nal instability, while some preliminary results on low β tests, with higher values of Δ_b, might be interpreted as due to a longitudinal limitation.

Eqs. (1) and (4) show that the energy dependence of the maximum current per bunch is the same for the two beam-beam limits, transverse and longitudinal, in the assumption that they are defined by a value of ξ_m and Δ_m independent from energy and that each beam transverse dimension is proportional to γ; the maximum current per bunch is proportional to γ^3 and the luminosity turns out to be proportional to γ^4.

A last remark on the space charge compensation method proposed by the Orsay group[10]; in the crossing of four beams (two positive and two negative) of equal current and charge distribution there are no beam-beam forces. The critical point of the method is the stability of the system of four beams, besides the practical question of their equality; at very high transverse density (corresponding to $\xi > 1$ for a single beam) the system is unstable for coherent motion[11], but, as far as I know, there are no quantitative evaluations of the transverse density at the stability limit.

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DISCUSSION

BEAM-BEAM LIMITS

E. D. Courant: One thing that puzzles me is why this linear tune shift should have anything to do with the beam-beam limit, because the blow-up is a nonlinear effect and the answer is presumably that the nonlinear components of the force are roughly proportional to the linear components of the beam.

A. M. Sessler: It is not only rough; it is exact.

F. Amman: Yes, it is exact because usually one defines the limit when the beam shape is not changed too much.

Sessler: That is, there is only one parameter that characterizes the problem.

Courant: Well, I would like to propose a second one. From all the ideas we have about the mechanism of the nonlinear blow-up it must depend upon the Fourier spectrum of the nonlinear excitations. Therefore, it should be expected that the higher-order harmonics would go down if the length of the interaction region is comparable to the β at the interaction region, and therefore the stochastic limit on the tolerable value of the linear Q shift should increase if the interaction region is more diffuse and is longer, compared to the β at the interaction region.

P. L. Morton: Would you have a similar conclusion if the number of interaction regions were changed, which certainly changes the number of resonances that are available for this nonlinear blow-up?

J. M. Paterson: We have very limited data on that point, so let me give you the observations. At SPEAR we did one quick experiment where, instead of having two interacting beams, we separated the beams in one interaction region and just observed the effects while we were running near the beam-beam limit. Whenever we separated the beams in one interaction region the beams were not quite lost but they blew up to very large dimensions and were on the verge of being lost, and the luminosity went down. We have not done more systematic measurements but it is certainly indicated that, for a periodicity of one, the beam-beam effect appears to be much larger than with the normal periodicity of two. I might point out that we should learn a lot from SPEAR about this periodicity question in the near future, because when we start up again there is a very large object known as the magnetic detector which, if we don't do something about it, makes the SPEAR periodicity one. We do plan to compensate for this effect with the corrections available and one should be able to change the periodicity effects by small amounts rather than just turning it on and off at one interaction region. It should be interesting to see the effects.

Morton: I would like to ask Amman a question. As I understand it, you use the mechanism of Arnold diffusion to get the particles' amplitude up to the stochastic limit for both protons and electrons. Now, the electrons have a radiation damping, which means that you need much more Arnold diffusion for the electrons. This mechanism appears to be in conflict with the results of the previous paper by Wiedemann which were that you shift the particles' linear tune to a single resonance which carries the amplitude up to the stochastic limit. If you use this mechanism instead of Arnold diffusion then, unlike Arnold diffusion, you would have the same beam-beam limit for protons and electrons.

Amman: My point is that there is a stochastic limit. The ADONE results with the two crossings instead of six (in my opinion) do not show a definite limit as given by one resonance. Then we have the energy dependence of ξ in ADONE, and I am not talking of Arnold diffusion in that case. What I am saying is that close to the stochastic limit nobody knows what the diffusion coefficient is. The coefficient as

calculated by Chirikov is valid only far from the stochastic limit. Our results are compatible with a diffusion coefficient proportional to ξ^{-2} which is completely different from the Arnold diffusion coefficient that is an exponential. Then Keil's computation on Arnold diffusion far from the stochastic limit in order to have a diffusion coefficient of the order of one day. Again, this is in two dimensions, and in going to three dimensions there may be other diffusion processes.

