

UNIVERSITY OF CALIFORNIA

Lawrence Radiation Laboratory  
Berkeley 4, California

DURANT HOTEL,  
BERKELEY

Sat. 8th July.

Dear Gerry,

Everything is going quite well. The only thing of note on the way over was a thunderstorm around Chicago. We flew well above it and the plane rocked a bit but the effects below us were quite something. The conditions at work are a bit hectic. Three of us share an office with the door wide open and it is a bit like working in a railway waiting room. Apart from K. Johnsen and me most of the others are nuclear physicists and the emphasis at Berkeley is more concerned with the nuclear physics case for the machine. Chew is much in evidence in all of these discussions and is the only theorist here who seems to take an active part in the possible nuclear physics programme. There are lectures every morning - I have been to one by Kerth and Keefe on experimental methods, one by Ticho on beam intensities and one by Dombey on neutrinos. Nothing specific has come out of this except that Kerth estimates Cerenkov counters of 100-300 feet long as the only likely method of particle identification for anything other than  $\pi$ 's or p's.

So far I have had casual conversations with quite a number of people and have fixed up further meetings (a) with Judd on the 88" cyclotron (b) Wentzel and Lambertson on extraction (this has come on a long way and they are very optimistic that they can get small spot and efficient extraction) and (c) with Bradner, who promises to give me the inside story of bubble chamber physics from the outside (he has left Alvarez and is now with Lofgren). I have also talked to Thresher and Murphy and went round the Bevatron with them. Crowe described an experiment on slow energy  $K^+$  experiment but I expect Godfrey knows about this. I shall include a list of beam equipment with costs which John Wilkins would like to have. Will you ask him if this answers his query.

The one thing that impresses me about the big machine is the concentrated effort that is now going into it. Sands and Walker arrived a couple of days ago and they have been working very hard on their small aperture machine. L. Smith has written a number of good papers on choice of parameters and Brookhaven have done similar estimates. There is general optimism that a 300 GeV machine can be made to work. Injection is still the outstanding problem and it is not clear which of several are best. I had begun to think that the 10 GeV booster was the right thing but I am not so sure now. The linac being suggested above 150 MeV is a travelling wave disc loaded guide with quadrupole lenses between 3 metre sections. Godfrey might like to know this. It would save a lot of structure work if this is possible. I have spent most of this week trying to get a clear picture of what everybody is saying. The Blewett's are coming next week. His estimate for the 1,000 GeV machine is 500 m \$, taking 10 years with total staff of 850.

One point that I have recalled - introduction of argon in the separators with glass electrodes improved breakdown. This was then tried with metal electrodes and showed some improvement.

The Murray 800 MeV  $K^-$  beam is still running - the longest time on record. Most beams are pulled down after a few weeks running time. At any time there are only about 3 beams running on the Bevatron.

Please write if there is anything specific for me to find out.

Best wishes



LAWRENCE RADIATION LABORATORY  
BERKELEY 4, CALIFORNIA

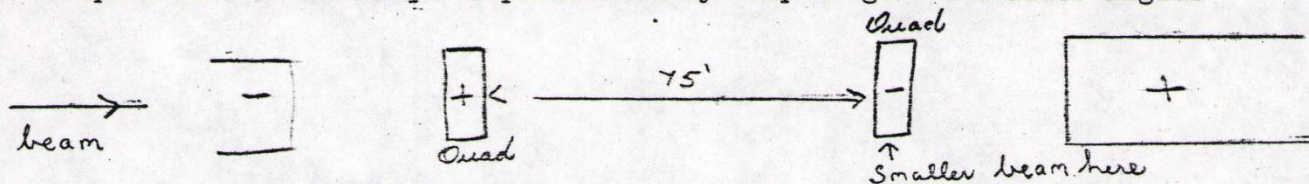
'THE DOORANT'

