SUMMARY OF THE 1970 PROTON LINEAR ACCELERATOR CONFERENCE

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The seventh of this series of Linear Accelerator Conferences has been in the tradition of this Conference; it has been interesting, relaxed and informative.

Under the skillful guidance of Don Young and his associates the Conference has run smoothly and efficiently; this was in spite of the fact that the NAL linac is barely coming into operation and is requiring all possible attention from its designers.

There were occasional minor and amusing disruptions. For example, the second speaker at the Conference managed to be promoted to first speaker by simply sitting on the briefcase and slides of the first speaker. Further interest was excited by conversation apparently self-generated by the public address system. To one speaker who had not yet said a word, the loud speaker said "I can't hear you."

We now turn to the more serious side of the Conference.

When one works from Monday to Friday, at the end of the week he usually feels that very little has been accomplished. But when we compare the results presented at this Conference with those recorded in the Proceedings of the 1968 Conference, it becomes evident that there have been many exciting and significant advances in the last two years. There have also been a few setbacks.

A few highlights are as follows:

— In 1968 NAL was setting up a 10-MeV "prototype" linac in a Butler building; now it is in its final location with most of the higher energy tanks in place and, a week ago, the NAL team produced a 139-MeV beam — the highest energy ever produced in a proton linac.

— At Brookhaven in 1968 there was a hole in the ground where there was to be a 200-MeV linac. Now all but one of the tanks are in place and the first 10-MeV tank has produced a proton current of 215 mA — the highest current ever accelerated in a proton linac.

— In the halls at the 1968 Conference there were heated discussions about multiple stems for drift tubes, about tunable post couplers, and about $\pi/2$ modes or doubly periodic structures. Now we know that all work as predicted by their proponents.

— At Los Alamos in 1968 the electron prototype had just been completed; a trench had been dug for the 800-MeV linac. Now the first 5-MeV tank of the machine is in full operation, and much of the remainder of the machine is complete.

— In 1968, Brookhaven and Karlsruhe were working on superconducting lead-plated cavities. There was no one at the Conference from the High Energy Physics Laboratory at Stanford.

Now Schwettman and Fairbank have converted us all to niobium and no linac conference would be complete without a progress report from Stanford.

— At the 1968 Conference Schmelzer reported on the design of the UNILAC without any information about whether the project would be supported or, if so, where it would be located. Now the project is approved and a site has been chosen near Darmstadt.

— Up to the time of the last Conference there had been much talk about the helix accelerating system but no one had built one. Now protons have been accelerated in helix systems at Frankfurt, and people are talking about superconducting helices.

— At the last Conference, Sessler reported on six months of preliminary work at LRL on the electron ring accelerator. Now electron rings have been compressed at LRL and have been compressed and extracted at Dubna.

— The major setback since 1968 has been the demise of the daring Intense Neutron Generator at Chalk River. Perhaps later it can be revived in a superconducting version.

Now we turn to the various specific fields covered at this Conference. We begin, logically, with ion sources and preinjectors. Here the progress has not been particularly spectacular; rather there has been a consolidation of technology discussed at previous Conferences. It is now clear that beams of high intensity and brightness are obtained when the accelerating column is as short as possible, using electric fields of 30 to 50 kV per centimeter, and when strict attention is paid to maintaining the Pierce geometry. These are the features of the high performance preinjectors described at this Conference by the experts from NAL, BNL, CERN, and other laboratories.

As we heard on Monday during the beam diagnostics session, also this morning and from time to time during the week, the arts of transporting beams and analyzing their properties, often without disturbing the beam, have become incredibly sophisticated. It is indeed a backward laboratory nowadays that can't push a button and display on a scope a three-dimensional picture of emittance and intensity. I never cease to be amazed and delighted at the performance of these gadgets, especially when I remember the ponderous methods we used only ten years ago.

Turning to the linac itself, we note, from Don Young's and Curt Owen's papers that the discussions of two years ago about the possibility of perturbing transverse field components with the post-coupled rf structure have been effectively settled by acceleration of a proton beam through several of NAL's post-coupled tanks. If the transverse components are there, they don't prevent the NAL team from accelerating protons.

In the field of rf structures, the most exciting development has been in Frankfurt, where, as I have mentioned, Klein and his colleagues have at last solved the problems associated with the helix structure. The question about how to include quadrupole focusing has been answered by making the helix in sections a meter or so long and alternating helix sections and quadrupoles. The Frankfurt group has not been satisfied merely to demonstrate the rf properties of the helix system; they have accelerated protons in a helix accelerator from 1.5 MeV to 2.5 MeV.

The helix structure seems very well suited to reductions in size of the low-frequency accelerating structures necessary at very low ion velocities and so should be important in accelerators for heavy ions. This has been recognized at the new German heavy ion accelerator center which will sponsor a helix post-accelerator for the Emperor tandem Van de Graaff accelerator in Heidelberg.

