 1/3 or 1/2 power of time. The emittance growth and therefore the final emittance is then a function of intensity (at low intensity the emittance may remain small). The heat transfer is created by the "wiggling" in the strong focusing device and a focusing system with less wiggling produces thus less emittance blowup than one with large wiggling.

This in a few words is what I meant by my thermodynamical model.

I would like to say, in addition, that some measurements are being made on a purely transverse effect in the beam injected into the synchrotron at 50 MeV without rf, i.e., without longitudinal force. It seems that in only one turn which is 600 meters, there is a change in emittance and a rounding of the beam. The effect is small but intensity dependent. The accuracy of the measurements is however, as yet, not very good.

GLUCKSTERN: If you bring together two things of unequal temperature, one will be cooled. Does that imply that the emittance for that coordinate would decrease?

LAPOSTOLLE: According to this model, yes. I haven't observed this however.

L. SMITH: We now invite contributions from the floor.

SLUYTERS - BNL: Are two bunchers really necessary? I think that we have seen, from talks today, that it is desirable to keep the preinjector rather simple, by only using a single buncher. I should like to present this problem to the panel. From all the calculations which are done up to now, can a single buncher do the job?

LAPOSTOLLE: I may say that a double buncher has been used recently on the Rutherford linac and according to what I know, it gave good results. Alan Carne will report about this later this week.

VAN STEENBERGEN - NAL-BNL: The comment comes to mind that: so far, the fact, that the linac sometimes serves as an injector for the synchrotron, seems to have been neglected. This comment is directed to the remarks by Dr. Gluckstern. Do we design the bunching section for high linac current or low current performance? Since not only voltage level and phase, but also component location is involved, it is important to consider this question. If the linac is to serve as an injector for a synchrotron, the optimum performance related to energy spread and beam emittance should also be achievable at low beam currents. This is because optimum performance of a Linac-Synchrotron combination might, with multiturn synchrotron injection, be achieved with lower linac beam intensity, especially if phase space dilution in the linac at high beam currents is taken into account.

The next comment I wish to make relates to the desirability of using the buncher for maximum trapping efficiency. The associated high current density in physical space at the beginning of the linac is probably dominant in transverse phase space dilution. This, again, might be more important, than maximum beam intensity. For this reason one gap or two gap bunching structures should be considered from a six dimensional phase space point of view. Would it actually not be more desirable to revive the adiabatic trapping structure again?

The last comment is directed to Dr. Lapostolle's phase space coupling model. Does the model suggest that for equal transverse emittances there would be no transverse blowup and, related to this, that transverse dilution could be avoided if circular symmetry could be maintained?

A last question relates to "equalness" in the model of transverse and longitudinal phase space projections. What are the units of equalness?

LAPOSTOLLE: On your comment about the aim of bunching, whether it is good to have bunches as tight as possible at the linac entrance, I agree completely with what you say: the beginning of the linac should be included in the computations to be sure that, what is produced, is good for it. One cannot treat the bunching at the linac input independently of the behavior of the beam afterwards. There is some relation between the two problems and one cannot separate them. Now about your question concerning an optimum injector, I would say that if you have at your disposal a high intensity linac you can also run it at low intensity over several turns and determine which is best; I am not sure however that this is a good answer. About your question concerning the possibility of having equal emittance and no blowup for circular symmetry; according to the model, that should be true. A round beam would remain round and of constant diameter but a flat beam would become round, at least if the intensity is high enough, but I don't know how focusing can be achieved with circular symmetry. Your last question was about units. In my model I make a Lorentz transformation into a frame of reference moving at the velocity of the center of gravity of the bunch; there I consider the velocities or rather kinetic energies along the three coordinates.

GLUCKSTERN: Does that assume that the spring constants are the same and that the restoring force gradients are equal?

LAPOSTOLLE: No, the restoring force gradients may be different.

GLUCKSTERN: Then the three dimensions won't be equal.

LAPOSTOLLE: They won't be equal, only the velocities would.

