# Reply to "Comment on 'Suppression of chaos by resonant parametric perturbations' "

Ricardo Lima
*Centre de Physique Théorique, Centre National de la Recherche Scientifique, Luminy, Case 907, F-13288, Marseille CEDEX 09, France*

Marco Pettini
*Osservatorio Astrofisico di Arcetri, Largo Enrico Fermi 5, 50125 Firenze, Italy*

(Received 9 February 1993)

The preceding Comment [Cuadros and Chacón, Phys. Rev. E **47**, 4628 (1993)] on the paper by Lima and Pettini [Phys. Rev. A **41**, 726 (1990)] contains a correct premise; however, erroneous consequences are drawn from it. In this Reply we explain why.

PACS number(s): 05.40.+j, 05.45.+b

For the sake of clarity, let us begin by summarizing the criticisms contained in the Comment to our paper of Ref. [1]. The authors of the Comment claim that: (a) the Melnikov function in Eq. (11) of Ref. [1] contains an incorrect polynomial prefactor, denoted by $B(\Omega)$; (b) "The discrepancies . . . between the analytical (Melnikov method) and numerical (Lyapunov exponents) results are shown to be basically due to an error in the calculation of the Melnikov distance . . ."; (c) "figures 2 and 3 of Ref. [1] . . . are affected by the error in $B(\Omega)$; (d) with the correct form of the Melnikov distance ". . . the degree of agreement between MM and LE predictions is the same near the principal resonance as near the resonances with the second and third harmonics of the forcing frequency." As far as item (a) is concerned, we do agree: the correct expression that replaces Eq. (11) of Ref. [1] is

$$\Delta(t_0)=\frac{2\sqrt{2}}{\sqrt{\beta}}\pi\gamma\omega\,\mathrm{sech}\left[\frac{\pi\omega}{2}\right]\sin(\omega t_0)+\frac{4\delta}{3\beta}-\frac{\pi\eta}{6\beta}(\Omega^4+4\Omega^2)\mathrm{csch}\left[\frac{\pi\Omega}{2}\right]\sin(\Omega t_0)\ . \qquad (1)$$

At variance, we do not agree with the consequences that are drawn from item (a). We think that the most convincing evidence of the inconsistency of items (b), (c), and (d) is provided by Figs. 1 and 2 (replacing Figs. 2 and 3 of Ref. [1]). These figures are obtained by computing the inverse of the time elapsed between two successive homoclinic intersections, $\tau_M^{-1}$, as a function of the parametric perturbation frequency $\Omega$, and derived from Eq. (1). The results show a very good *qualitative* agreement with the previous results of Ref. [1]. Obviously Figs. 1 and 2 show some difference with respect to Figs. 2 and 3 of Ref. [1]; for instance, Fig. 1 shows that smaller perturbations are effective in suppressing chaos, and the low-frequency beat is now absent, which is satisfying. Moreover, $\tau_M^{-1}(\Omega)$ in Fig. 2 shows a broader "line" than in Ref. [1].

Conversely, no trace appears of higher harmonics. Hence the main criticism is rejected.

Let us now explain why. The parametric perturbation method to reduce or suppress chaos, as described in Ref. [1], is based on a *fundamental* ingredient: its *interferential nature*. The possibility of describing such an interferential effect for homoclinic intersections is the peculiarity of the Melnikov technique; apparently, the authors of the Comment did not appear to grasp this point. In fact, focusing attention only on the threshold values of $\eta$ through the functions $D(\Omega)$ and $D'(\Omega)$ is insufficient and misleading. In Ref. [1], two lines below Eq. (12), we say "Let us first consider the effect of the modulus of the correction $B(\Omega)$, disregarding the phase factors initially set equal to 1." This means that Lemma 1 holds true under this condition of definite phase relation between the forcing term and the parametric perturbation term. Then Lemma 1 just gives the necessary conditions for the suppression of chaos to take place: resonance and suitable values for the parameters.

