ROUND TABLE DISCUSSION ON RF STRUCTURES

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DÔME, CERN: Essentially, I shall report on the latest measurements we have made at CERN, on the tank 1 and tank 2 models that I have already spoken about.[1] These are nonuniform structures, representing variable energy drift tube linacs.

We have found it is possible to compensate tank 2 model (10 - 30 MeV), by using á crossbar structure, with stems as shown in Figure 1.

It is necessary to change the stem diameter progressively along the structure in order to keep the zero mode frequency of the stem resonance equal to that of the Alvarez zero mode. We started by looking at the theoretical stem diameters which are necessary to achieve this. We have rounded off these theoretical values to the nearest mm because such stem diameters were available in stock at the laboratory. We measured perturbations of the axial electric field due to a perturbing rod introduced at one end of the tank. The results, which are reported elsewhere in this conference,[2] are summarized in Figure 2.

The first curve (Figure 2a), which was obtained by using theoretical values of the stem diameters, was not too bad. However, we were able to improve it quite a bit, by changing the stem diameters by trial and error (Figure 2b). The introduced perturbation essentially excites the modes adjacent to the zero mode. Therefore, we measured the axial field distribution in these adjacent modes, and varied the stem diameters to ensure that these modes were propagated through the structure. For the stopband being closed all along the tank, it is necessary that no part of the tank be cut off for these modes. This seems to be the easy way of approching experimentally the problem of local compensation.

For tank 1, the low energy tank (0.75 - 5 MeV), we know that a possible compensated structure would employ drift tubes, each having two stems spaced 45° apart. The stems on successive drift tubes would be rotated 180° as shown in Figure 3, and as described in the Proceedings of the VI International Conference on High-Energy Accelerators (Cambridge 1967)[3]

When we started measuring this structure, it turned out that it was very well compensated without further adjustment, due to the property of the short cells: for compensation of short cells, the essential feature is an appropriate angle between the stems, it is not the diameter of the stems. This is the reason why, in a tank that does not go too high in energy, you can keep the same diameter for the stems and the same angle between stems throughout the tank, and still be close to local compensation everywhere.

The theory shows that for confluence between the stem and the Alverez passbands, the angle between stems should be about 135°. This angle is in fact the largest angle which occurs in the structure just described, but it is also present in the structure shown in Figure 4, where each drift tube is supported by two stems at 135°, with no alternation of the stems from one drift tube to the next.

The larger angle (360° - ϕ) which is formed, would however produce a 0 mode-stem resonance at a lower frequency and hence a lower passband. We looked for this and found, not only this passband, but at the same time, some unforeseen properties of the structure. For angles ϕ between 135° and 180°, we observed a very large effect upon the upper dispersion curve (see Figure 5), which suggested the existence of a second lower passband close to the Alvarez-passband and corresponding to the smaller angle. However, we could never detect this: the measured stem passband always corresponds to the larger angle between stems. We found that the modes were widely spaced on the upper passband. The maximum mode spacing occurred at 135°, but without field stabilization.

So far we don't understand these properties. We don't understand why, as one varies the angle ϕ between 90° and 180°, some kind of discontinuity occurs at 135°. At angles outside the range 135° - 180°, the mode spacing becomes very small. We can, in fact, only obtain an approximate field stabilization for this structure, using an angle of about 150°.

GIORDANO, BNL: I am going to discuss some results appearing in two papers in these Proceedings [4,5] by Joe Hannwacker and myself concerning some measurements on multi-stem structures and the properties of these structures when used in variable β cavities.

A model cavity was constructed with a variation of β along its length approximately eight times larger than will exist in a tank of our new proton linac, in order to accentuate any new or unsuspected effects that might be inherent in the multi-stem structure. We found that with single stems, this structure had a field tilt of about 11 dB but, when converted to a four-stem structure, this tilt was greatly reduced. Upon scaling back to a proton linac tank this corresponded to an improvement in flatness of about twenty-five. Precision bead pulling measurements along the axis, and H field probes measurements on the wall were in excellent correspondence, indicating that everything was very well behaved.

Upon considering the operation of these structures and reviewing all the measurements made on multi-stem structures and post-coupled structures, one comes to the following conclusion: many kinds of structure can be generated from the one-stem Alvarez structure in which the TM dispersion curves are quite different. However, it is found that each of these produce about the same amount of compensation for the average field. Figure 6 shows the dispersion curves for the single-stem, post-coupled, and multi-stem structures. The latter two have an equivalent field compensating effect, even though their dispersion curves are different.

At higher values of β the TM_{01} dispersion curve of the post-coupled structure resembles that of the Alvarez structure, but this fact does not seem to interfere in any way with the stabilization of the cavity. One can alter this structure, for example, by putting all of the post-couplers on the same side of the tank. The TM dispersion curve is essentially unaltered but the lower pass-band becomes very narrow. About the same amount of stabilization is achieved, but it is extremely difficult to adjust this structure.

In trying to devise a theory which accounts for the fact that compensation is achieved in structures with considerably different dispersion curves, we might assume that the region around the TM_{010} mode is of major significance. With normal shorting ends on a cavity, we can excite the TM_{01n} modes and the lower TS modes. With appropriate open ends, we can then excite the TS_{110} mode in either the multi-stem or post-coupled structure while suppressing the TM_{010} mode. Measurements showed that the TS_{110} mode has E_z and H_φ components and when this mode is perturbed there is a tilt in these fields.

We postulate in one of our papers[5] that the actual field compensation is a result of the superposition of the TM_{010} mode and the fields of a TS_{110} mode which are induced by the perturbation.

KNAPP, LASL: Thank you Sal.

SWENSON, LASL: Part of what I will say appeared in the Proceedings of the Cambridge Accelerator Conference,[6] and part is prepared for publication in the Proceedings of this conference.[7] Figure 7 shows the Los Alamos post-coupled drift tube structure. This is a 500 MHz model of a 35 cell post-coupled structure. The geometry, at the low energy end, scales from a 40 MeV proton linac geometry and the high energy end, from a 100 MeV geometry. Ed Schneider has made all the measurements on the structure. Figure 8 shows a view inside the structure. You see the drift tube supported by a single stem in the vertical plane, and the posts coming in from alternate sides. Each post is directly opposite the center of a drift tube. We find that the alternation is an important part of the performance, as Sal just noted. These posts were the first ones that we used. Subsequently we have used smaller posts, and the power losses on the posts were correspondingly smaller. Figure 9 shows a model with which we recently worked. Its geometry corresponds to an energy range from 5 MeV to 30 MeV. We cover the range from 5 to 30 and 40 to 100 MeV and Curt Owen at the National Accelerator Laboratory has a model that goes from 190 to 200 MeV. All these models perform well.

In both the conventional linac and the resonantly coupled structure, the axial fields in all the gaps are in the same direction. We analyze the circuit analog in terms of chains of coupled resonators. If there are six cells, they are represented by six resonators in a chain and will have six modes on the diagram as shown in Figure 10. Of course, the zero mode is the one which is used in the conventional structure. The resonant coupled structure is drawn as having the same length with the same number of cells, but the posts have been added. There are now six accelerating cells and five post couplers, a total of eleven resonators in the chain. We now find eleven modes in the mode spectrum. The lower five modes are modes where most of the stored energy resides in the resonant coupler. The upper six modes, of course, have most of the stored energy in the accelerating cell. The fields in these six modes are very similar to the fields in the six modes of the conventional structure.

In referring to the circuit analog, we define the phase shift between the accelerating cell and the coupler to be φ. Then it's true that the phase shift will be 2φ between two accelerating cells, for a strictly biperiodic structure, and this leads us to call the mode we are using to $\pi/2$ mode. We prefer this terminology because it gives us clearly an indication of which cells have energy and which do not for every mode in the entire spectrum. If you chose to call our $\pi/2$ mode the zero mode then you require more terminology to describe the excitation of the mode. This mode, however, is the only mode in the entire spectrum (lossless case with no tuning errors) that has energy in the accelerating cavities and no energy in the resonated couplers.

In the last four months Ed Schneider has made some careful measurements looking for the excitation of the post-couplers and I will describe this if it comes up in the discussion. Figure 11 shows the dispersion relation derived from the circuit analog and quite often, depending on the values of the five parameters of the circuit equation, it consists of two passbands separated by a stopband. However, as George Dôme said, if the resonant couplers are tuned until these two bands come together, the group velocity in the $\pi/2$ mode becomes finite. The analysis indicates that the stability of the tank is inversely proportional to the width of the stopband and is also a function of some coupling constants. As can be seen and as we have found in practice, the degree of stability one can achieve, depends on the patience used to adjust the resonant coupler.

