# Reply to ''Comment on 'Quantum suppression of chaos in the spin-boson model' ''

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We dispute the claim by Bonci *et al.* [Phys. Rev. E **56**, 2325 (1997)] that we have misidentified a chaotic trajectory, and point out that their new results actually support the main conclusions of our original article. [S1063-651X(97)01508-0]

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In the preceding Comment the authors attempt to look independently at the problem studied by us in a recent publication [1]. They make a number of points, the most important of which is that, in their opinion, we have mistakenly identified as chaotic a trajectory that has, in fact, a vanishingly small Lyapunov exponent.

We did state in our paper [1] that we had found nonvanishing Lyapunov exponents for trajectories in the neighborhood of the (+) branch, and we stand by that claim. Since we have not been able to compare our algorithm to the one used by Bonci *et al.*, we cannot say precisely why we do and they do not. As we explained in our paper, the chaos is confined to the neighborhood of the unstable periodic orbit, and it seems that the truly chaotic trajectories lie in a very narrow region of phase space around this orbit. This makes the calculation of Lyapunov exponents for these orbits a little tricky. If the numerical integration routine takes too large a step in the wrong direction it may leave the chaotic region entirely and the Lyapunov exponent will then approach zero [2].

In particular, the trajectory that starts from the Autler-Townes (AT) initial conditions that we quote in our paper ($x=0.483$, $y_0=0$, $z_0=-0.876$) may or may not be chaotic itself—two different algorithms we have tried yield different results. We need to point out, however, that these are not exactly the same as the initial conditions for the trajectory we show and study in our paper [Figs. 1(a) and 1(b) of [1]]. Our approach was typically to take the AT initial conditions as a starting point in our search for the periodic orbits, which are always to be found nearby. The initial conditions for the periodic orbit shown in Fig. 1(a) of [1] are somewhere in the range $x_0=0.4738\pm0.0002$, $y_0=0$, $z_0=-\sqrt{1-x_0^2}$. For $\overline{n}=81$ we find consistently chaotic trajectories in this neighborhood.

In any event, our diagnostic of chaos did not rely solely on Lyapunov exponents or visual observation of the trajectories. Our paper [1] shows, for instance, a remarkable change in the spectrum [Figs. 2(a) and 2(b) of [1]]—not just the apparition of many new lines but of actual broadband structures, characteristic of chaos. On the same page of [1] we give a detailed account of how the periodic orbit becomes unstable, based on our Floquet analysis of a linearized model.

Bonci *et al.* seem to disregard all this evidence and instead use some Poincaré sections (their Figs. 3 and 4) to bolster their claim that nothing unusual happens in the neighborhood of the (+) branch. We agree that those sections do not show anything special, but since an actual instability clearly *does* take place, all their figures prove is that they are