14th July, 1961

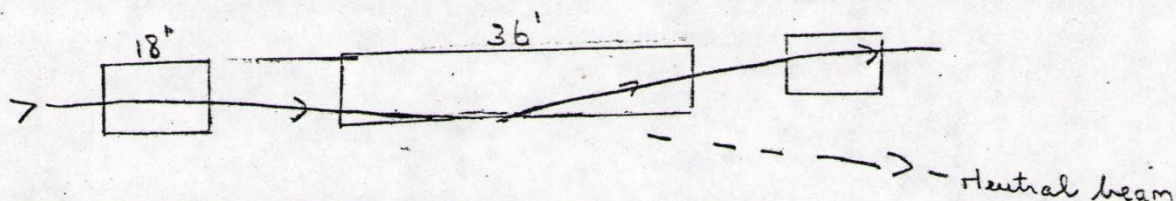
Dear Gerry,

I thought I had better try and send you a weekly newsletter and you can spread the snippets around. I am going to the mountains for the weekend with the Lloyd Smiths and Al Garren. The idea seems to be to camp under the stars but it is all a bit vague. Lloyd refuses to discuss it - he says it is his wife's idea, and Al Garren is picking me up in his car at 7 a.m. tomorrow morning.

(1) Snyder and Courant have suggested a method of putting long straights in the 300 GeV synchrotron. In principle it matches in the same way as the Liverpool idea but uses quadrupoles and may help to get to smaller angles



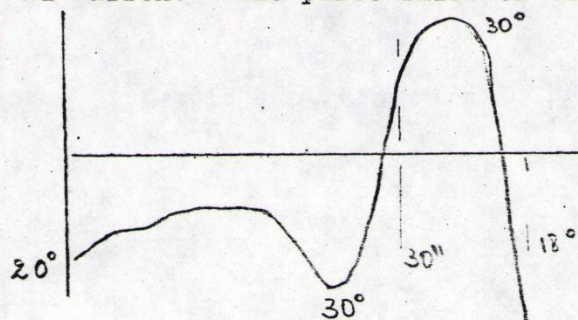
They have also suggested kinking the beam with an 'achromatic' ? bend



It may have some application to the electron machine.

For John Lawson

(2) I wrote to John about the 88" cyclotron. Since then I have talked to Al Garren. The phase shift of the bunch (computed) looks like



(a)  $\nu_R = 1.0$  at 37.6"

(b)  $\nu_z = 2 \nu_R$  occurs at  $\begin{cases} \nu_R = .935 \\ \nu_z = .467 \end{cases}$

at 38.2"

(c) At  $\nu_R = 1.0$  they may have radial blow-up

due to first harmonic and this causes vertical blow-up at  $\nu_z = 2 \nu_R$ . Roughly he estimates that the vertical blow-up could be 3 x horizontal amplitude before it is limited by non-linear coupling. He thinks he has to keep the first harmonic down to 2 gauss.

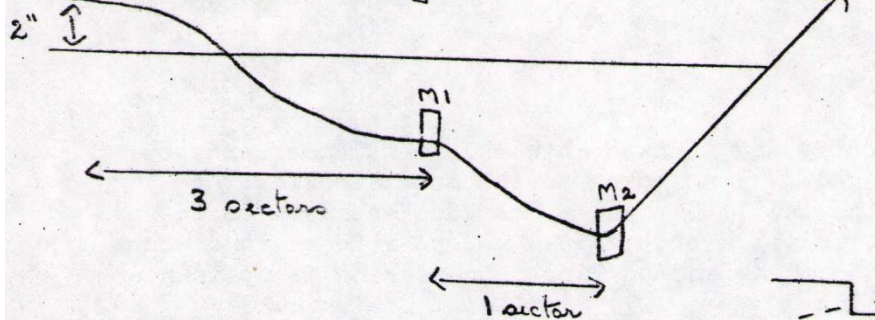
(c)  $\nu_z = .1$  up to about 37" and then increases to 0.5 at 38" and 1.0, at 39" so there are lots of possible resonances near his extractor at 39", but he thinks he can pull 10% out.

(3) Thornton said that the electronics for the beam lengthener is too complicated and is being redesigned. He could give John the circuit that Kerns used but does not think it is worth while. The duty cycle can be as much as 80%.