Figure 12 shows the result of a bead perturbation measurement on the 26 cell model (5 to 30 MeV). The center curve represents a flat tank, that is, a constant average electric field on the axis. This is more or less the field distribution that we wish to have. The upper three traces correspond to the structure without post-couplers, the lower three are for the structure with post-couplers properly tuned. The standard experiment, to test the stability of the tank, is to make tuning errors on the end cells. We always make equal but opposite perturbations on opposite ends of the tank so that resonant frequency of the structure remains constant. If we look at the case of a conventional structure, taking this to be the initial condition, and make a +5 MHz tuning error at one end and a -5 MHz error at the other, you see that the field distribution changes quite a bit. If you then make tuning errors in the opposite direction, the field tilts in the other direction. However, for the post-coupled structure and the same size end perturbations the corresponding two plots are very similar to each other.

KNAPP, LASL: Now I think we should have a discussion amongst the members of this table. Perhaps I could start. I think our position of Los Alamos is, and has been for some years, as follows: all of the structures, starting with loop-coupled pill box structures (proposed in 1955 or so at the Rutherford Laboratory), the crossbar structure (which has been considered for an intermediate energy proton linac), the side-coupled structure, the alternating-periodic structure, the multi-stem structure and now the post-coupled structure, operate on the same basis. A resonant element is located in the system in such a way that it is unexcited in the operating mode; but if power needs to flow, or if perturbations occur in the cavity geometry, a weak excitation of the resonant elements serves to give very high stability to the operating mode field distributions. I gather from the comments that Sal made that he perhaps felt that this was an adequate understanding of the system. Would you agree with that Sal?

GIORDANO, BNL: No! At this time I think the multi-stem, and post-coupled structures operate on one mechanism, but the alternating-periodic and side-coupled structures operate on a different mechanism. One approach which I used, was to look at the resonant conditions in the structures. Another approach, which I hope to take in several months when I get a few more cavities, is to look at these conditions from a propagating point of view. I feel that this particular approach to measurements has been bypassed. I think that the next question one asks is: "what are the basic differences between the field of the multi-stem and post-coupled structures and how do their field differences affect the beam?" We all agree that the basic mechanism is the same for both structures, but each one has a different field configuration. This is borne out by the fact that the dispersion curves are different, showing that the coupling coefficients are different; however, it is still possible in each case to get a flat average field.

KNAPP, LASL: Don Swenson and Ed Schneider have a considerable body of data concerning the fields introduced by the weak excitation of the posts. Perhaps Don should comment on this now.

SWENSON, LASL: We looked at these and I would like to discuss them.

We pulled a slug along the wall of the 35 cell (40 to 100 MeV) model. The slug was offset in the tank so that it was closer to the even number posts then to odd. From MESSYMESH calculations, we expected that, in the absence of perturbations the magnetic field at the wall should be larger opposite each drift tube and weaker opposite each gap and the posts should be unexcited. The variations should be about 2% at the low energy end and increase to 10% at the high energy end. Our measurements confirmed this (Figure 13a). From a first order perturbation theory, we would expect that a positive tuning error at one end of the tank and a negative error at the other should cause the posts to be uniformly excited. Since the slug is nearer the even posts than the odd, we should be able to detect a fluctuation with a period twice that in the unperturbed case. This too was confirmed by experiment (Figure 13b).

Now if the signs of the tuning errors were reversed we would expect the posts to be excited again, except in the opposite sense. We measured a very similar pattern except something was reversed, which we could explain in terms of the superposition of the fields, due to currents in the posts and the TM_{010} fields (Figure 13c). First order perturbation theory predicts that, if you make a tuning error at one end and another in the center, only the posts between these points should be excited while the remainder should be unexcited. In Figures 14 (a and b) one can see the results when the tuning errors are in the first half of the tank, and then in the second half.

We did another experiment using the same technique. If these post excitations are associated with propagation of rf power along the structure, we should see differences in the field pattern as we move the drive from place to place. I have brought no slides of the patterns we obtained, but we could see no difference in these field patterns, as a function of drive location. This means that the post excitation that you need to satisfy different beam current conditions is very small, and the excitations due to tuning errors are the only ones which we can see. For the nominally tuned-up condition of our present rough model, these are very small.

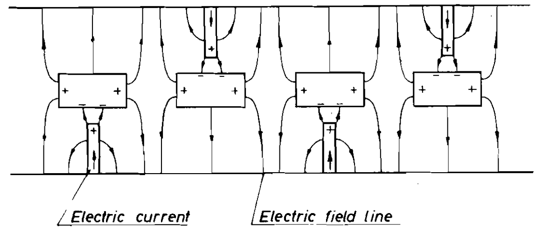
KNAPP, LASL: Sal has a comment.

GIORDANO, BNL: I realize that the perturbations you are introducing must be quite large, just to accentuate this effect. If you run the bead along the wall on one side of the cavity, and then repeat this measurement on the wall diametrically opposite, you will find the same ripple, due to stem excitation, only displaced in space-phase by π radians. The only field configuration, that would fit here, would be a skewed field which has a transverse component. It is difficult to measure the transverse field on the axis, in the presence of the TM_{010} fields. If you put any sort of dielectric on the axis, you distort the fields, and if you use a needle of appreciable length, it becomes the main thing which determines the field shape. Just looking at the fields on the wall, it would appear that transverse fields exist, and this bothers me.

SWENSON, LASL: Well, of course, most of our effort in the last few months has been to try to measure such fields and whether we have succeeded or not is debatable. We have established this field for various tuning errors and the results are reported in our paper in this Proceedings.[7] The measurements involved rotating a needle mounted on a very small hollow ceramic rod, aligned with the linac axis. As the rod is rotated, the resonant frequency should vary between maximum and minimum as the needle is perpendicular and then parallel to the transverse field. We are looking for a very small transverse field in the presence of a large axial field. To produce noticeable effects, we had to produce tuning errors in the end cells that were 100 to 1000 times as big as one would expect to normal practice. Then, however, we found that the vector sum of the accelerating and transverse fields were 4 to 6 degrees from the axis. This illustrates again, that the post excitation is due to tuning errors. On the basis of the very large tuning errors that we had to introduce, we would expect that these transverse fields, due to post excitation in a real linac, will be 100 to 1000 times smaller than we have measured. They will be small compared to the normal radial impulse, due to rf defocusing forces that a proton will experience in crossing a gap and can be easily accommodated with the quadrupole focusing system.

We made the same measurements in a conventional drift tube structure without resonant coupling. In this case, we could introduce detuning errors only one-tenth as large and still maintain resonance in the proper mode. The transverse magnetic field was however about one-tenth as large also. While this may have been a coincidence, it seems that the amount of transverse field per MHz of tuning error was about the same.

DÔME, CERN: In fact, in the 0 mode of the post resonance, the periodic-field pattern and the currents are as in the sketch.



Periodic-field pattern of the post 0 mode

The charges on the plane faces of each drift tube will have the same sign since this case corresponds to the zero mode. Essentially, the field in the gaps has rotational symmetry and is symmetrical about the center of the gap, resulting in no net voltage across the gap. This means that excitation of the posts will produce a negligible longitudinal effect on the particles. I also feel that the transverse effects should be very small.

KNAPP, LASL: Curt would like to comment on some of the measurements you have made.

OWEN, NAL: Our results have been quite similar to those that Don has reported. We haven't had the mechanical stability, until about 2 weeks ago, to even consider investigating this effect. We do see the very large H field variation whenever we put large perturbations in the end cells. We use a similar technique to the Los Alamos technique, and when the structure is reasonably well tuned these variations essentially vanish and the structure looks like a normal linac. This has briefly been our experience.

CARNE, RHEL: I can't really make any comment about the Alvarez structure, but several years ago at CERN we tried to do a similar kind of experiment with the crossbar structure, whose normal mode of operation depends essentially on the transverse resonance of the stems. We made perturbations, rather more crudely, I think, then Don did in order to detect any asymmetry. Essentially, we whirled a bead around in the drift tube gap at 2 kc/sec, and detected and amplified through a tuned amplifier. In our case we saw nothing outside the normal general noise of the experiment. We concluded at that time that the drift tubes smooth out any asymmetry that may be introduced. I think that Don's results are very comforting.