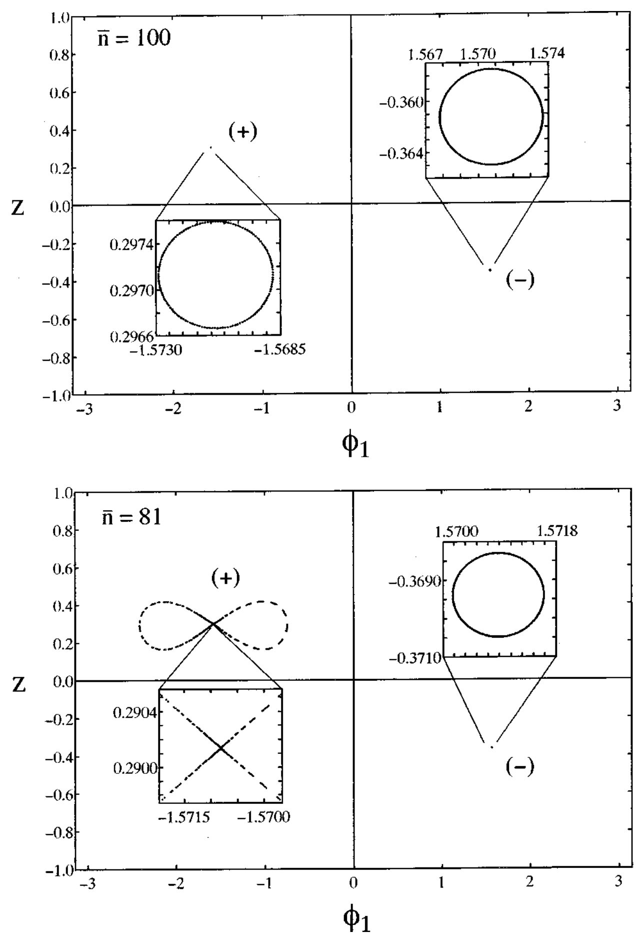


FIG. 1. Poincaré maps generated by one trajectory in the vicinity of the (+) periodic orbit and one trajectory in the vicinity of the (−) periodic orbit, for $\overline{n}=100$ and 81, respectively. For $\overline{n}=100$, the enlargements of what look like single points show the trajectory winds over a narrow stable torus around the periodic orbit. For $\overline{n}=81$, the (−) branch is still stable but the (+) branch has become unstable: in the Poincaré map, the fixed point associated with the (+) periodic orbit has become a hyperbolic, homoclinic point. Chaos is generic in the vicinity of such points.

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looking at a poor choice for the surface of section. Consider instead Fig. 1, which shows the intersections of a trajectory around the (+) branch, and a trajectory around the (−) branch, with the hyperplane $\phi_2 = 3\pi/2$ (for $d\phi_2/dt > 0$, and projected on the $z$-$\phi_1$ plane). Note that these *are* all canonical variables [see Eqs. (4) and (5) of [1]]—the claim by Bonci *et al.* that we were somehow ''deceived'' by our use of noncanonical variables is baseless.

Figure 1 shows clearly that the (−) branch is associated with an elliptical fixed point of the Poincaré map (the stable periodic orbit), whereas the nature of the (+) branch fixed point changes from elliptic to hyperbolic (i.e., unstable) as $\overline{n}$ goes from 100 to 81. The trajectory plotted in Fig. 1(b) is very near the separatrix for this hyperbolic point (the initial conditions are $x_0 = 0.473995 \pm 5 \times 10^{-6}$, $y_0 = 0$, $z_0 = -\sqrt{1 - x_0^2}$). The chaos *is* confined to the immediate neighborhood of the separatrix, which is a standard result for weakly chaotic systems. However, it is clear that the dynamics in the neighborhood of the (+) branch is more unstable than in the neighborhood of the (−) branch, and that the quantum system does not reflect at all this instability, nor does it seem to be affected in any particular way by this transition in the semiclassical dynamics. These two points were the basic message of our paper.

It is interesting to note that Bonci *et al.*'s own sample calculation for $\overline{n} = 25$ actually *supports* our conclusions. In Fig. 2 of their paper they show a chaotic trajectory in the neighborhood of the (+) branch and a regular trajectory in the neighborhood of the (−) branch, according to the Lyapunov exponents. In their Fig. 6 they calculate the growth of quantum uncertainty for those two initial conditions (the top two curves). The results are essentially indistinguishable. This is quite convincing evidence for our claim that the quantum system's evolution does *not* exhibit any particular correlation to the semiclassical chaos.

Bonci *et al.* prefer to draw the somewhat weaker conclusion that the presence or absence of chaotic behavior for an *individual* semiclassical trajectory may not be a good indication of how the uncertainty will grow in the quantum system; instead, they suggest, an average over nearby semiclassical trajectories should be taken. The two bottom curves in their Fig. 6 show the results of such an average. This time, the suggestion is that the uncertainty should grow faster along the (−) branch, presumably because ''the phase space surrounding the (−) branch is probably more chaotic than that in which the (+) branch is imbedded.'' And yet, the main point here, once again, is that the quantum system does *not* reflect this alleged difference between the semiclassical phase space regions. The quantum uncertainty along the (−) branch does not grow faster than along the (+) branch in any significant way.

This agrees entirely with our conclusion: to quote from our paper: ''there is no correlation between any particular dynamical variable and the presence or absence of semiclassical chaos.'' Figure 6 of Bonci *et al.* exhibits just such a lack of correlation, whether one chooses to measure the irregularity of the semiclassical system by the Lyapunov exponents of individual trajectories or by Bonci *et al.*'s own method of averaging over nearby semiclassical trajectories.

[1] G. A. Finney and J. Gea-Banacloche, Phys. Rev. E **54**, 1449 (1996).
[2] We have occasionally had to set the relative precision of our adaptive-step integration routines as low as $10^{-13}$, which corresponds typically to a step size of between $10^{-3}$ and $10^{-4}$, in units where $\omega = 1$.