CURTIS - NAL: I'd like to return to the question of the buncher for a moment. It is well known that a saw-tooth wave form is ideal for bunching low intensity beams. I would like to ask the panel: If someone were to come up with a saw-tooth buncher, would this be useful for high intensity beams? Granted, it is difficult, but there have been suggestions from some quarters on how to do this. It has also been suggested that a wave form of arbitrary wave shape may not be any more difficult than a saw-tooth wave for a single resonant cavity. If such a cavity were possible, would this be a useful device when large space charge forces are present? Would it be an improvement over the other bunchers?

AGRITELLIS - BNL: I made some runs with saw-tooth wave for 100 and 200 milliamperes. It seems this model works very well and one can have a homogeneous beam in phase-energy space at the entrance of the linac. The particles are spread very nicely and one can achieve very good bunching efficiency.

NEAL - SLAC: I'd like to continue the early remark that Haimson made regarding some of the earlier experience with electron machines. In particular he referred to the use of continuous bunching or tapered bunching which was very widely used in some of the early electron machines 10 - 15 years ago. I sometimes think that we may have dropped this particular type of system too quickly. One of the main reasons it was abandoned in favor of discrete-type bunching using independent cavities is that when one has to work over a very wide dynamic range of currents, as is typical with most physics type electron machines, it is quite difficult to always optimize the continuous or tapered type buncher. If the conditions for a high current beam are optimized, these conditions may turn out to be far away from optimum for a much lower current beam. For this reason people went to the individual cavity type of bunching in which independent control of the phase, the power input to the cavities, and the relative phase between the cavities and the accelerator are possible. In the continuous type bunching, at least with electrons, one could make use of essentially two effects. One was the increase in the so-called relativistic longitudinal mass of the particles; in addition, the continuous buncher could be designed to have a continuous increase in the field strength as a function of distance. Both of these factors led to a rapid phase compression. One can finally take advantage of some additional phase compression in the process of capture. Typically, a factor of five and even as high as ten in final compression can be obtained in the transition to the asymptotic phase angle in electron machines. Finally, I wonder whether any attention has been given to the grid type of phase pre-selection prior to the use of velocity modulation; that is, rather than using a combination of longitudinal and transverse modulation, one might obtain initial phase selection by a rf grid type device which might, for example, select 60 degrees out of the total 360 degrees. This selection would then be followed by a velocity type of modulation. Such a method might be advantageous in high current machines and might result in fewer difficulties, in terms of exciting beam breakup and such phenomena, than would result from the use of combined longitudinal and transverse modulations.

GLUCKSTERN: Let me just make one comment. If I'm not mistaken these schemes which have adiabatic capture lead to fairly large oscillation amplitudes. This would correspond to starting with a synchronous phase of 90 degrees in a reduced field and then increasing the field gradually. The impression I have is that one obtains fairly large amplitudes of oscillation which might not be good for some purposes. I think this probably should be looked at again.

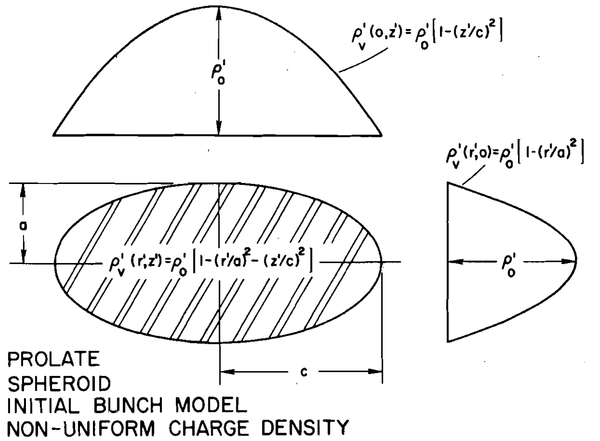
BARTON - BNL: I have a question about the results in the paper today of Mrs. Chasman. Apparently, we have growth of emittance in this linac. Space charge forces are "Liouvillean" so we either don't have the matching right or there is some mechanism like Lapostolle's thermodynamics in evidence. I'd like to know what's going on here.