SWENSON, LASL: I am glad to see, from the diagram that Georges Dôme drew, that the arrows are pointed in the same relative direction that our analysis indicates. The fact, that the excitations which do occur are of this general configuration, means that a particle crossing successive gaps will get equal and opposite kicks, due to the alternation of the posts. The kicks are very slight because the stored energy is primarily between the post and the drift tube where it is relatively well shielded from the particle. When we measured the transverse fields we used three different needle locations, and we did find a horizontal component to the field which had just this property. However, there was a much bigger vertical component which was also present in the conventional structure.

GIORDANO, BNL: Just to keep the record straight, I feel that there is a transverse component. I don't know the magnitude. It may not be negligible.

KNAPP, LASL: The discussion is opened to floor.

MILLER, SLAC: With the field configuration you have drawn there, it would appear that you have a transverse magnetic field at the symmetry point that wouldn't cancel in one gap, but would cancel in two gaps. I believe you can measure the effective transverse impulse on the particle going through a gap by measuring dE_z/dx or dE_z/dy, the derivative of the longitudinal field on the axis, and I believe this measurement effectively includes the effect of both magnetic and transverse electric fields. I wonder if this is not a better method of measuring the effect on a particle.

DÔME, CERN: These kind of measurements would be very difficult.

MILLER, SLAC: If you are measuring a 6° tilt in the field, I think this would be relatively easy to measure. We can make field measurements to a fraction of a percent, without great difficulty on our linac. In regard to the symmetry problem with our "rf-couplers", we took the following approach: we measured the transverse impulse that the particle would get traveling through the rf-couplers which were excited by a measured longitudinal field, as a function of displacement in the X or Y direction. Then the slope of this function at the axis gave us the effective transverse field seen by the traveling particle.

FEATHERSTONE, CERN: Would the power losses associated with excitation of the post-couplers, due to normal fabrication and assembly errors, be so small that one could reasonably hope to omit ball tuners?

SWENSON, LASL: Yes, the power losses due to the power-propagation excitation of the posts are very, very small. The power losses associated with the resonant-couplers are due to a perturbation in the TM_{010} magnetic fields that pass on each side of them. Now this is simply proportional to the radius of the post or stem. We plan to use posts with a diameter of one inch, which add something like 5% to the overall cavity loss. Now the single stem supporting the drift tube needs to be larger because of the services required by the drift tube quadrupole, etc. The loss here is 10 to 12% per stem. We first became somewhat concerned in the study of the multi-stem structure, when we realized that the copper in the stems required an additional 30 - 40% of rf power.

You asked about the need for ball tuners in a compensated structure. The field distributions are completely independent of tuning errors. Ball tuners would be nothing for you. You might ask, "What if you build a variable β structure and its field distribution is slightly different from what you had planned, when you chose the drift tube spacing and the energy?" You would then need some way to redistribute the fields. We would certainly prefer to adjust the coupling constant in a way I could describe, but there isn't time. In any of these structures if you have an adjustable coupling constant, you could redistribute the field at your will.

GIORDANO, BNL: One other point, the structure is so stiff that you are stuck with a flat distribution. The only purpose of the tuners would be to adjust several tanks to the same resonant frequency. And as far as the additional loss in the multi-stem structure is concerned, I think that one has to have at least one large or two fairly small stems to carry services to the drift tube. It depends upon the amount of power you are putting into the beam, how significant these stem losses are.

CARNE, RHEL: I think I'd like to make a comparison between the two alternatives on this subject. Having operated the P.L.A. for several years, we have realized the advantages of having the ability to tilt or shape the field to "peak-up" the performance of a linac. This is particularly true if we want to control, for example, the beam energy spread. The case of an injector might be slightly different because once you have a certain field distribution, operational requirements will not allow you to vary the characteristics of the linac. In setting up, and finally peaking up a linac, I think that the great advantage that post-couplers have is the flexibility to tilt the field, if you want to. I find it difficult to imagine how you can do this in the case of the multi-stem structure.

GIORDANO, BNL: Well, in the case of the multi-stem structure you can tilt the field slightly by putting quite severe perturbations in the end cells. You can actually change the gradient in the end cells and hence to some extent have a handle on the energy adjustment of that particular tank. Putting such a severe perturbation in that one cell does not change the overall frequency very much.

L. SMITH, NAL: In this matter of the fields in the gap, it seems to me, that the effect of axial asymmetry is quite easy to see. The longitudinal field component will affect the transit time factor slightly and the radial component will change the rf defocusing force slightly.

SWENSON, LASL: I think I agree with you.

SCHOPPER, KARLSRUHE: We know that in ordinary biperiodic structures, in order to close the gap in the dispersion curve, you need coupling to the second nearest neighbor. I would expect that the effect would be more important in a structure like this.

KNAPP, LASL: Perhaps I could comment on the necessary criterion that the dispersion curve be "closed". I think, in his talk this morning, Lee-Whiting[8] showed that in a "coupled circuit approximation", the stabilities and power propagation characteristics are not affected by the existence of "second nearest neighbor" coupling. George Swain from LASL has also demonstrated this same result, which is reported in the minutes of the 1966 Linac Conference at Los Alamos.[9]

It is our feeling that "second nearest neighbor" coupling is not important as far as stability is concerned. However, the sign of this coupling, as G. Dôme[1] pointed out this morning, is important in determining the excitation of other modes.

SWAIN, LASL: We've done some analysis on a circuit analog similar to that presented by Dr. Lee-Whiting[8] except that we also have included couplings to other than just the nearest neighbor. There is just a small additional effect. As for the simply-coupled case, the limit to the amount of field stability depends mainly on how far one is willing to go to close the stopband. Assuming one can close it exactly, there is a small residual, that one cannot eliminate, that depends on these high order couplings. However, the residual has the very nice property that it is limited to just the region of the linac where the error occurs. It does not create a tilt down the whole structure. Such residual effects have been observed during the tests of field stability made on our drift tube linac models.

HUBBARD, NAL: Why do you alternate the side in which you put the post-couplers and is there an analogous requirement in the other stabilized structures which presumably are describable by the same theory?

KNAPP, LASL: I think that this is the same point to which I was addressing myself in my last remark. Don, would you comment?

SWENSON, LASL: The alternation is important in the post-coupled structures because, in these structures the next-nearest-neighbor coupling is important. By alternating the posts, one changes the sign of the coupling between adjacent posts. The width of the lower passband is associated closely with the post-to-post coupling, i.e., next-nearest-neighbor coupling. As far as alternating posts are concerned, it is just a matter of choosing the most advantageous sign of the coupling constant to give you the widest total passband.

DÔME, CERN: I would like to say that we have a somewhat similar situation, when we have two stems per drift tube. By alternating the stems, we are able to join the two dispersion curves. If we don't alternate the stems, the lower dispersion curve can never reach the frequency of the upper curve.

KUNTZE, KARLSRUHE: We have done some work at Karlsruhe on closing the stopband of a 800 MHz biperiodic slotted iris structure. We did this by detuning the accelerator cells containing drift tubes with respect to the coupling cells. We reduced the stopband width to about 400 kHz (in a total band width of 100 MHz). There is a question I want to ask Dr. Dôme. Did you say, that you can prove that the slope of the dispersion curve is finite, if you have confluence of two passbands?

DÔME, CERN: I would say that we have no general proof of it so far, but my feeling is that the only way to really achieve zero slope would be to have two uncoupled passbands. Then you could probably join the two bands together, keeping the zero slope. Two passbands having no coupling at all, is really very difficult to achieve. You can get it, if the confluent modes are degenerate, but I think this is the only case.

LEE, BNL: I am curious about the effect of the adjacent modes. During transients, these modes are excited. If you get transverse fields associated with the nearby adjacent modes, I wonder how serious this problem of deflecting the beam may be?

DÔME, CERN: I think these fields have been investigated by Don Swenson of Los Alamos, the only difference being that there, he measured the transverse fields coming from zero mode excitation of the post. You are probably speaking of the adjacent modes other than the zero mode. The kind of transverse field you get is probably very nearly the same as with the zero mode.

KNAPP, LASL: We do have some practical experience on this particular point. Some years ago we measured the fields in the side cavities of a side-cavity-coupled chain during the transient turn-on. The general results were that: during the filling time of the tank, the fields in the side cavities seemed to about double the value they were at steady state, due to the additional transmitted power required to fill the tank. The field in the side cavity rose quickly to this value and remained constant while the field in the main cavities was increasing and then it dropped to the equilibrium value afterwards. We have already said that the field of the post excitation, due to power flow, was very small. Double this field and it's still probably very small. In addition, the fact that the coupling of the fields to the posts in the post-coupled drift tube linac is much stronger than it is in the side coupled linac, leads us to feel there is no problem.

