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DISCUSSION

THE 'HEAD-TAIL' INSTABILITY

B. Zotter: All of the machines mentioned behave well, but the ISR does not. We have a positive chromaticity, and we see what we think are head-tail oscillations. The behavior is also inconsistent in that we increase the chromaticity to get enough damping for the transverse oscillations of the unbunched beam and this also seems to help in stabilizing the single bunched oscillations.

A. M. Sessler: I would like to ask about the magnitude of the driving term. As I recall, there was a discrepancy between the value of the driving term one might reasonably expect and the value which is required to fit the data. Is that still the situation?

J. M. Paterson: I believe this is true. For example, at ADONE Pellegrini calculated this term for resistive wall, cavities, and plates. In all cases he had difficulty explaining the rise time of the instability; he came closest with resonators, but he was still too low.

A. M. Sessler: So, that's a real problem. For the first year or two you can say that the orders of magnitude will get sorted out, but after a few years maybe you have to go back and think about it more seriously.

Paterson: It is rather surprising, and it didn't dawn on me until very recently, and I used to think that CEA had a very low threshold, but if you write it in particles per centimeter and include one power of gamma for scaling with energy, then all thresholds are within one order of magnitude. For most theories one considers this good agreement, and they are all very different machines in terms of their detailed characteristics.

F. Amman: The type of force should not influence the sign of the stability criterion. If negative chromaticity gives instability for the zero mode, then positive chromaticity should still give instability for the higher modes.

Sessler: That's if you have no Landau damping?

Amman: Yes, but the sign of the criterion would be the same.

Sessler: You could have it unstable for one sign; however, with Landau damping it may still be stable.

Ruggiero (NAL): In the head-tail theory one takes an average of the synchrotron oscillations over the bunch. Now it is important what kind of field you have, whether you have a rather smooth field or a fast field, for probably you would have different stability criteria.

Sessler: Are you saying that if the wake field oscillates rapidly, then the stability criterion of the sign of the chromaticity can change for the higher modes?

Ruggiero: I don't know about the sign of the chromaticity for stability, but we only see the zero mode at NAL booster. We know what drives the instability is the laminations around the beam, and they look like a string of resonators at about 50 MHz, which is a short wavelength compared to the bunch length.

Sessler: That's only a theory. You don't know what really is driving the instability.

Ruggiero: Yes, that's right.

Morton: Do you pick up any coherent signal near the 50 MHz?

Ruggiero: No, we cannot.

Sessler: The fact that the threshold is about the same as all of the other machines which don't have laminations makes one very skeptical.

Amman: I would like to add to Zotter's comment. One must be very careful in applying theory when the chromaticity is large. The theory is a linearized theory and is only good when the chromaticity divided by the momentum compaction is small, and is also good only for small Landau damping, which has been introduced in an inconsistent way.

Zotter: It still does not explain the ISR results.

Sessler: Is the ISR in a different range, i.e., is the chromaticity over the momentum compaction large?

Zotter: For the ISR, the phase shift is large, but we did adjust the theory to take this into account.

Sessler: I know that you extended the theory. What did you find?

Zotter: We found that the higher-order modes would only have much higher thresholds.

Morton: Isn't it true that what Paterson has presented here is only growth rates when he talks about factors of four between the zero and first higher mode? There also is a real tune shift that must be included for determining thresholds, and this real tune shift is considerably different between the zero and first higher mode. Again, you would expect very different behavior between different machines.

Paterson: You are right that I was speaking about growth rates, but in Pellegrini's calculation, where he tried to take into account Landau damping with different model fields, the thresholds were only a factor of four different.

Sessler: Has anyone done the theory for higher-order modes with a wake field that drops off fast compared to the length of a bunch?

Paterson: I don't believe so.

Sessler: Therefore, it is possible that such a theoretical model which assumes that what derives the instability in all these different machines is a very high-frequency effect, might predict that the thresholds would be very different for all higher-order modes. I think one might still have great difficulty fitting the order of magnitude of the zero-mode threshold.

Paterson: That's why I think one must come up with a new source for the driving fields, not cavities or plates. Perhaps every change in vacuum chamber dimension may be a low-Q high frquency resonator.