GLUCKSTERN: I think it's a phenomenon that's been run into before. One possibility is that one gets filamentation, which just does not show up on the scale in which the calculations are done. In this way one gets apparent beam growths which do not necessarily violate phase space conservation. Also, one is looking at a two dimensional projection and you can get increases there even using Liouville's theorm. But, I believe that filamentation is the explanation for the apparent growth of phase space.

NAGLE - LASL: I'd like to make a reservation about the agreement of the two space charge calculations from LASL namely "many-rings-averaged" and "point-by-point". It was remarked by Crandall that there's apparent good agreement between these two calculations. This is, at first sight, somewhat surprising since MAR has a ring symmetry and the "point-by-point" calculation seems to be as far removed from that as possible. However first of all, there are elements of symmetry in the point-by-point calculation, since the XZ planes and YZ planes have been made symmetrical by introducing imaged charges in those planes in order to get effectively four times as many particles. That introduces a considerable element of symmetry. Furthermore, it introduces a problem since if the charges are approaching planes of symmetry, you must at some time have a collision. This collision radius is defined as (one over the cube root) of the original "lump-charge" radius and in this procedure the interaction force between particles within this radius is assumed constant. It seems to me that this procedure is rather similar to the renumbering-of-rings procedure which goes on in "many-rings-averaged" calculation.

References

1. R. Chasman, "Numerical Calculations of the Effects of Space Charge on Six-Dimensional Beam Dynamics in Proton Linear Accelerators", these proceedings, p.372.
2. T. Nishikawa, "A Unified Theory of Beam Dynamics in Proton Linacs", these proceedings, p.395.
3. P. M. Lapostolle, "Bunching", these proceedings, p.317.
4. J. Haimson, "Non-Cryogenic High Duty Cycle Electron Linear Accelerators", these proceedings, p.775.
5. J. Haimson, "High Current Traveling Wave Electron Linear Accelerators", The First National Particle Accelerator Conference, IEEE Transactions on Nuclear Scinece, NS-12, No. 3, June 1965 p. 997.
6. J. Haimson, B. Mecklenburg, "A Relativistically Correct Three Dimensional Space Charge Analysis On Electron Bunching", U.S. National Particle Accelerator Conference, IEEE Transactions on Nuclear Science, NS-14, No. 3, June 1967 p. 586.
7. H. Hirakawa, "Electromagnetic Field of Traveling Charge in Beam Duct", these proceedings, p.400.



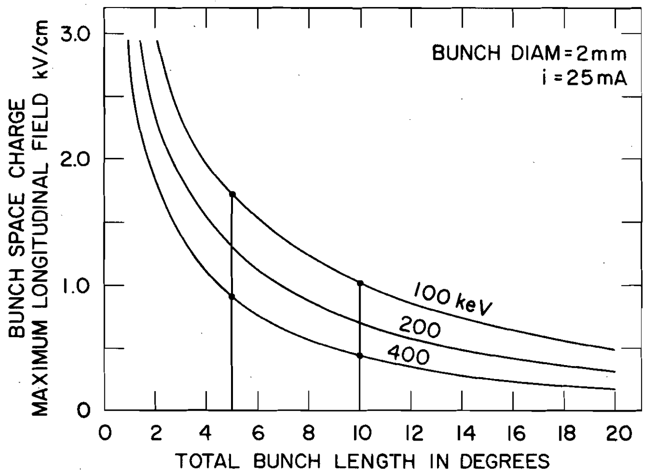


Figure 1 - Space charge maximum longitudinal field for nonuniform charge density spheroidal bunch.