(4) Beam Extraction from Bevatron: [ For Marshal ]

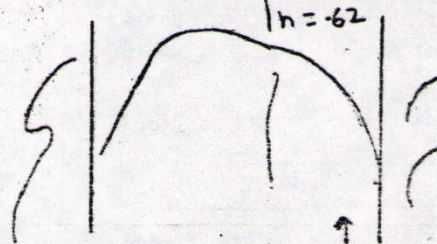
5 mev loss in target



The bending magnets are backed by separate quads. Final adjustment by varying target position and quad. strengths.

they have not designed a quad transport system yet.. They estimate a beam size of

The beam comes out at very small angle but



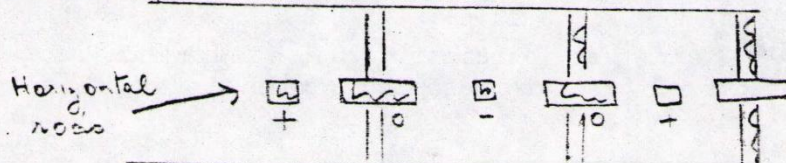
a few inches with 8 n. radians divergence. Their fringe field is as bad as ours if not worse but they seem very hopeful that they will get  $1/8'' - 1/4''$  spots. The magnets are in fixed positions at present and they are almost ready to try it. I shall talk to Lambertson next week. The above came from Al Garren.

(5) Bruce Cork has an ion source working for the new injector giving 70 m. amps. He is sending reports to Leo and Godfrey.

(6) Aron and Hitchcock quote the following for superconducting magnets.  
Nb-Zr At 4.2°K.

89 K gauss	- No superconductivity	98% cold work reduction
80 K gauss	- $2.5 \times 10^2$ amps/cm <sup>2</sup>	Was not affected by low
70 K gauss	- $2.5 \times 10^4$ amps/cm <sup>2</sup>	cycle modulation
30 K gauss	- $9 \times 10^4$ amps/cm <sup>2</sup>	

(7) I went to Stanford on Wednesday with K. Johnsen and John Blewett. Panofsky has now resigned his directorship of the lab and has joined the M-project full time - so has Ginzton but has to sort himself out with Varians. They are now all but ready to get approval and think this will come in October. I talked to Chaw about the crossed wire structure and discovered that someone in Brooklyn Poly is proposing to use it for a linac. They call it Jungle Gym and it has a very wide passband. I think it is worth looking at for for the P.L.A. at  $\beta \approx 0.5$  but with one rod bars occurs. The  $\pi/2$  - Mode  $\pi$ -Mode for horizontal for  $C = 1/2$  almost exactly. They had no reliable shunt impedance measurements. I think the way to operate it is



I intend to go back and talk to Chaw. I may also visit Brooklyn Poly.

This is  $\pi$ -Mode for the horizontal bars and  $\pi/2$  when the vertical rods are put in. There should be no current on the vertical rods (or little) at the operating frequency with standing wave. But they (a) open up the dispersion curve and (b) the drift tube cuts down transit time. The drift tubes could carry quadrupoles. I have now checked calculations for 3 metre lengths of guide and it looks as if we can put (+ -) lens between sections and



get away from quads in drift tubes. This looks alright for 200 MeV upwards. We could in fact use a disc loaded waveguide without drift tubes.

(8) I don't think it is worth relating all the details of the discussions on the 300 GeV machine. Most of the lectures are on nuclear physics but nothing of any consequence has appeared. There is an undoubted enthusiasm for a machine like this and it is certain that something definite will come out of the summer study. J. Blewett seems a bit more realistic on the possibility of joining forces with the Russians than I had thought but he is carrying out his part of the bargain and will present a set of design parameters at their joint meeting in September. He said that all his attempts to correspond with them have failed so he does not know if they will produce proposals. Well off to the mountains!

Bill

P.S. I spent an enjoyable evening with John Thresher and Paul Murphy



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4th August, 1961

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Dear Bill,

Many thanks for the spate of letters. I have had them copied and circulated. You have certainly fulfilled your promise to let us know what is going on out there.

It seems that people are serious about the big accelerator; I suspect that feasibility rather than necessity is the driving force, but I do not quarrel with this - the brute will surely be useful and somebody has to build it.

Our big news, which you may have heard already, is that the injector gave its first 15 MeV beam on August 1st - exactly a month after it was first buttoned up as a complete machine. There were no serious troubles - just the usual period of multipactoring, and pinpricks such as blown fuses and faults in quadrupole power supplies. No measurements yet - but the beam comes at the expected RF level, and shows the expected sensitivity to injector voltage. Leo and his men worked very hard, and the customary barrel of beer was opened yesterday.

I look forward to meeting you in New York on 5th September (at the Wellington, in the evening).

Best wishes,

Yours,