Reply to "Comment on 'Molecular gyroscopes and biological effects of weak extremely low-frequency magnetic fields' "

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It is stated in the Comment that the interference mechanism, which hypothetically drives some magnetobiological effects, occurs only in circumstances that are implausible and does not lead to a detectable magnetobiological effect. The reasoning underlying such a statement was analyzed. The statement is shown to be unsubstantiated.

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The Reply to the Comment consists of the following remarks, in order of importance.

(1) In Ref. [1], we noted that the number of angular states of a molecular rotator, in an idealized protein cavity, populated at room temperature approaches 10^3 and we considered that only the lowest states could result in the magnetic effect.

We assumed thereby that (*A*) *higher states do not contribute to the effect*, which then maximally equals $1/2n$, where n is the number of states taken into account $(0 \leqslant m \leqslant n)$,

$$\rho = 1 - \frac{S_0}{S} = 1 - \left[1 + \frac{|\sigma_{-m,m}(0)|^2}{\sum_m |\sigma_{mm}(0)|^2} J_1^2(h') \right]^{-1} \approx \frac{1}{2n} J_1^2(h'), \tag{1}$$

if n states are equipopulated at $t = +0$. This assumption was justified by the observed convincing consistency between the theory and many experiments.

In the Comment, it was assumed, in contrast, that (*B*) *higher states with values of m up to* 10^3 *contribute to magnetobiological effects (MBE)*. Naturally, given that, an MBE of the order of 10^{-3} was found. That implicit assumption is based only on the possibility of formally using large values for n in the theory. However, the theory, as presented in Ref. [1], has been developed for the case of several low-lying states.

It is now seen that MBE magnitude, as estimated in the Comment, was built on an additional assumption and was not related to the original mechanism. It is a logical error, a kind of thesis substitution: criticizable in fact was not *our* theory, but a theory developed *from* ours by the replacement of postulates *A* and *B*; in that, deductions were ascribed to the original theory.

The logically correct question would be which of the two assumptions, *A* or *B*, is closer to reality. Assumption *A*, being in agreement with experiments, is, of course, more vulnerable from a theoretical viewpoint. However, it provides the simplest form of our theory. We would like to specify this with more details.

There are different ways to account for why the higher states would not contribute to the reaction. For example, the overlapping of the exponential tails of the electron wave functions of both reagents determines the probability of a reaction. If the active site on the cavity wall is movable radially, in proportion to density $p_\tau(t)$, then the reaction probability varies directly with $\exp(-a/p_\tau^2)$, where a is a positive coefficient. In this version of the theory, the relative magnetic effect may be set maximally at 100%, at any n, by fitting a. Similar parameters appear in other scenarios that explicitly allow for the loss of contribution to MBE with growing m. However, those parameters cannot be determined from comparison with experimental data, because an end biological effect also depends on a number of irrelevant factors. On the other hand, we cannot avoid having such a parameter appear in the theory. Therefore, we used the version of the theory where such a model parameter appeared just implicitly: it is n, the number of accountable low-lying m states.

(2) 30 Å cavities are stated in the Comment to be impossible in a biological system. No physical reasons were provided for that statement. We cannot see why empty cavities with molecular rotators cannot appear in the course of slow protein conformational changes [2]. It does not contradict physical laws, at least until the opposite is proven. Therefore, it is an error to state that the aforementioned cavities are impossible in biology.

(3) It is stated also that (i) the cavities must have almost perfect symmetry, correct to 10^{-8}, in order for the eigenstates of angular momentum to be stationary states and the interference to occur and (ii) the potential we used would destroy the interference. There are two errors in these statements.

The cavity potential for the *oscillator* may be presented as a sum of the axially symmetric part and the part having no such symmetry. The latter is a small perturbation and we assume it to be less than about 0.1 (a small parameter), for the idealization of axial symmetry to make sense. Every perturbed eigenfunction [a sum of $\exp(im\varphi)$ and a small component] is stationary and suitable for calculating different observables that now decline from their unperturbed values as well, the declination being approximately less than 0.1. The perturbation of any rotational symmetry does not split the degenerated (at zero magnetic field) states $m, -m$ [3]. It means that the Zeeman doublet shifts synchronously, as a whole, under such a perturbation. Thus, the interference between the Zeeman states remains unperturbed and only the accuracy of our calculations can be discussed, 0.1 being suf-

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ficient since we compare them with biological data featuring a large variance.

This is even more true for the *rotator*, whose unperturbed eigenstates $\exp(im\varphi)$ are formed by the condition of free rotation. A small parameter here is the ratio of the perturbation, i.e., the characteristic potential value U, to the rotation energy scale $\hbar^2/2I \sim 10^{-8} k_B T$. It may be derived from the assessment

$$\frac{U}{k_B T} \sim \frac{\tau_D}{\tau} \sim 10^{-10},$$

where τ_D is Debye relaxation time, and τ is gyroscope relaxation time gained in our numerical computations. Then, the small parameter is about 10^{-2} and our analysis, with $\exp(im\varphi)$ as approximations to eigenstates, is correct.