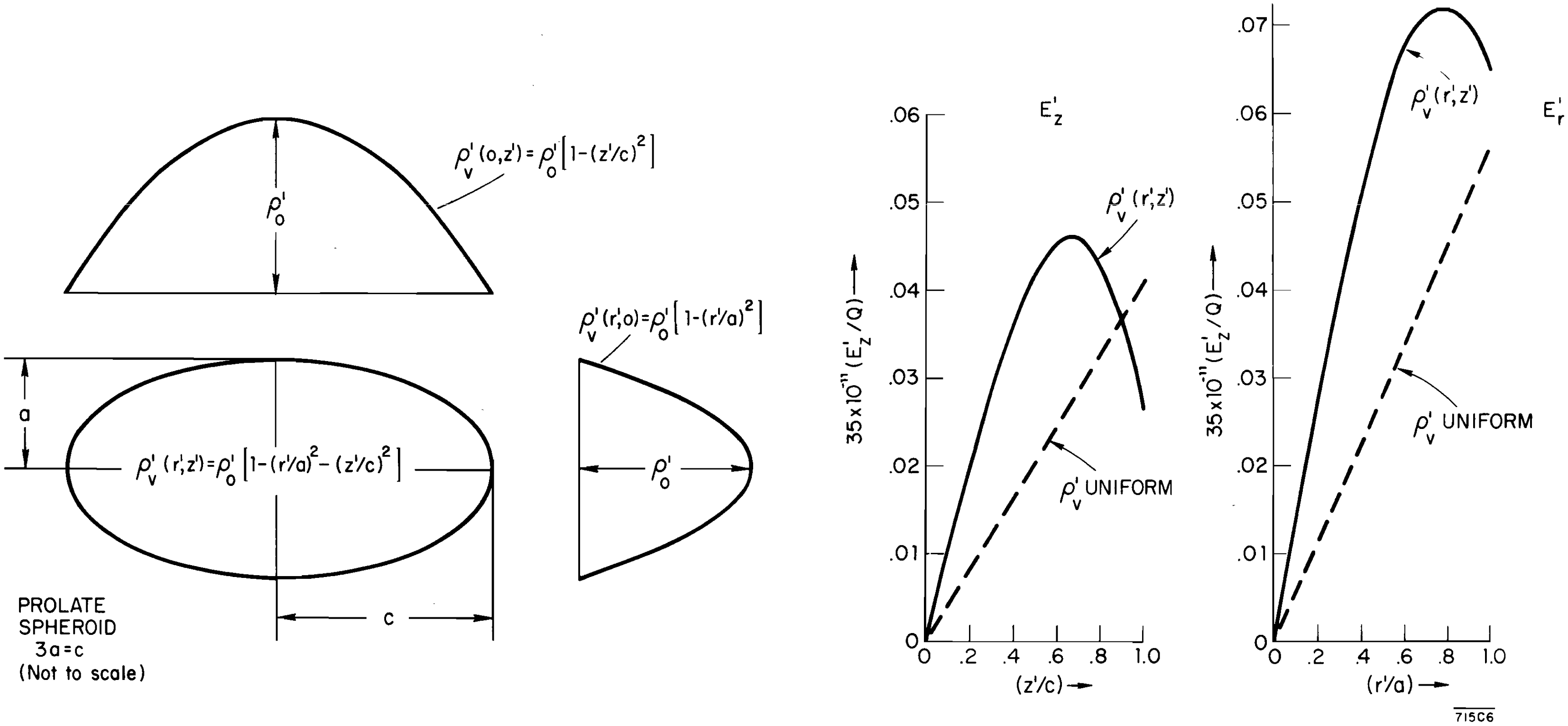


Figure 2 - Field distribution for a nonuniform charge density prolate spheroidal model.

