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Comment

A Few Remarks on 'Combined Action of DC and AC Magnetic Fields on Ion Motion in a Macromolecule'

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Zhadin and Barnes [2005:26:323–330] concluded that they solved the differential equation describing combined action of DC and AC magnetic fields on thermal motion of ions in a biological macromolecule and, as a result, a diversity of biological phenomena could be explained. It is shown here that biological phenomena cannot be explained based on this model. Adair [2006:27:332–334] gave several arguments for the statement that the interaction of weak magnetic fields with ions trapped in protein cavities cannot produce detectable biological effects through changing the character of the ion orbits. The arguments are analyzed here and some are shown to be questionable or unjustified. We stress the difference between the conclusion made by Adair and that stated in this article. Bioelectromagnetics 28:409–414, 2007. © 2007 Wiley-Liss, Inc.

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The model proposed by Zhadin and Barnes [2005] (ZB model below) is based on the dynamics of an ion bound in a protein cavity under exposure to combined DC and AC magnetic fields and thermal noise. ZB came to the inference that the ion may gain energy significantly exceeding that of ion thermal motion, being exposed to the combined magnetic field at the cyclotron frequency Ω_c. However, the magnetic energy μH of the particle motion is many orders of magnitude less than its thermal energy: $\hbar\Omega_c \sim \mu H \lll kT$, where μ is the magnetic moment of ion motion. Obviously, magnetic effects are not possible under this condition. A model that claims such effects must contain errors. Some of them, pertaining to the *form* of the differential equation used by ZB and to the *method* used for solving the equation, are given below.

Consider a particle moving in a viscous medium under the influence of random thermal forces. As is known, both the viscous and thermal forces have a common physical origin in random collisions of the particle with molecules of the media. Therefore, the damping coefficient γ is in certain dependence with the correlation function of the external forces [Reif, 1965]. There is no such dependence between the terms of the equation used by ZB; it is a physical error.

However, ZB concluded: "weak ... combined magnetic fields increase the temperature of the ion thermal motion ... by about 60°C." Why did they get such a surprising result? Two errors have been made. First, the random nature of thermal perturbations, which makes ion motion incoherent, has not been taken into account. In fact, ZB consecutively reduced, or idealized, thermal forces first by the sum of sine-wave (harmonic) signals and then by a single sine wave. So, actually studied were particle oscillations in the presence of perturbations of two sorts: parametric perturbation (magnetic force) and nonparametric external force, both being harmonic. The differential equations of that type and their solutions are well known. Under sine-wave perturbations, the motion of ion is almost coherent and in resonant conditions the ion energy may significantly change. Consequently, the relative change of ion energy is great because ZB discarded the random character of thermal perturbations. Absolute increase of the energy is limited only by the damping coefficient γ, which was taken by ZB small enough to get that unrealistic change.

Second, even in the absence of any thermal perturbations, magnetic forces can alter the ion energy

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significantly just for an unlikely great time interval. ZB have not calculated a time required for the energy changes they observed. With the numerical values of the parameters ZB used, and even with zero damping $\gamma = 0$, it takes a few months.

When ZB took damping coefficient $\gamma = 120\ \mathrm{s}^{-1}$, they factually postulated that the ion moves coherently within time periods of about 0.01 s. This means extremely effective isolation of the ion motion from the chaotic thermal oscillations, because usually the thermal relaxation is much shorter, on the order of 10^{-11} s. On the other hand, ZB concluded about the magnetic field effects on the thermal motion. This is a manifest and visible logical contradiction: the authors should choose one; either they study a thermal motion and then $\gamma \sim 10^{11}\ \mathrm{s}^{-1}$, or $\gamma \sim 100\ \mathrm{s}^{-1}$ and no essential interaction with thermal bath, i.e. a coherent motion takes place. If ZB took into account the dependence of γ on thermal noise, they would not make this mistake. Exhaustive criticism of this and similar models has been developed [Binhi, 2002] in terms of both classical and quantum dynamics. Thus, the ZB model contains physical, mathematical, and logical contradictions and has no connection to reality.

Adair [2006] criticized the ZB model from another viewpoint. In this work, Adair does not make any distinction between the ZB model and other such models but directly addresses the assumptions of such models by focusing on disruption of ion orbits with the ultimate goal of showing that any change in the character of the ion orbits under weak magnetic fields cannot produce biological effects.