In numerical computations, we used a potential of the fourth order rotational symmetry. Since van der Waals forces are weak and quickly drop with distance, that potential U, even with its thermal agitation, did not destroy rotation coherence within 0.1 s for 30 Å cavity. Note that we knowingly chose a low-symmetric potential, which is ''bad'' for an MBE, since actual biological cavities are likely to be more symmetric. Even in that case, we obtained a positive result. It means, again, the rotational number m is a ''good'' quantum number, and its eigenfunctions $\exp(im\varphi)$ are suitable for dynamic description.

Consequently, for an MBE to take effect, the cavity potential may significantly decline from perfect axial symmetry, since perturbations of the potential do not affect the energy gap of the Zeeman sublevels, nor do eigenfunctions $\exp(im\varphi)$ become unsuitable within the adopted accuracy. The bad low-symmetric potential we used retained rotational coherence within the limits required by an MBE. Hence, both statements (i) and (ii) are wrong.

(4) The reaction probability taken in our work was proportional to the squared, smoothed, probability density p_τ^2 of the rotator to be in a certain angular position as an approximation of general dependence. It is stated in the Comment that the dismissal of the linear term is a mistake. We do not agree with this. Consider the contribution to ρ of linear term $p_\tau(t)$, which is equal to (correct to a multiplier)

$$p_\tau(t) = \sum_{mm'n} \sigma_{mm'}(0) \frac{\sinh(\beta\tau)}{\beta\tau} e^{-i(m-m')\varphi} e^{-\beta t} J_n(z_{mm'}). \tag{2}$$

Integration gives the mean reaction probability

$$P_\varphi = w \lim_{\theta\to\infty} \int_{-\theta}^{t} p_\tau(t-t')dt'$$

$$= w \sum_{mm'n} \sigma_{mm'}(0) \frac{\sinh\beta\tau}{\beta\tau} e^{-i(m-m')\varphi} \frac{1}{\beta} J_n(z_{mm'}),$$

that depends on φ and should be averaged over $\varphi \in [0,2\pi]$:

$$P = \frac{w}{\Gamma} \frac{\sinh\eta}{\eta} \sum_m \sigma_{mm}(0).$$

We conclude that the linear term does not contribute to an MBE, however, it contributes to the field-independent part of the reaction probability and decreases, generally speaking, the relative *magnitude* of MBE. Given this, we note, that (i) relative weights of the linear and quadratic terms are unknown and (ii) comparison of theoretical and experimental MBE *magnitudes* gives no information. The only thing we may do is compare *forms* (amplitude dependencies) of responses. The linear term does not change the form and gives nothing essential. This discussion returns us to item (1) of the Reply.

(5) A simplified method was used in the Comment: wave function formalism instead of the density matrix approach. As a result, the reasoning, in part, lacks a relationship to the criticizable object. The smoothing constant τ enters all the expressions in our theory in combination with the inverse lifetime Γ, as a product $\eta = \Gamma\tau$. Analysis given in the Comment cannot take into account this context since it fails explicitly to allow for Γ. As we have shown, the product has to fulfill inequality $\Gamma\tau < 1$ in order to manifest an interference. On the other hand, τ has to be large enough to ensure smoothing of fast oscillations. This gives

$$\omega_g^{-1} < \tau < \Gamma^{-1},$$

the inequalities that provide a rather free choice for τ. It was a mistaken statement in the Comment that τ must be of a certain value.

(6) With regard to formula (18) in [1], it should read

$$\rho = 1 - \frac{1}{1 + \frac{1}{4} J_1^2(h')},$$

as follows from the above general expression (1); we thank J. C. Gill for having pointed out that inaccuracy.

In conclusion, the main statement of the Comment ''the mechanism proposed by BS would not lead to any detectable MBE'' is not substantiated in view of the reasoning listed in this Reply.

[1] V.N. Binhi and A.V. Savin, Phys. Rev. E **65**, 051912 (2002).
[2] V.N. Binhi, *Magnetobiology: Underlying Physical Problems* (Academic Press, San Diego, 2002).
[3] For that splitting a perturbation must include differentiation over the angular variable, which obviously is not so for the discussed cavity deformations of some symmetry group. Under

such a deformation each Zeeman sublevel appears to be split once more in accordance with an irreducible representation of the symmetry group of the perturbation. However, it would be quite improbable that new splitting is exactly of the same value as magnetic splitting (it is likely much stronger than magnetic splitting). Therefore, several similar Zeeman multiplets appear instead of the one unperturbed. This does not influence the interference.