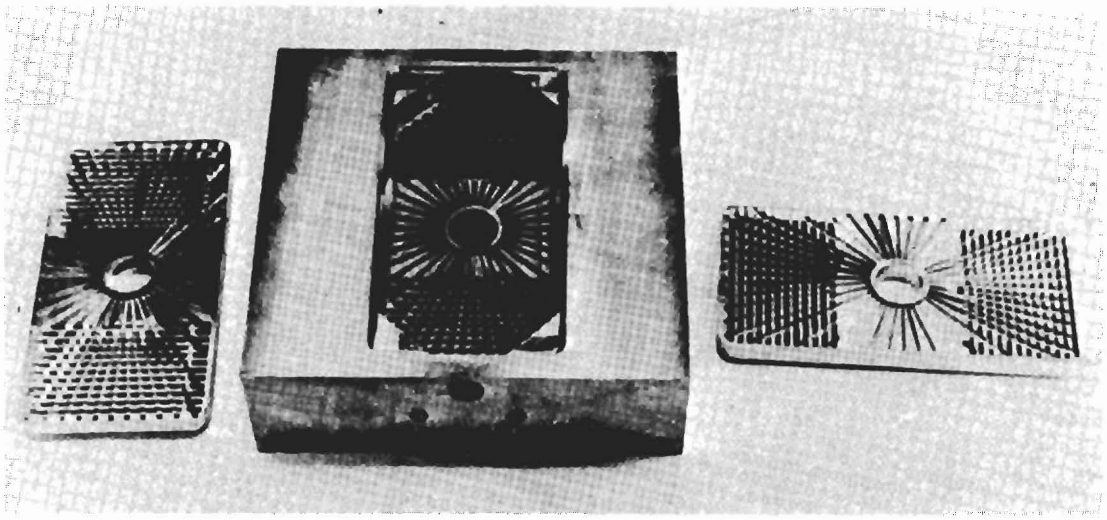


Figure 3 (a) - Transverse deflection higher order mode dual cavity assembly for beam rotation and monitoring of rf bunch length.

Figure 3 (b) - Tungsten screen photographs showing beam rotation and rf chopping at S-band frequency.

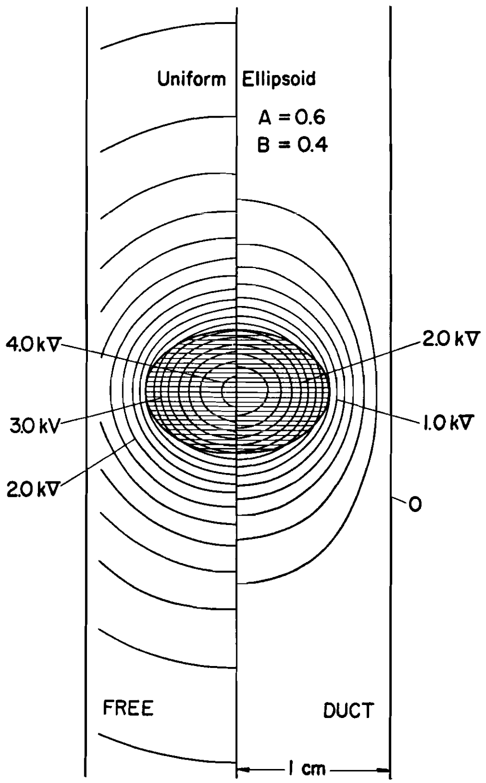


Figure 4 - Equipotentials of uniform charge ellipsoid with semi-axes A, B, cylindrical duct (right half) and in free space (left half). Total charge = 10^{10}e = 1.602 x 10^{-9} coulomb.

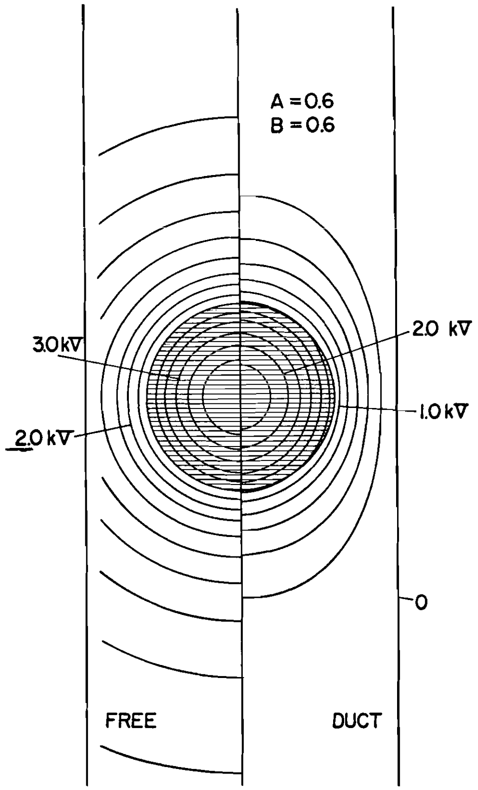


Figure 5 - Equipotentials of uniform charge ellipsoid with semi-axes A, B, and cylindrical duct (right half) and in free space (left half). Total charge = 10^{10}e = 1.602 x 10^{-9} coulomb.