LEE, BNL: At the operating frequency, I think that the reason why the transverse fields are small is because your boundary condition doesn't support them. In contrast, the adjacent modes can be supported by the boundary conditions. Once these modes are generated by transients, I don't know what might happen.

KNAPP, LASL: I can only comment that this was an experimental measurement on the side-coupled cavity.

PERRY, ANL: We have a very badly tuned linac; yet Lewis[10] showed at the Cambridge Conference that the field tilt, during a period of constant rf gradient does not shift, even when we were accelerating a beam. There are, however, variable tilts during the filling time. We would like to improve the tuning. My question to the panel is: would either of the two proposed methods of tuning offer any benefits which could not be obtained by any other method of tuning of the individual cells of the linac?

GIORDANO, BNL: This is a very good question. One of the first things we have to ascertain is the effect of the beam on both the amplitude and phase of the tank fields. It has been shown in previous work that the beam changes both the amplitude and the phase of the fields, and that these changes represent excitation of spatial harmonic and higher order modes respectively. The amplitude changes are caused by spatial harmonics, and the phase changes are caused by the higher order modes. Multi-stems or post-couplers reduce the effects of the space harmonic, i.e., reduce the amplitude changes. Multi-stem or post-couplers also give the structure a greater band width, resulting in a larger mode spacing, and a corresponding reduction of the phase changes. I believe a lot of people have seen amplitude variations along a tank as a function of beam loading. It has been observed here.

BATCHELOR, BNL: We certainly have made these measurements, we have observed both phase shifts and amplitude changes along the length of the cavity, the amplitude change for the BNL linac for a 30 mA beam, as I recall, was of the order of 1% from center to end of cavity and phase shifts of the order of 3 to 5 degrees. Indeed if you care to go to the linac today you could observe just these variations as you select various probes along the linac.

HAHN, BNL: I would like to come back to the question of finite group velocity in the confluent case. I would like to ask if this is a necessary condition? I do know that you will say we have improvement even if a small gap remains, but shouldn't one consider this as a perturbation of the case of finite group velocity?

DÔME, CERN: Yes, you may consider it as a perturbation of a finite group velocity in the sense that all these properties vary continuously when you continuously deform the structure. When going through the compensated case everything is continuous. In this sense, you may consider that, when you are close to compensation, what you get is a result which is also close to the compensated case. Is this not the answer?

HAHN, BNL: Yes and no. The question is, if I want to design a structure without making many measurements, shouldn't I just look for the maximum group velocity, not in the fine detail?

KNAPP, LASL: In our experience this has not proven adequate. We find in the post-coupled drift tube accelerator, that looking at the dispersion curve is not a good way to tell whether a structure is compensated or whether the stopband is closed. The best way to do this, we think, is to make tunable elements which can be varied to give optimum stability when various perturbations are imposed on the structure.

HAHN, BNL: I am particularly interested in the biperiodic iris-loaded waveguide. There you can start with various beam holes and always get confluence by changing the resonant frequency of the coupling cells. Depending on the beam hole size you start with, you will get a different group velocity at confluence. Do you say, that it is not enough to achieve confluence and also at the same time a very high group velocity in order to obtain a good stability?

GIORDANO, BNL: You made the assumption that group velocity is going to change as you get confluence, and I don't think anybody here, from what I have read, has looked at the group velocity changes, if they do exist, at that point.

HAHN, BNL: I think there exist a lot of theoretical studies in the literature. So far they all have proven that you have a finite group velocity in the case of confluence. I do not know of any case, with the exception of uncoupled modes, which can coexist at the same frequency. But you cannot have a mode which couples into another one and not get a finite group velocity.

DÔME, CERN: This was my answer to Dr. Kuntze. It would be extraordinarily difficult to get zero group velocity. Your question was, I suppose: if you have several methods to achieve confluence, and these methods would give you different group velocities, what is the best structure? I would be tempted to think that the best structure is the one which has the highest group velocity.

JAMESON, LASL: I would just like to add, for the record, another side of the story that hasn't come out. This discussion has centered on the steady state aspects of all these problems, and to some of us the transient aspects are also interesting and wholesome. There are some snags which are not worked out, as M. Lee pointed out. Hopefully, when you are all finished and have a complete understanding of the steady state aspects, you will go on and bring out some of these other points of view. We are going to make some of these measurements at Los Alamos; I will mention a few of the things tomorrow[11], that we have started. Sal mentioned that he intends to make some measurements from a propagation point of view; I wondered if you also have some transient aspects in mind.

GIORDANO, BNL: Not to start with.

LEISS, NBS: I'd like to get back to Hahn's comments because I, for a long time, carried the conviction that nothing but group velocity counted and I think I've come to realize that in these coupled structures, something more is involved. You can think of it, for example, in terms of a resonant post, serving as the major coupling between the two cells. There's a coupling between the post and one cell, and a coupling between the post and the other cell. I think the analysis shows that when one coupling changes the other also changes in essentially the opposite direction. This may be the cause of much of the stabilization of the entire structure. It doesn't show in an overall dispersion curve which is a mass property of the whole structure.

KNAPP, LASL: I think this is true. We don't feel that a high group velocity or high coupling is really important as long as there is enough to cover the possible stopbands, that you might introduce, due to manufacturing tolerances. The sensitivity to errors is proportional to the group velocity.

LEISS, NBS: I don't agree completely with you on that. I think that the sensitivity, due to variations, is the thing that is uniquely improved by the coupling. However, I do believe that a high group velocity is quite important.

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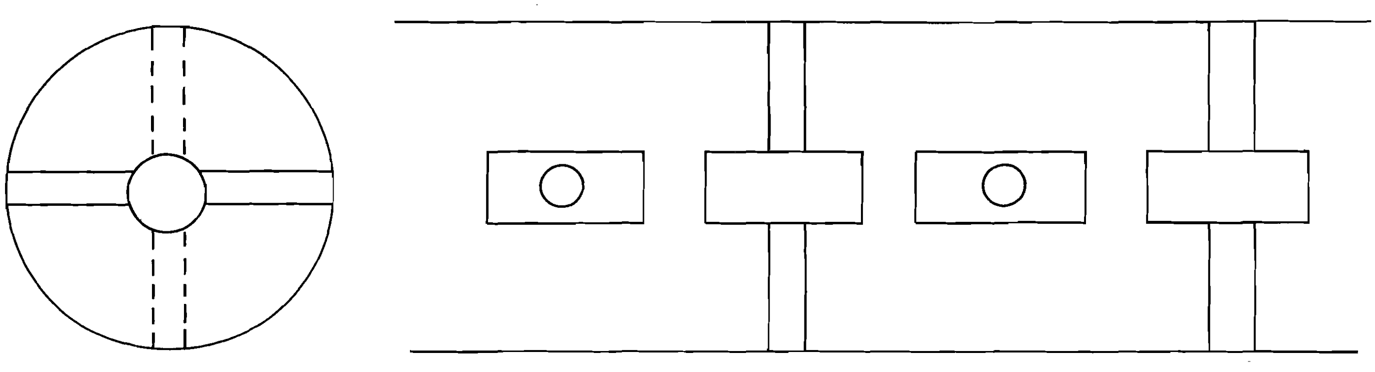