B. Zotter: There were some preliminary experiments on the ISR to see if Arnold diffusion explained the enhanced beam loss which we now believe is due to electron-proton coupling. The experiment was to neutralize several pockets rather than one. The results of this experiment indicated that this enhanced beam loss was not due to Arnold diffusion.

Morton: I think that it is very important what the real mechanism is because, according to Amman and Keil, the beam-beam limit is down by 1/10 due to Arnold diffusion for protons.

Sessler: There is a real principle involved here which is extremely important. What Wiedermann is saying or suggesting is that you use the old Amman-Ritson formula for the Q shift except that you don't take the shift to the integral resonance but only to the nearest important resonance. That's one theory. The other theory is much more sophisticated, and may be wrong, and that says that nonlinearity dominates the problem and, of course, if you are stupid enough to sit near a strong machine resonance then of course you will have trouble. But if you operate the machine intelligently, far from such a resonance, and then the great body of theory says you are not able toshift all the way over to the nearest strong resonance, but long before that you hit a stochasticity limit. That is, at a beam intensity much less than that required to shift to this resonance you get rapid diffusion. These are quite different pictures and we need to find out which is really true, in order to extrapolate to new machines -- especially proton storage rings.

Zotter: Don't you expect that SPEAR, with a tune of 5.1, is far enough away from 5.25 for the Arnold diffusion theory to apply?

Morton: Yes, and the data indicate that one can go all of the way to 5.25.

Sessler: But the data may not indicate that.

Morton: It appears that we will not agree here on the criteria for the beam-beam for protons to be used in ISABELLE and PEP designs. So, in the remaining few minutes I would like to ask people to direct their comments to the types of experiments or studies that are necessary to settle this question.

Zotter: I would like to comment that we have an experiment by Keil coming up that uses a nonlinear lens to excite nonlinear forces very close to the beam in order to settle the question of Arnold diffusion.

Sessler: That will be for an unbunched beam, and of course one will want to do that for a bunched beam. Furthermore, there will always be some people who will say that not all nonlinear forces are the same and that a magnet makes different nonlinear forces from a beam. So it would be interesting to have some real beam experiments.

Zotter: Well, we also hope to eventually reach the beam-beam limit with the ISR.

A. van Steenbergen (BNL): In the SPEAR experiment when you go from two-fold to one-fold periodicity, do you know how you change the parameter ξ ?

Paterson: No, we don't change ξ per interaction region directly. Let me put this measurement in perspective. This experiment was done in the last two hours on the last day of the January cycle and, given another day to think about it, we might have done different measurements. The observation is quite simple: All we did was separate the beam electrostatically in one interaction region and turn on and off the separation plates, and observe the beam behavior. When the beams were separated in one interaction region, the beams blew up. When they were brought into collision at both interaction regions again the beam size returned to normal.

Amman: The interpretation of this result depends upon the numbers, because when you have only one crossing region the relationship between the ξ and the ΔQ changes. So, if the real ΔQ is the good parameter the ΔQ is too high, and one must know the numbers for interpretation.

Amman: Anyway, in ADONE we don't observe any of these effects. If we have only two crossing regions, the ΔQ allowed per interaction region is only slightly higher than the ΔQ allowed per interaction region with 6 crossings, but only slightly higher-about 1.5, certainly not 3 times as high.

Sessler: The last 40 minutes have been devoted to the beam-beam interaction and that is, of course, vital because if the ΔQ differs by a factor of 5 or 10 from what we think, then we will be off by a factor of 25 - 100 in the luminosity of the next generation of storage rings. However, one can take comfort from the fact that there are experiments going on. I don't for a minute believe the theory. From the observations on ISR and SPEAR we will have a solid basis for future designs. I want to point out that there is another phenomenon which is important in the design of these new machines; in particular, the electron-positron machines. That is the bunch length which is a topic we didn't discuss. This is almost of the same importance and this is another area in which we need experiments and hopefully we will get those experiments before we design bigger machines.

Amman: I must make a comment. We need theories because all of the experiments agree. The experiments on ACO, ADONE, and SPEAR all agree, so what we need is theories, not experiments.