Reply to “Comment on ‘Ratchet universality in the presence of thermal noise’ ”

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The Comment by Quintero et al. does not dispute the central result of our paper [Phys. Rev. E 87, 062114 (2013)] which is a theory explaining the interplay between thermal noise and symmetry breaking in the ratchet transport of a Brownian particle moving on a periodic substrate subjected to a temporal biharmonic excitation $\gamma [\eta \sin (\omega t) + \alpha (1 - \eta) \sin (2\omega t + \varphi)]$. In the Comment, the authors claim, on the sole basis of their numerical simulations for the particular case $\alpha = 2$, that “there is no such universal force waveform and that the evidence obtained by the authors otherwise is due to their particular choice of parameters.” Here we demonstrate by means of theoretical arguments and additional numerical simulations that all the conclusions of our original article are preserved.

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The foregoing Comment by Quintero et al. [1] offers some criticisms on certain particular (numerical) aspects of our previous paper [2], in which we studied theoretically and numerically a universal model – a Brownian particle moving on a periodic substrate subjected to a biharmonic excitation,

$$\dot{x} + \sin x = \sqrt{\sigma} \xi (t) + \gamma F_{\text{bihar}} (t),$$

$$F_{\text{bihar}} (t) \equiv \eta \sin (\omega t) + \alpha (1 - \eta) \sin (2\omega t + \varphi), \quad (1)$$

where $\gamma$ is an amplitude factor, and the parameters $(\eta \in [0, 1], \alpha > 0)$ and $\varphi$ account for the relative amplitude and initial phase difference of the two harmonics, respectively, while $\xi (t)$ is a Gaussian white noise with zero mean and $\langle \xi (t) \xi (t + s) \rangle = \delta (s)$, and $\sigma = 2k_b T$ with $k_b$ and $T$ being the Boltzmann constant and temperature, respectively.

Quintero et al. state in their Comment’s abstract that “The authors claim that their simulations prove the existence of a universal waveform of the external force which optimally enhances directed transport, hence confirming the validity of a previous conjecture put forward by one of them in the limit of vanishing noise intensity.” We disagree with this statement. Note that the existence of such a universal waveform was clearly conjectured for the first time in Ref. [3] in the context of a criticality scenario: Optimal enhancement of directed ratchet transport (DRT) is achieved when maximally effective (i.e., critical) symmetry breaking occurs. The mathematical proof of the ratchet conjecture was subsequently completed in Ref. [4] where such a universal waveform is shown to be unique for both temporal and spatial biharmonic forces. This universal waveform is a direct consequence of the degree of symmetry breaking (DSB) mechanism: It is possible to consider a quantitative measure of the DSB on which the strength of directed transport by symmetry breaking must depend. Such a theory of the ratchet universality has been applied to predict successfully the behavior of two kinds of soliton ratchets [5,6]. The theory demonstrated in our original article [2] uses, among other theoretical ideas, ratchet universality to explain the interplay between thermal noise and symmetry breaking in the ratchet transport of Eq. (1). The authors of the Comment neither take into account nor do they cite Refs. [3,5,6].

All the criticisms made by Quintero et al. are based solely on numerical simulations of Eq. (1) for the particular case $\alpha = 2$. However, the theoretical discussion of the structural stability of the ratchet scenario under changes in the parameter $\alpha$ (cf. the text and Fig. 1(c) in Ref. [2]) is essential to understand the theory demonstrated in our original article. Indeed, the authors claim that the numerical results shown in Fig. 1 of their Comment allow one to reach their two main conclusions:

(a) “First of all, our results are not compatible with the existence of an optimal force waveform [...] The apparent (approximate) confirmation of the prediction $\eta_{\text{opt}} = 4/5$ seems to arise from the specific choice of simulation parameters made by the authors.” As anticipated above, this conclusion arises from a misunderstanding on the part of the authors concerning the theory demonstrated in our original article [2]. Specifically, we demonstrated in Ref. [2] that the effect of thermal noise on the purely deterministic ratchet scenario can be understood as an effective noise-induced change of the potential barrier which allows one to understand the existence and behavior of the deviation $\Delta \eta (\alpha) \equiv \eta_{\text{opt}} (\alpha) - \eta_{\text{opt}}^0 (\alpha)$ as $\alpha$ is changed, while keeping constant the remaining parameters. Clearly, as mentioned in Ref. [2], the effect of noise on the DRT depends on the amplitude of the biharmonic excitation while keeping constant the remaining parameters. This means that the deviation $\Delta \eta (\alpha)$ will depend in its turn on the amplitude factor $\gamma$, while our theory, which was shown to be numerically confirmed for the particular value $\gamma = 2$ just as an illustrative example, holds...
The rest of the Comment presents a few examples of the application of a numerical fitting method for the average velocity which is solely subjected to obvious constraints regarding the breaking of relevant symmetries in Eq. (1). Two observations concerning this method are in order. First, as is well known, numerical and experimental data providing the dependence of the average velocity on relevant parameters, such as $\eta$ and $\varphi$, in many ratchets fit generally into soft curves that exhibit few extrema. It is thus not so surprising that the authors’ fitting method yielded good fits having so many free adjustable parameters (see, for example, Fig. 3(b) in the Comment). Even just a few parameters are enough to fit an elephant [8]! And second, the authors’ fitting method is not a physical theory since it has no predictive power at all—precisely because of its dependence on many adjustable parameters. This is in sharp contrast to the theory of the ratchet universality which predicted and explained, for example, why the directed soliton current is dependent on the number of atoms in the DRT of bright solitons formed in a quasi-one-dimensional Bose-Einstein condensate [6], as well as the strong enhancement of DRT of topological solitons in Frenkel-Kontorova chains due to the introduction of phase disorder into the asymmetric periodic driving [5]. Finally, the authors again misunderstand the ratchet universality when commenting on their results shown in their Fig. 4 (final paragraph): The ratchet universality does not predict a sinusoidal dependence of the average velocity on the initial phase difference $\varphi$ for general values of the amplitude factor. This is only valid for sufficiently small amplitudes (cf. Ref. [3]). Only the optimal values of $\varphi$ are predicted to be universal. In the present case, these values are $\varphi_{\text{opt}} = \{\pi/2, 3\pi/2\}$ (cf. Ref. [3]) which are indeed confirmed by the numerical results shown in Fig. 4 of the Comment.

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FIG. 1: Fig. 1 (Color online) Value of $\eta$ where the average velocity is maximum, $\eta_{\text{opt}}$, versus $\alpha$ [cf. Eq. (1)] for $\varphi = \varphi_{\text{opt}} \equiv \pi/2, \omega = 0.08\pi, \sigma = 2$, and two values of the amplitude factor: $\gamma = 2$ (squares) and $\gamma = 6$ (dots). Also plotted is the theoretical prediction for the purely deterministic case $\eta_{\text{opt}} (\alpha) \equiv 2\alpha/(1 + 2\alpha)$ (dashed line) and the function $\eta^*(\alpha) \equiv (4\alpha - 2) / (4\alpha - 1)$ (solid line, cf. Ref. [2]).

darüber muss man schweigen [7].

The rest of the Comment presents a few examples of the application of a numerical fitting method for the average velocity which is solely subjected to obvious constraints regarding the breaking of relevant symmetries in Eq. (1). Two observations concerning this method are in order. First, as is well known, numerical and experimental data providing the dependence of the average velocity on relevant parameters, such as $\eta$ and $\varphi$, in many ratchets fit generally into soft curves that exhibit few extrema. It is thus not so surprising that the authors’ fitting method yielded good fits having so many free adjustable parameters (see, for example, Fig. 3(b) in the Comment). Even just a few parameters are enough to fit an elephant [8]! And second, the authors’ fitting method is not a physical theory since it has no predictive power at all—precisely because of its dependence on many adjustable parameters. This is in sharp contrast to the theory of the ratchet universality which predicted and explained, for example, why the directed soliton current is dependent on the number of atoms in the DRT of bright solitons formed in a quasi-one-dimensional Bose-Einstein condensate [6], as well as the strong enhancement of DRT of topological solitons in Frenkel-Kontorova chains due to the introduction of phase disorder into the asymmetric periodic driving [5]. Finally, the authors again misunderstand the ratchet universality when commenting on their results shown in their Fig. 4 (final paragraph): The ratchet universality does not predict a sinusoidal dependence of the average velocity on the initial phase difference $\varphi$ for general values of the amplitude factor. This is only valid for sufficiently small amplitudes (cf. Ref. [3]). Only the optimal values of $\varphi$ are predicted to be universal. In the present case, these values are $\varphi_{\text{opt}} = \{\pi/2, 3\pi/2\}$ (cf. Ref. [3]) which are indeed confirmed by the numerical results shown in Fig. 4 of the Comment.

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