Figure 1 - Crossbar Structure

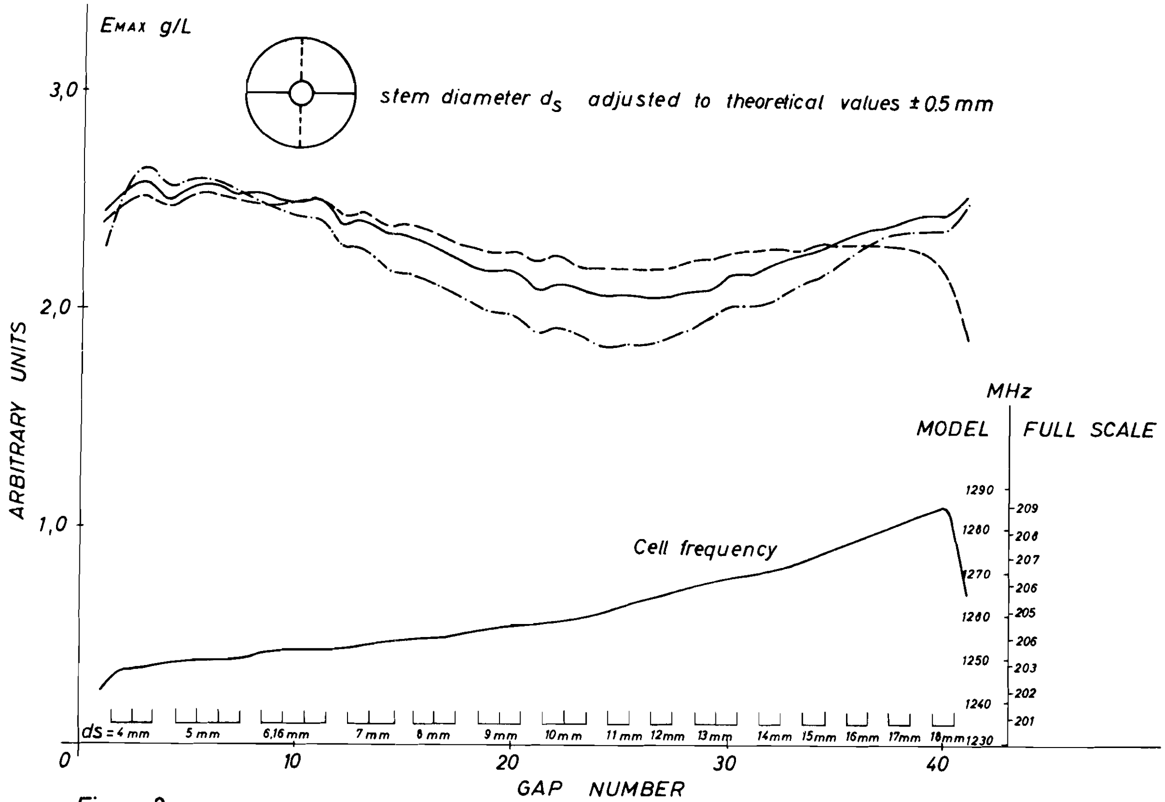

Figure 2a

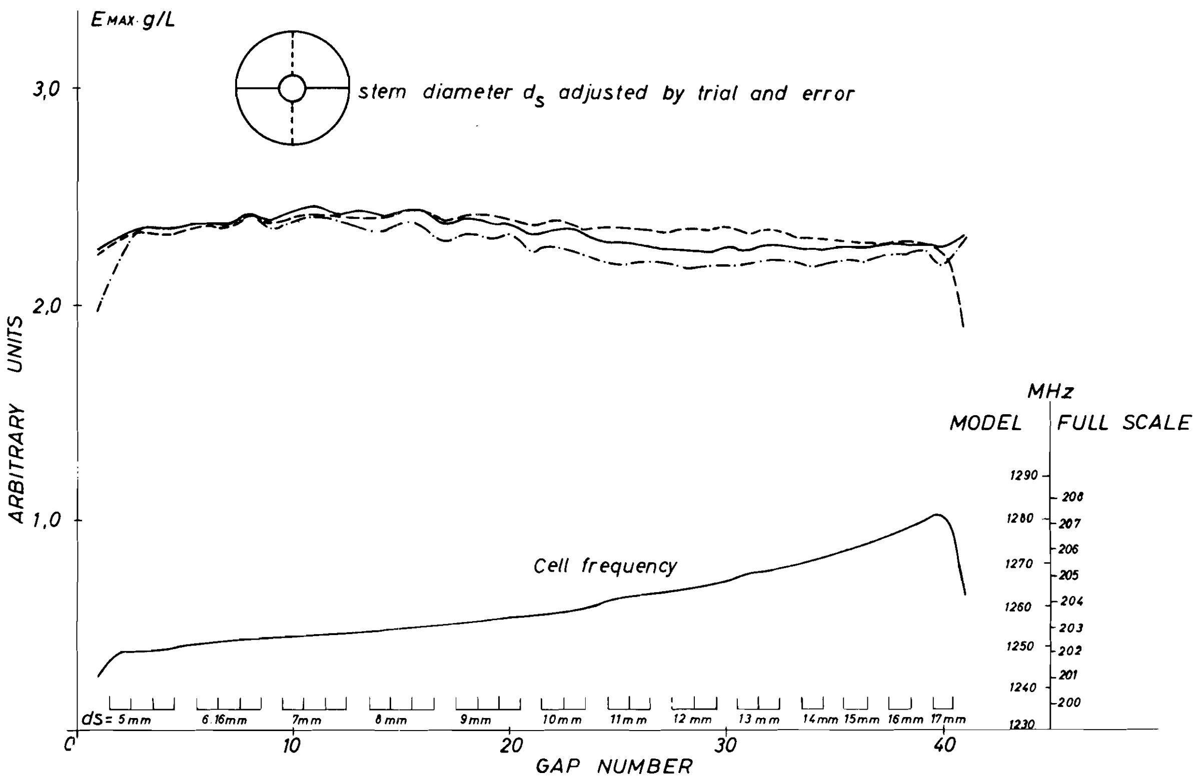

Figure 2b

Figure 2 - Axial peak field and resonant frequency of the individual cells in the Alvarez 0 made, for tank 2 model.
Crossbar structure, compensated locally.

___________ without perturbation

----------- with a tank perturbation of - 2 MHz in cell 41

-.-.-.-.-.- with a tank perturbation of - 2 MHz in cell 1

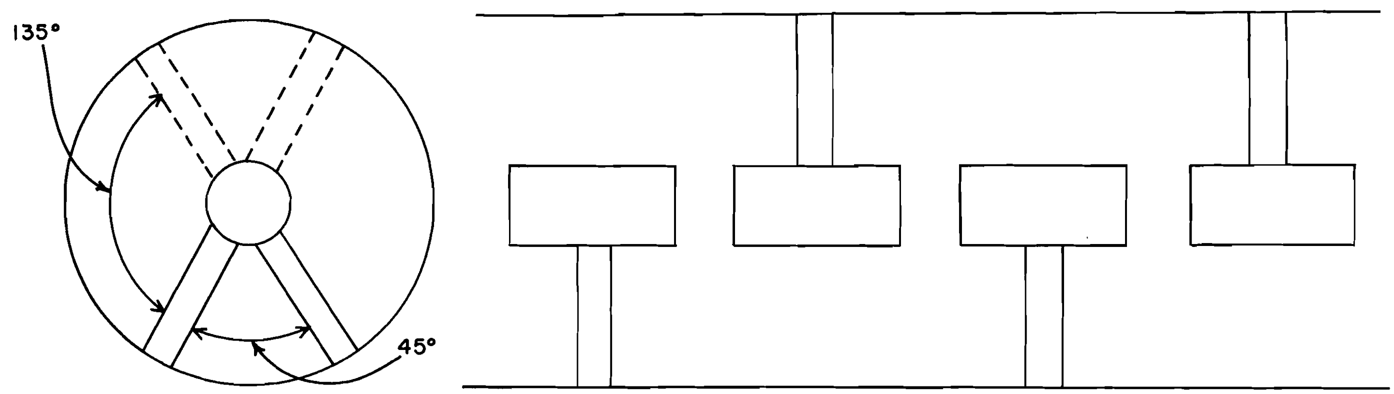

Figure 3 - 2 stems $\phi = 45^{\circ}$, stems on successive drift tubes rotated 180°.