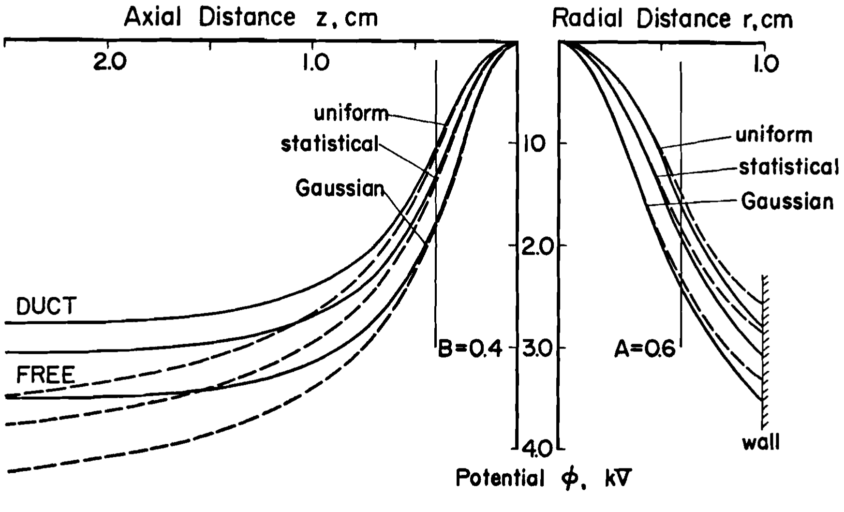


Figure 6 - Electrostatic potential on z-axis and r-axis of three types of ellipsoidal charge distribution, (i) uniform, (ii) statistical, and (iii) Gaussian. Total charge = $10^{10}e$ = 1.602×10^{-9} coulomb. Potentials in cylindrical duct (solid lines) are compared with potentials in free space (dotted lines).

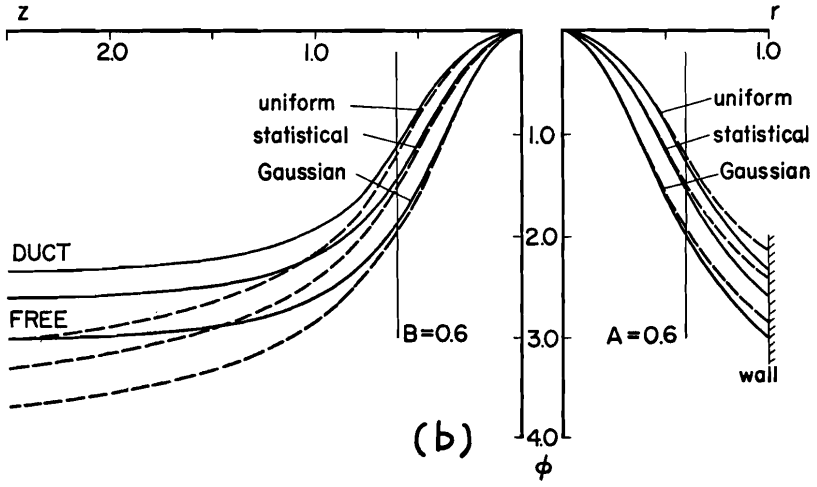


Figure 7 - Electrostatic potential on z-axis and r-axis of three types of ellipsoidal charge distribution, (i) uniform, (ii) statistical, and (iii) Gaussian. Total charge = $10^{10}e$ = 1.602×10^{-9} coulomb. Potentials in cylindrical duct (solid lines) are compared with potentials in free space (dotted lines).