The helix naturally leads us into discussion of superconducting rf systems. Superconducting niobium helix structures have caught the fancy of many linac groups including the one in Frankfurt, as we have heard in the interesting papers of Klein and Vetter. In this country they are under study at Stanford, CalTech, Oak Ridge and Argonne.

The superconducting helix presents some very sticky problems. For example, it has a very high mechanical Q; in the presence of high rf fields coupling exists between the electrical and magnetic fields and the mechanical vibration modes, and the helix is easily set into persistent vibrations that detune it completely. Since it operates at about 2°K there is a shortage of viscous damping media.

These problems were discussed at a luncheon session yesterday and a few ideas were suggested. Perhaps the helix can be constructed of tubing filled with a porous plastic or with sand to provide damping of mechanical motions. We can hope that a solution to this problem will be presented at the next Linac Conference.

The picture is brighter with respect to the more conventional superconducting structures that are to be used in the Stanford High Energy Physics Laboratory. Alan Schwettman described the latest progress of this pioneering group, a very small group that has led the world in introducing this technology. The observed phenomena and the limiting mechanisms are still rather incompletely understood but the Stanford group has made so many breakthroughs in the past two years that there is good reason for continued optimism.

As we heard from Greg Loew, SLAC has been convinced of the advantages of superconducting cavities. Recently SLAC produced a very impressive design study describing a SLAC converted to superconducting cavities and yielding a 100-GeV electron beam. There are even proposals for recirculating the beam through the linac to obtain 200, 300, 400 or who knows how many GeV.

The heavy ion front has been a busy one during the past few years, sparked by the prediction of islands of stability far beyond the end of the periodic table. This has set a design requirement that an accelerator must yield ions of all masses up to uranium, having energy of 6 to 8 MeV per nucleon. More than twenty proposals have been submitted to the Washington agencies, mostly suggesting tandem-cyclotron combinations. But the only one that has been approved is Bob Main's Super Hilac that he described yesterday. In exploitation of the new energy and mass range he will be joined by the groups at Orsay, Darmstadt and Heidelberg, whose plans also were described yesterday. A later powerful competitor may well be the electron ring accelerator. Even now the electron ring heavy ion accelerator has proponents who describe it as the truly outstanding machine in this field.

There have been a number of steps forward on the theoretical front. There has been nothing as spectacular as Rena Chasman's revelation in 1968 of her analysis in six-dimensional phase space of the effects of space charge in the early stages of linac acceleration, but much further work has been done along these lines. On Monday Promé pointed to the significance of the sum of the two transverse admittances and the longitudinal admittance. According to him this sum tends to be constant, or to be linearly dependent on beam current.

In a round table discussion on operating machines on Tuesday there was a cogent discussion about the question as to whether we now have agreement between theory and experiment. Gluckstern thinks that general agreement has been demonstrated. Colin Taylor is doubtful. Gluckstern pointed with pride to the observation at NAL that the normalized emittance does not increase between 10 MeV and 66 MeV; Chasman and Gluckstern predicted this two years ago.

Unfortunately Gluckstern yesterday slightly undermined our simple faith in his predictions when he described an analysis of the oscillation modes of a Kapchinsky-Vladimirsky beam. After covering 60 pages of mathematical analysis he resorted to the computer to find the real answer. He traced 500 particles and derived an answer indicating considerable beam growth. Then, just to check, he traced 1000 particles — to his distress the beam growth was less. With 2500 particles the growth was becoming quite tolerable. If he traced a million particles would there have been no growth at all?

In conclusion I should like to comment about the prospects for future Linac Conferences. During this Conference I have heard a few voices of doom asking "When the NAL and BNL 200-MeV machines are finished and when the Los Alamos meson factory is operating, what will there be left to discuss at Linac Conferences?"

There are, I think several answers to that question. First, I note that when the first two of these Conferences were held at Brookhaven, none of these machines had even been approved; some had not even been proposed. But there seemed to be lots to talk about.

Second, although work on these three machines has, to some extent, dominated this Conference, there have also been many new items of great and presumably continuing interest — to emphasize a few already mentioned there is the advent of superconductivity in our field both in cavities and in helices. The helix is of great interest even at room temperature. The acceleration of heavy ions is presently and for some time will be very exciting. We have tacitly admitted, by inviting Denis Keefe to speak, that the electron ring accelerator is a variety of linac. And, as we approach space-charge limits much remains to be done and discussed on the theory of collective effects. Finally, an enormous amount of electrical and mechanical engineering remains to be done to make linacs cheaper, more versatile and more reliable. In summary, the linear accelerator provides unique and important applications in the field of particle acceleration. I believe that its future will continue to be as brilliant as its past.