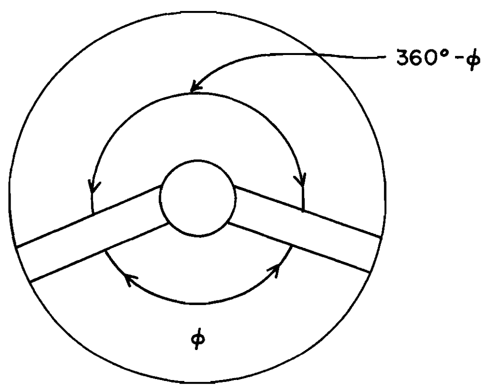

Figure 4 - All stems parallel $\phi = 135^{\circ}$

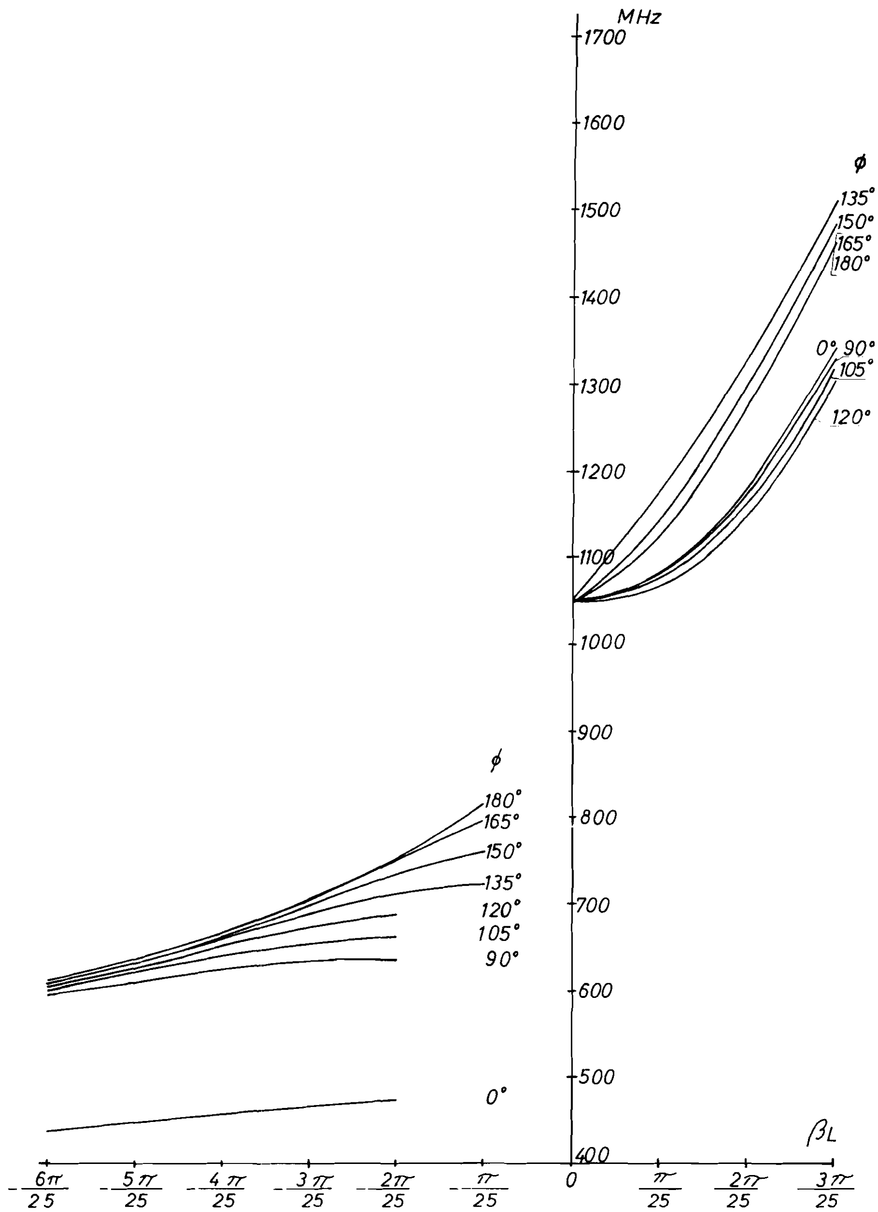

Figure 5 - Dispersion curves for tank 1 model. All stems parallel.

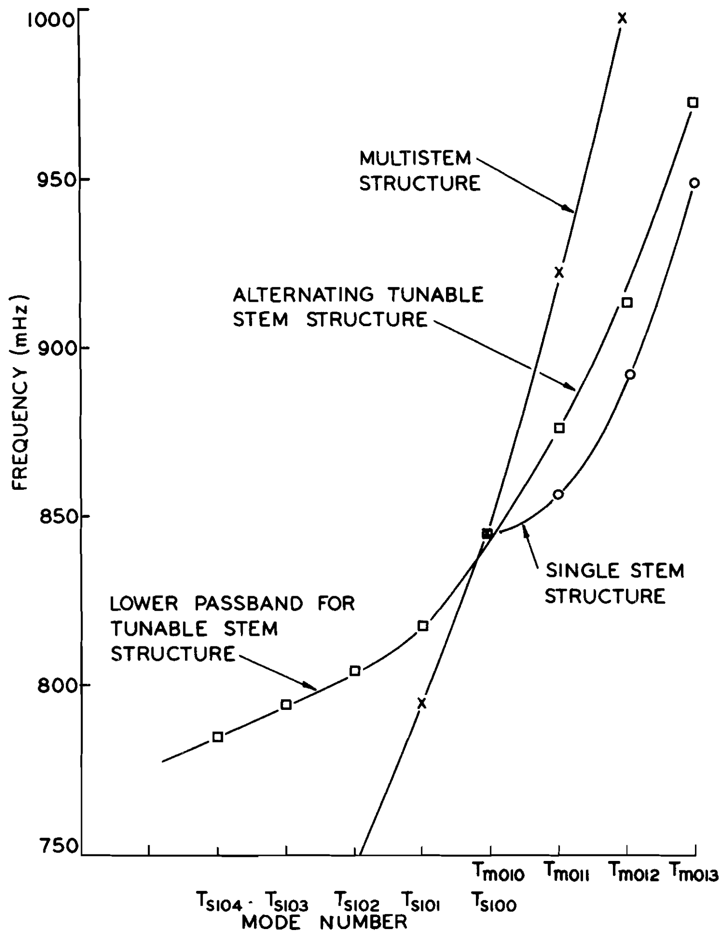

Figure 6 - Dispersion curves for the single-stem, post-coupled (alternating tunable stem structure) and multi-stem structures.

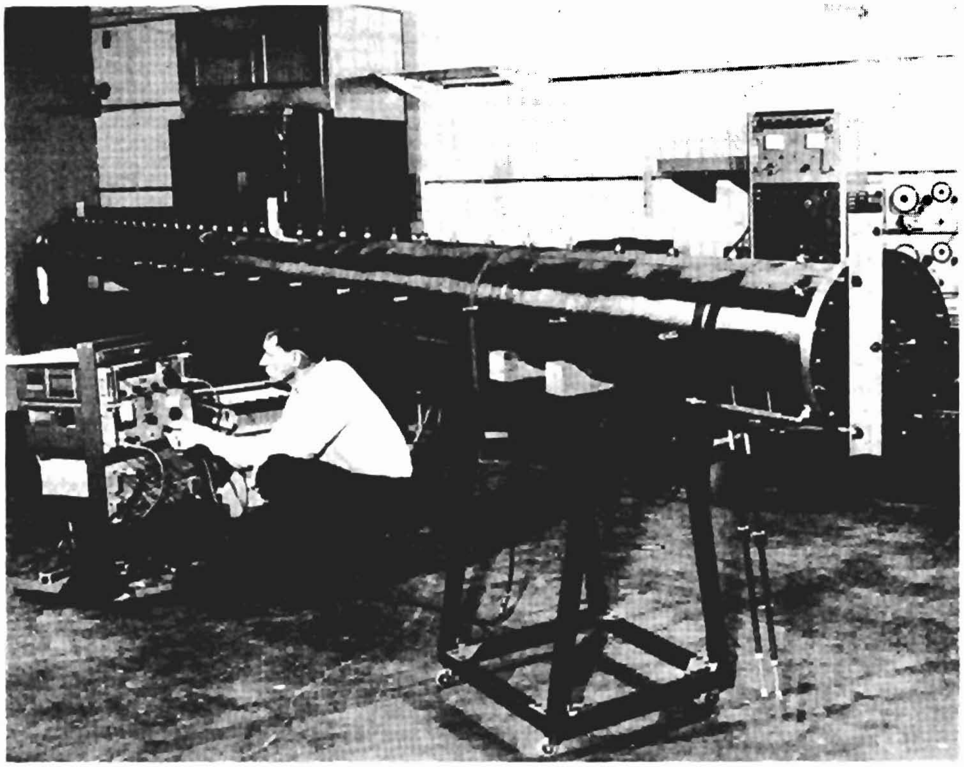

Figure 7 - 500 MHz post-coupled model (10-100 MeV).