First, Adair discusses the idealization of central potential used in the ZB model. He states that even infinitesimal deviation from perfect spherical symmetry, quite probable in biological molecules, will disrupt any magnetic effects and hence the approximation of a central potential fails. The arguments used here by Adair are wrong. Adair considers an ion in the cavity as a classical point mass bouncing in collisions with cavity walls. The cavity is taken with a size 10^{-9} m, so that de Broglie wavelength is much less than the cavity diameter—an assumption that allows Adair to safely consider an ion as a classical point mass. However, the effective potential actually found from X-ray studies is about 7×10^{-11} m for Ca in calmodulin and de Broglie wavelength and the size of the potential are of the same order. Therefore, a trapped ion is a quantum wave rather than a classical particle. Adair introduces a mistake applying a classical model to an essentially quantum phenomenon, which cannot be reliably described in a classical way by some fundamental reasons. To make it clearer, in quantum mechanical (QM) language, the difference in classical orbits of a charge in a magnetic field is associated with the Zeeman effect. Practically, Adair states such an effect is impossible for any potential that deviates from the ideal one. It is not true. A lot of room-temperature magnetooptical phenomena in solids and liquids provide evidence for the Zeeman effect. Observation of the effects related to the Zeeman splitting does not require perfect central or axial symmetry [Binhi, 2003].

Next, it is stated that thermal motion of the boundary atoms of the cavity will disrupt orbits of an ion inside in a random manner so that any effect based on the existence of orbits should vanish. The mean disruption time is estimated to be about 10^{-11} s, while a time greater than 10^{-2} s ($\sim$1/60 Hz) is needed for the ELF magnetic effect to occur. In QM language, this means the lifetime of ion quantum states should be greater 10^{-2} s. It is interesting to note that not ions but small molecules rotating in cavities, or molecular gyroscopes, could have suitable lifetimes [Binhi and Savin, 2002] and a criticism similar to that of Adair may at least be argued [Binhi, 2003].

One more argument is that "the forces on the ion from the magnetic fields can be expected to be very much smaller ... than the forces from random noise electric fields and those electric fields will then disrupt the ion orbits." I note here that any electric field can be decomposed into a uniform field and a nonuniform field. In considering effects of electric field on ion motion Adair's model based on 'orbits' fails again, since a uniform electric field disturbs the orbits, but does not disturb Zeeman levels and hence magnetic effects, see for example Binhi [2002], section "Influence of an electric field on interference of ions." As to the nonuniform electric fields, they are produced mainly by neighboring molecules of the cavity wall, which form the ion potential. This contribution is addressed in a preceeding Adair comment. So the above argument about electric field contribution is partially wrong and partially excessive.

Further, Adair calculates a time constant for the dissipation of the kinetic energy of a calcium ion. The ion oscillates in a cavity of an idealized spherical macromolecule of radius a. In turn, the macromolecule oscillates in water and loses energy through the viscous resistance. First, a numerical error in Adair's calculation is seen in the formula (1) for the time constant: physical dimensions are *time* at the left of the formula and *squared length* at the right. One may guess from the text that the calculation was based on Stokes's law for viscous resistance of a spherical body. In this case, the correct formula for the time constant would be M_m/γ, where damping constant $\gamma = 6\pi a \eta$, η is the viscosity of water, which gives, with numerical values of parameters used in Adair [2006], about 10^{-11} s rather than

10^{-7} s as cited by Adair. Second, as was said above, an ion in such a small potential is better described as a wave than a particle, so that the "bounce/recoil" description is poor. However, even taking Adair's estimate of the ion mean free time between consecutive bounces $\delta t \sim 10^{-10}$ s and the estimate of the ion binding macromolecule velocity $V_m = v_i(m_i/M_m)$, the oscillation displacement of the macromolecule $V_m\delta t$ will be less than 10^{-12} m. Actually, it should be substantially less, as Adair did not take into account elastic, viscous, and inner inertial properties of the macromolecule itself. Furthermore, since Adair used nearly a 10-fold increased mean diameter of the ion bounding cavity versus the real size of the ion potential 0.7 Å, the displacement is less than 10^{-13} m or 0.001 Å. For such small displacements at and below 10^{-3} Å and high frequencies of about $1/\delta t \sim 10$ GHz, usage of Stokes's law is problematic. The rate of macromolecule energy absorption depends on the oscillation frequency and its calculation should be based on other physical principles, apparently the relaxation absorption of hypersound waves. In any case, physics faults and the numerical error in the calculation of the relaxation time make Adair's approach unrealistic. Numerically, the relaxation times of that sort are well known to be 10^{-11} to 10^{-12} s in liquids, and thus Adair's estimate is not only wrong but useless.