Moreover, in the Remark following Lemma 1, it is

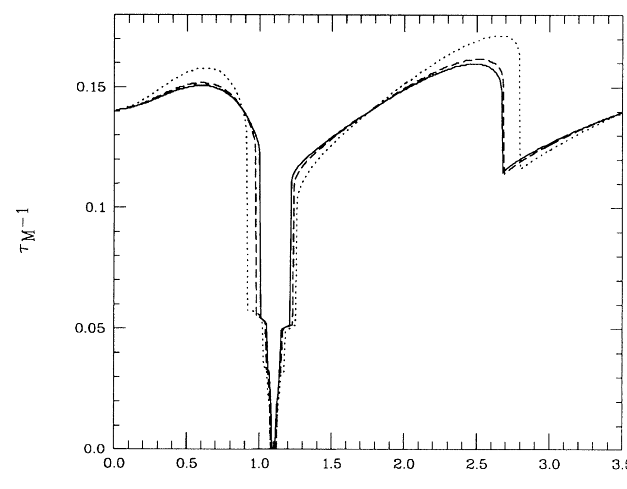


FIG. 1. Inverse of the time $\tau_M$ elapsed between two successive homoclinic intersections, computed using Eq. (1), is plotted vs the parametric perturbation frequency $\Omega$. Parameters are $\beta=4$, $\delta=0.154$, $\gamma=0.088$, and $\omega=1.1$. The continuous line corresponds to $\eta=0.09$, the dashed line to $\eta=0.1$, and the dotted line to $\eta=0.15$.

1063-651X/93/47(6)/4630(2)/$06.00  

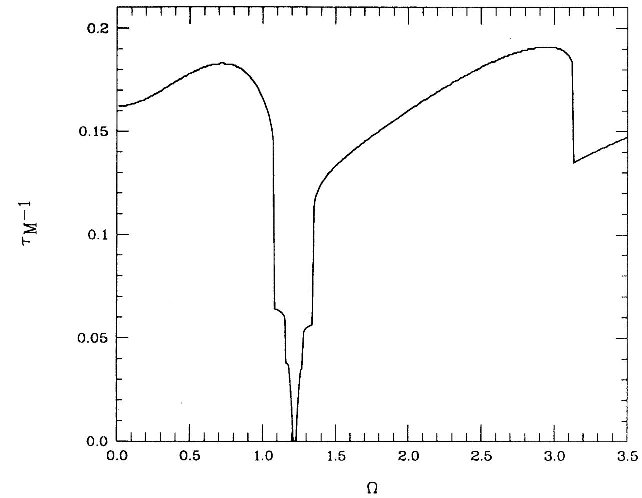


FIG. 2. $\tau_M^{-1}$ vs $\Omega$ is reported for $\beta=4$, $\delta=0.154$, $\gamma=0.114$, $\omega=1.22$, and $\eta=0.17$.

clearly stated that if we replace $\sin(\Omega t_0)$ by $\sin(\Omega t_0+\varphi)$, then "the reasoning breaks," i.e., the prediction based on the Melnikov method is phase sensitive; $A(\omega)$ and $B(\Omega)$ are only related with the threshold value, not with the interferential effect.

In other words, choose some $t_0$ as origin, $\omega=\Omega$, $A$, $B$, and $C$ such that $\Delta(t_0)=A(\omega)\sin(\omega t_0)+B(\Omega)\sin(\Omega t_0)+C$ is always positive; then take $\Omega=2\omega$; choose new parameters $A$, $B$, $C$ that according to the corrected threshold function of Lemma 1 *could* make $\Delta(t_0)$ always positive; now some appreciable effect can be obtained if for some $t_0$ we have $|\sin(\omega t_0)|=|\sin(\Omega t_0)|=1$, i.e., only if $\sin(\Omega t_0)$ is shifted by a constant phase of $\pi/2$, but this is no longer good for the fundamental harmonic.

Such a phase sensitivity is *absent* in both numerical simulations [1] and experiment [2]. Thus what is claimed in item (d) is here disproved.

In conclusion, the Melnikov method certainly provides some degree of explanation of the effectiveness of the parametric resonant suppression of chaos, but we insist on its serious limitations in describing the origin of chaos *partly* stemming from its perturbative nature. Perhaps a deeper reason might exist; in fact, let us recall that the existence of homoclinic intersections ensures the existence of a hyperbolic invariant set $\Lambda$ for the Duffing-Holmes oscillator here concerned, but other arguments exclude that $\Lambda$ is an attractor [3]; this is to say that some major reason for the existence of chaos in this system still seems unclear. Hence we feel the claim in item (b) is wrong, oversimplifies things, and is contradicted by Figs. 1 and 2.

[1] R. Lima and M. Pettini, Phys. Rev. A **41**, 726 (1990).
[2] L. Fronzoni, M. Giocondo, and M. Pettini, Phys. Rev. A **43**, 6483 (1991).
[3] J. Guckenheimer and P. J. Holmes, *Nonlinear Oscillations, Dynamical Systems and Bifurcation of Vector Fields* (Springer-Verlag, New York, 1983), p. 267.