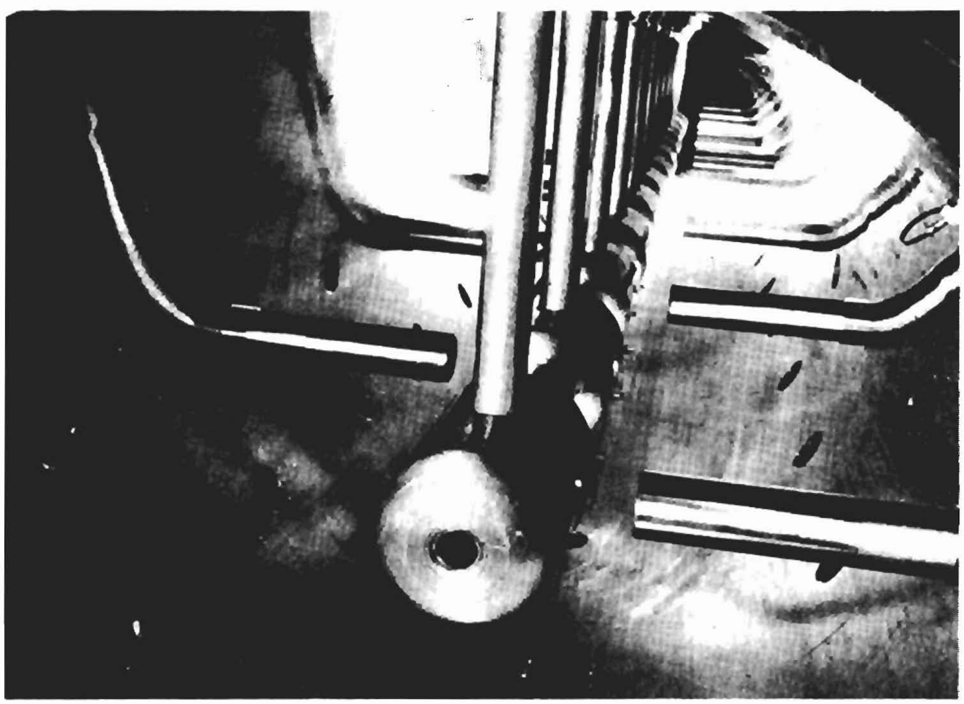

Figure 8 - Single stem post-coupled structure.

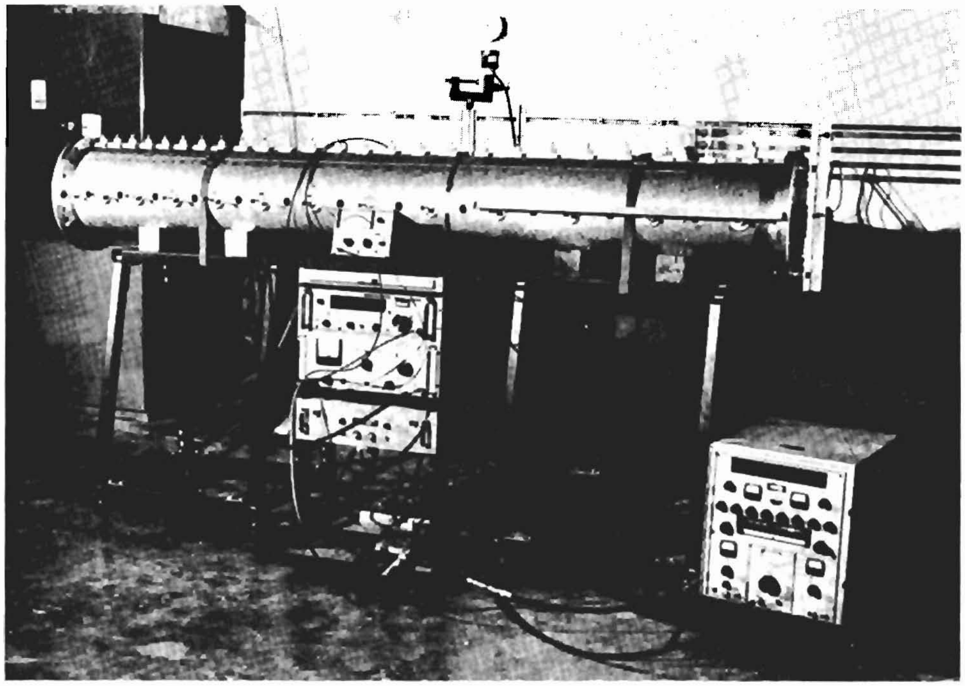

Figure 9 - Post-coupled model (5-30 MeV).

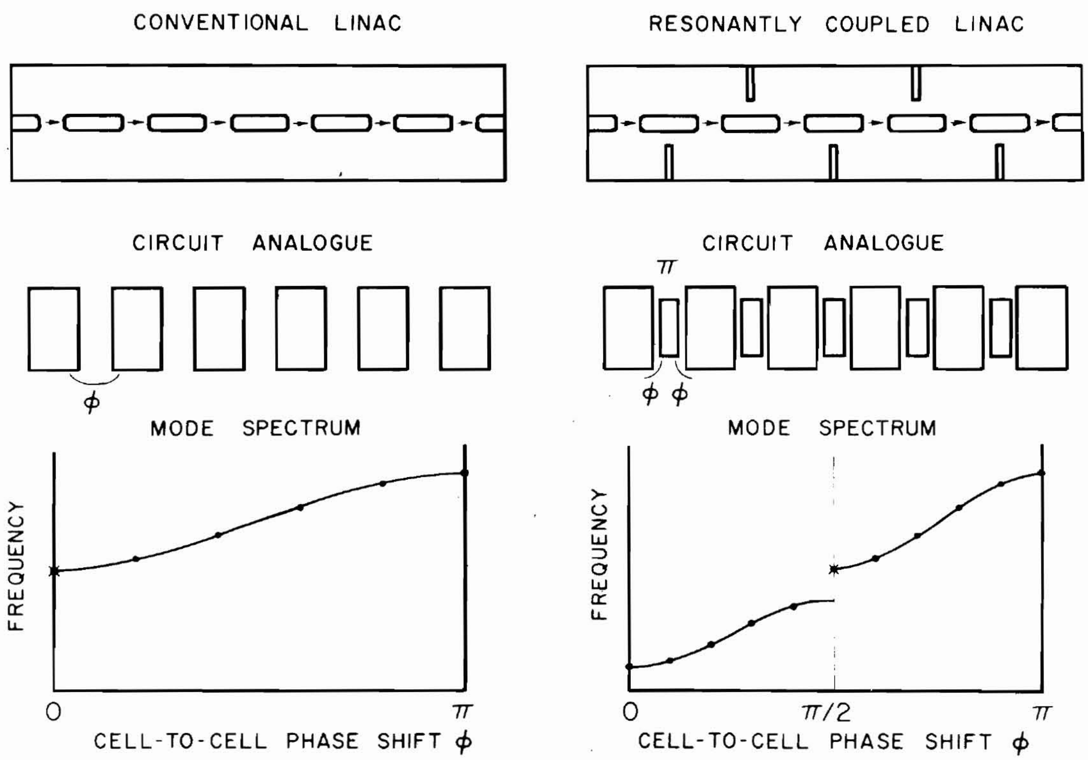

Figure 10- Circuit analog of drift tube linac structures.

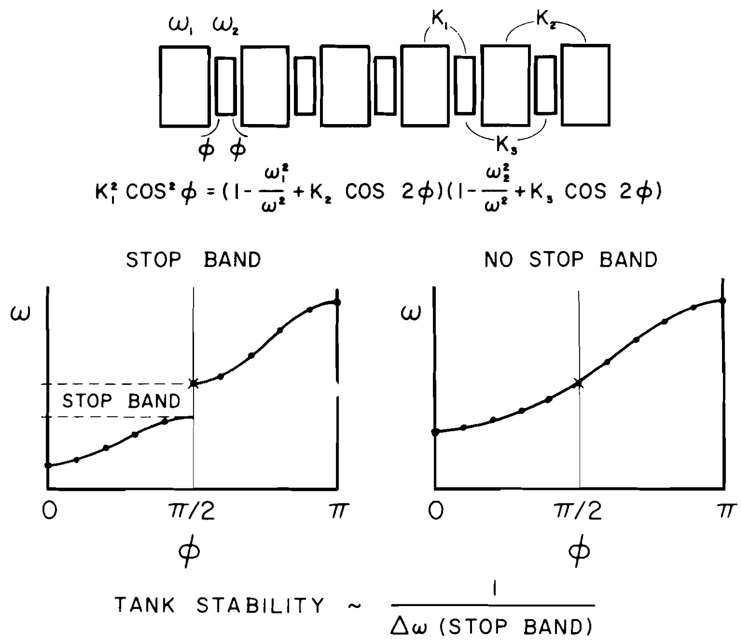

Figure 11- Circuit analog of a post-coupled structure.