In the section "Accumulations, Ensembles" Adair concludes that there are no cumulative effects for magnetic fields acting on *trapped* ions. However, such a conclusion is evident in advance: there is no place in protein traps for many ions. In addition, this correct conclusion is based on irrelevant speculations about motion of *free* ions in a solution. Furthermore, the speculations include statements that are wrong: "The forces from the fields, acting always transverse to the direction of motion of the ion, do not change the ion's kinetic energy" and "Since for a large ensemble of ions at any given time the flux of ions moving in a given direction at a given velocity will be equal to a similar flux moving in the opposite direction with that velocity, the magnetic field will not change the mean overall momentum of the ions." First, though DC magnetic fields alone do not add to ion's kinetic energy, AC fields do so by the induced electric fields every half period. Second, the overall momentum of the ions, of any sort, actually appears in dc magnetic field. It is a diamagnetic-like momentum, whose origin is due to the 'transverse' magnetic forces [Binhi, 2002] and whose magnitude is that that appears in the discussed ZB and Adair's 'orbit' model.

Taken together, Adair's arguments are in a rhetorical style that is designed to convince readers educated mostly in life sciences. However, taken separately, and from a physical viewpoint, each argument looks rather weak or, at least, insufficiently substantiated. It should be emphasized that the criticism given here to Adair's arguments does not mean, as should already be clear, that the models he contests are correct. In fact, as has been repeatedly proven by many authors and has been exhaustively shown by Binhi [2002], they are actually false. In that book, more fundamental arguments are presented to demonstrate why the models cannot be effective. What is interesting is that these arguments do not concern the notion of temperature at all; they prove that the models are ineffective even at zero temperature. Thus, the 'thermal arguments' created by Adair against such models are of little importance, overall.

In conclusion: Do we need still more arguments to prove the models discussed are baseless? Note, that all of Adair's reasoning relies on the implicit assumption that the trapped ion is in thermal equilibrium with its boundary walls. But, as soon as it is stated that the "particle is in thermal equilibrium with medium"— there is no longer any necessity to study details, since the magnetic energy of the particle is many orders of magnitude less than its thermal energy: $\mu H \lll kT$. Obviously, magnetic effects cannot exist here! This is the most basic 'thermal argument' missed by Adair. It makes no sense to consider and discuss other reasons, like the potential asymmetry, electric noise, etc.

At the same time, the fact that effective frequencies in magnetobiology often coincide with characteristic frequencies of biologically relevant ions [Blackman et al., 1985; Liboff et al., 1987] requires further attentive and careful theoretical study.

Two clear possibilities follow. To explain observed biological effects of weak ELF magnetic fields (magnetobiological effects) one needs to equalize the inequality $\mu H \lll kT$: either the magnetic moment μ of a suggested magnetic target in an organism should be sufficiently large, as in the models based on magnetosomes, or the effective temperature T of the target should be sufficiently small, as it is in the molecular gyroscope model. Another interesting conceptual framework for low effective temperature is domains of low viscosity in water [Preparata, 1995]. Apparently, all these possibilities are hypothetical, but they are not at variance with physics, and they do not impede development of theoretical magnetobiology indicating promising theoretical directions in bioelectromagnetics.

REFERENCES

Adair RK. 2006. Comment: Analyses of models of ion actions under the combined action of AC and DC magnetic fields. Bioelectromagnetics 27:332–334.

Binhi VN. 2002. Magnetobiology: Underlying physical problems. San Diego: Academic Press.
Binhi VN, Savin AV. 2002. Molecular gyroscopes and biological effects of weak extremely low-frequency magnetic fields. Phys Rev E 65(051912):1–10.
Binhi VN. 2003. Reply to "Comment on 'Molecular gyroscopes and biological effects of weak extremely low-frequency magnetic fields'". Phys Rev E 68(023902): 1–3.
Blackman CF, Benane SG, Rabinowitz JR, House DE, Joines WT. 1985. A role for the magnetic field in the radiation-induced efflux of calcium ions from brain tissue in vitro. Bioelectromagnetics 6:327–337.
Liboff AR, Smith SD, McLeod BR. 1987. Experimental evidence for ion cyclotron resonance mediation of membrane transport. In: Blank M, Findl E, editors. Mechanistic approaches to interaction of electric and electromagnetic fields with living systems. NY: Plenum Press. 109–132.
Preparata G. 1995. QED coherence in matter. Singapore: World Scientific.
Reif F. 1965. Fundamentals of statistical and thermal physics. NY: McGraw-Hill.
Zhadin MN, Barnes F. 2005. Frequency and amplitude windows in the combined action of DC and low frequency AC magnetic fields on ion thermal motion in a macromolecule: Theoretical analysis. Bioelectromagnetics 26: 323–330.