Dear Joe,

I am enclosing the review of Karney's paper which I found interesting and important. It certainly deserves publication, with some revision though. To minimize the delay in publishing I am simultaneously forwarding a copy of the review directly to the author. Dima and myself presented similar results for another map at the Kiev Conference last year but we never published it in English.

Recently I spoke to our physicists in Moscow on chaos in the Yang-Mills fields. I found that the difference between the classical integrability (for <u>all</u> initial conditions) and the KAM integrability (for <u>most</u> ones) is not commonly known. In particular, some physicists (and mathematicians) do still use the separatrix splitting as a criterion for nonintegrability. Fermi made a similar mistake many years ago (in 1923!). There are funny recent comments on this his work in his Collected Papers, one due to Segre, which is comprehensible, and another one due to Ulam, which is quite strange.

Best personal regards,

3/XI 82 г.　　Boris

Boris Chirikov

P.S. I have learned with a regret that Dr. Cray had been expelled from his own famous firm (see Electronics, 1981, No 24). Have you heard anything about a new super-super computer Japanese promise to show at Expo-85?

LONG-TIME CORRELATIONS IN THE STOCHASTIC REGIME

Charles F.F.Karney

Reviewer's comments to Manuscript No FO-108.

The reviewing paper treats a novel aspect of stochastic phenomena, the long-time correlations. Since the latter determine the statistical properties of chaotic motion this problem is of a primary importance for the general theory of dynamical systems as well as for a number of various applications. The main author's result is in that the presence of an even small regular component of motion embedded into stochastic region typically changes the exponential correlation decay to a power (algebraic) one (Fig. 6). Moreover, the exponent of the power dependence turns out to be less than one, thus leading to a rather unusual and complicated statistical behavior. This result was obtained via a vast computation using a very simple but representative model for a stability islet inside chaotic component. I would add that the success of the reviewing work is, partly, due to a very skilful combination of the simplest model with the best computing facilities, the philosophy which I am certainly an ardent adherent of.

Another significant point relates to the fact that the behavior found in the reviewing paper is surprisingly similar (both qualitatively as well as quantitatively) to that for a complitely different model in Ref./1/(below) where the quantity P_t was computed. It is one more sign of universality for such a behavior (as discussed in Ref./2/) which is worth mention in the paper. From this point of view a puzzling result is just exponential correlation decay observed in spite of the stability islet present (Fig. 3).

My main objection relates to author's treatment of the diffusion (Section 6). It is in sharp contradiction to the meaning of his basic result discussed above. I mean the finite value (6400 ∓ 800!) ascribed to the diffusion rate D while it is actually infinite. The author as if got scared by his own discovery and tried to find some reservations as, for example, by saying on pp. 1 and 27 that the correlation decay is about critical ($\sim \tau^{-1}$) even though the numerical data in Fig. 6c clearly show the slower decay. In view of this contradiction I would suggest to withdrow the whole Section 6 from the paper to publish its material separately after additional studies which could include, in particular, direct measurements of the diffusion rate as a function of time, or, better to say, of the time dependence for the second moment which is no longer linear.

I would also recommend to publish Appendix B on the numerical methods as a separate paper which perfectly suits the Journal of Computational Physics. Moreover, if written at length, with some listings, especially those in CAL, that paper could give a very instructive example of how to use most efficiently the unique possibilities of the Cray-1 supercomputer.

A few minor remarks:

1) it is not common to require zero correlation time for the random variable (p. 3);

2) the two different probabilities, related to segment frequecy and to the measure, should be explained in a clearer way (p. 11);

3) the correlation (5) and diffusion (6) could be introduced from the beginning (p. 11) in a more common manner as is done later, on pp. 24, 25;

4) the estimate on p. 21 seems to contradictswith f(t) continuation to $t > 10^8$ ($\alpha = 4 > 2.4$) while actually it does not;

5) I suspect that the title of Ref. 4 must read: 'Stochasticity and Heteroclinic Oscillations...'

The reviewing paper certainly should be published in Physica D Journal, yet some revision is desirable, im my opinion.

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Boris V.Chirikov

/1/ B.V.Chirikov, D.L.Shepelyansky, Statistics of the Poincare Recurrences, and the Structure of Stochastic Layer of a Nonlinear Resonance, 9th Intern. Conf. on Nonlinear Oscillations, Kiev, 1981; Preprint INP 81-69, 1981 (in Russian).

/2/ B.V.Chirikov, Chaotic Dynamics in Hamiltonian Systems with Divided Phase Space, 7th Sitges Conf. on Dynamical Systems and Chaos, Barcelona, 1982.