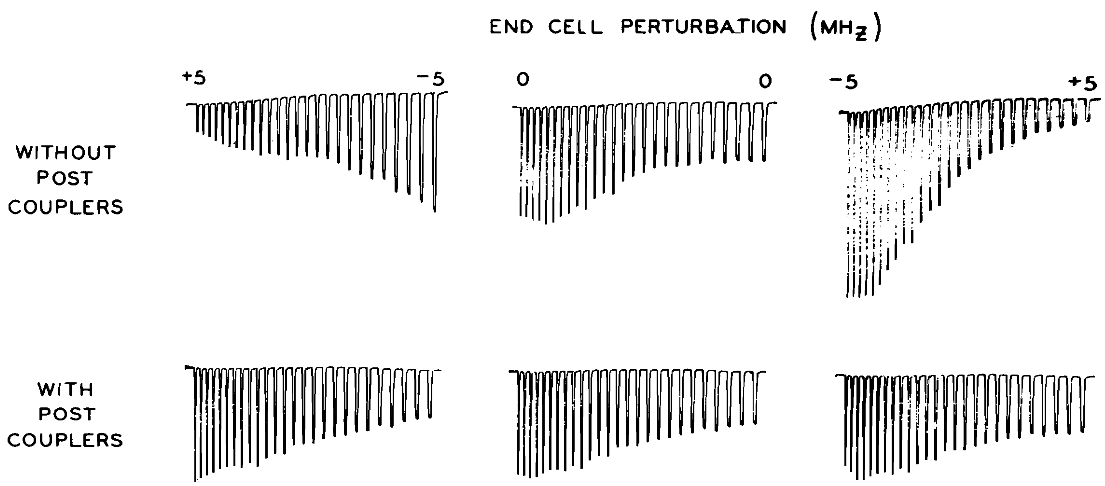

Figure 12- Results of bead perturbation measurements on 5-30 MeV model.

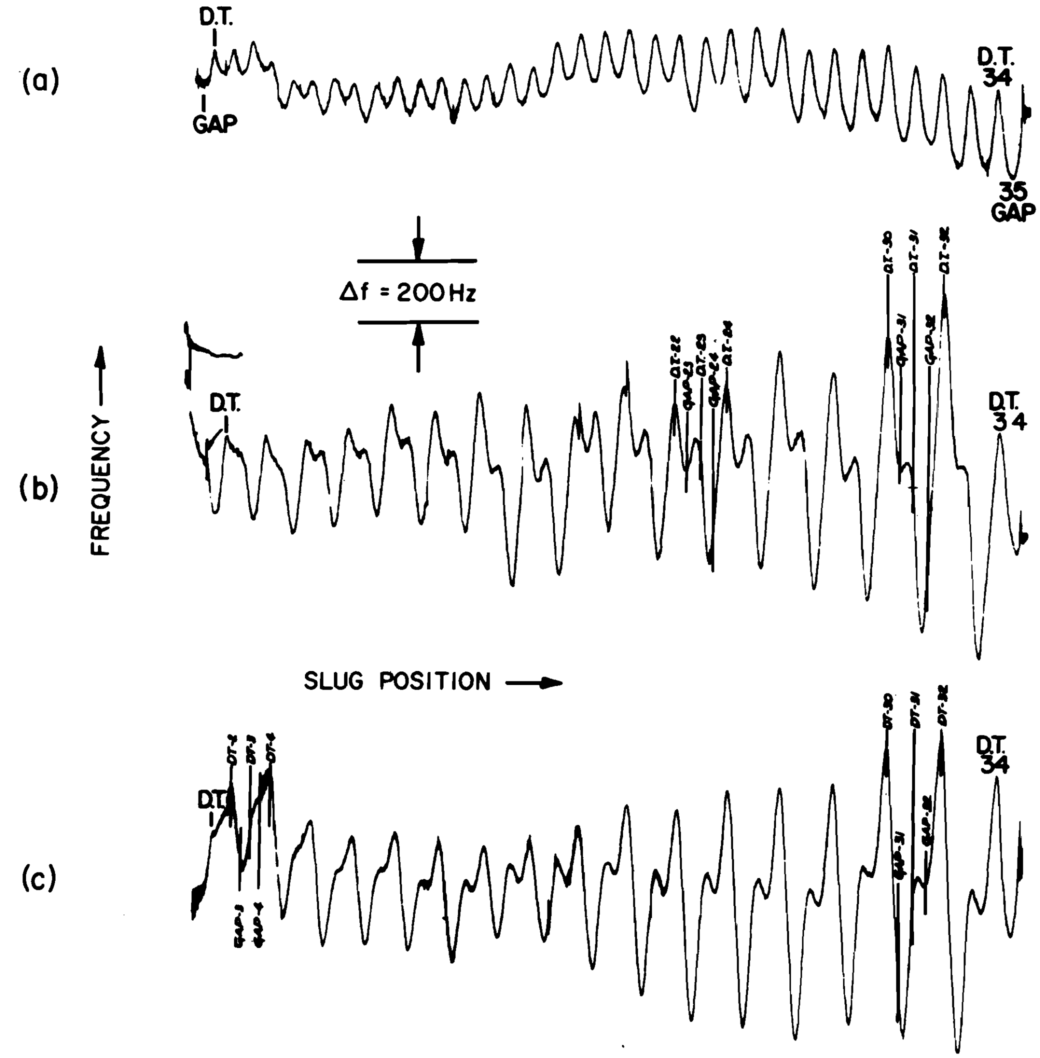

Figure 13- Bead measurements along the wall of a post-coupled structure. (a) unperturbed, (b) positive tuning error at one end of the cavity and negative at the other, (c) position of tuning errors reversed.

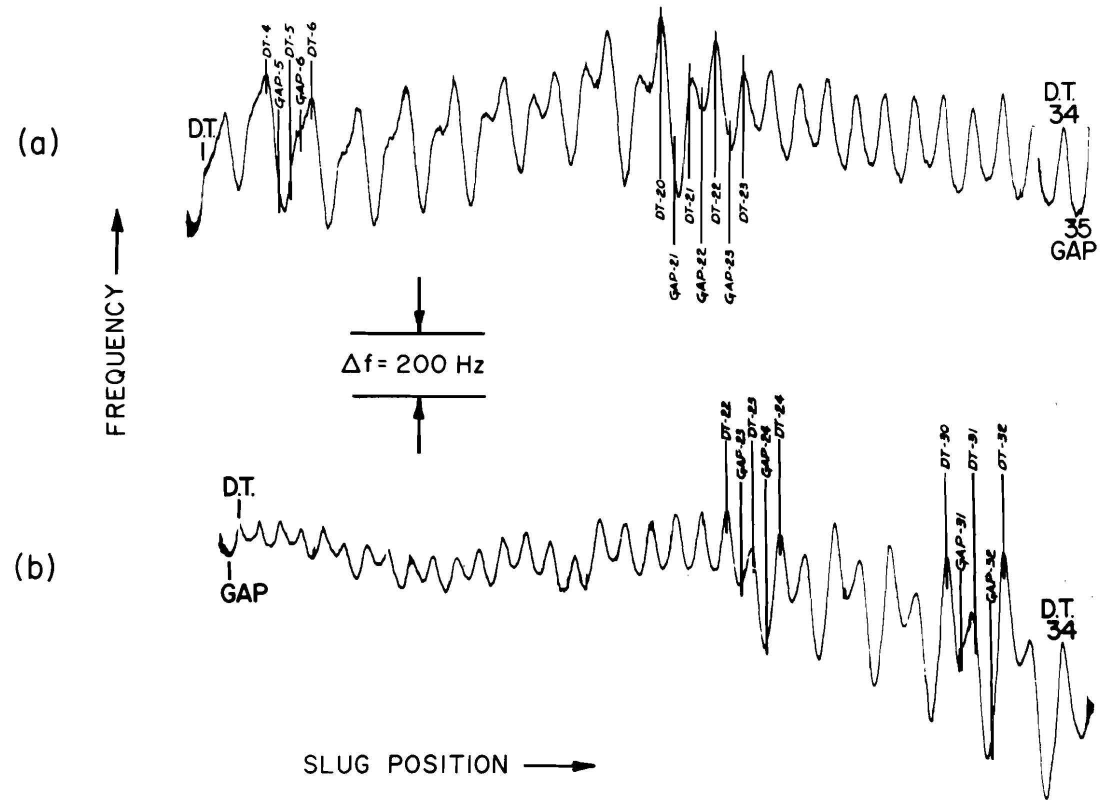

Figure 14- Bead measurements along the wall of a post-coupled structure. (a) tuning errors in the first half of the cavity, (b) tuning errors in the second half.