Correct denition of the Poynting vector in electrically and magnetically polarizable medium reveals that negative refraction is impossible: reply

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Abstract: In the preceding Comment, Marques has criticized my derivation of the heating rate in electrically and magnetically polarizable media. The main thrust of the Comment is that the macroscopic magnetization current $c\nabla \times \mathbf{M}$ is "not enough for properly characterizing a *physical* portion of the body". This and other Marques' statements are critically analyzed and rebutted in this reply.

OCIS codes: (160.1245) Artificially engineered materials; (350.3618) Left-handed materials

References and links

1. V. A. Markel, "Correct definition of the Poynting vector in electrically and magnetically polarizable medium reveals that negative refraction is impossible," Opt. Express **16**, 19152–19168 (2008).
2. R. Marques, "Correct definition of the Poynting vector in electrically and magnetically polarizable medium reveals that negative refraction is impossible: comment," Opt. Express **17**, 7322-7324 (2009).
3. V. A. Markel, "Scattering of light from two interacting spherical particles," J. Mod. Opt. **39**, 853–861 (1992).
4. V. A. Markel, "Antisymmetrical optical states," J. Opt. Soc. Am. B **12**, 1783–1791 (1995).
5. V. A. Markel and E. Y. Poliakov, "Radiative relaxation time of quasi-normal optical modes in small dielectric particles," Phil. Mag. B **76**, 895–909 (1997).
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Equations of the Comment are referenced below as (C#).

In the classical electrodynamics of continuous media, the heating rate q is given by the well-known conventional formula (C1). The heating rate is defined as the energy absorbed and transformed into heat by a material per unit volume (or surface, if there is a surface contribution), per unit time; it is a macroscopic quantity and a function of position. In a recent paper [1], I have argued that Eq. (C1) does not always yield the correct heating rate and that instead one must use the more fundamental formula (C2) in which the total current induced in the medium, $\mathbf{J}$, is given by the right-hand side of Eq. (C5). Note that both expressions (C1) and (C2) apply only to stationary fields. In Comment [2], Marques has stated that expressions (C1) and (C2) are fully consistent if, instead of Eq. (C5), we use a different expression for $\mathbf{J}$ in Eq. (C2). According to Marques, this alternative expression properly takes into account "all the currents associated with a continuous distribution of magnetic moment". In spite of this claim, Marques did not provide a formula for this alternative current, at least not in a form that can be directly and unambiguously used. In what follows, I will show that the Comment is based on an incorrect

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understanding of the origin and mathematical properties of surface currents in magnetics and on an unsubstantiated and factually incorrect assumption about the magnetization vector **M**.

Marques starts by observing that expressions (C1) and (C2), when integrated over the whole volume, yield the same total heat Q absorbed by the body per unit time. This observation is, of course, correct, but it has already been stated in several places of my paper [1], in particular, on p. 19161. What seems to be important here is that while the Marques' conclusion about the equivalence of Q in both theories is correct, his derivation of this conclusion has revealed a misconception. Specifically, Marques refers to his expression (C5) as to the "volumetric current density" and suggests that an additional surface current given in Eq. (C6) should be added to it. In fact, Eq. (C5) already contains both the volume and the surface contributions to the current density. The surface current is obtained by differentiating $\nabla \times \mathbf{M} = \nabla \times \chi_m \mathbf{H} = \chi_m \nabla \times \mathbf{H} + \nabla(\chi_m) \times \mathbf{H}$. The last term in this chain of equalities contains a gradient of the function $\chi_m(\mathbf{r}) = [\mu(\mathbf{r}) - 1]/4\pi$ which jumps at the body surface; differentiation of this jump results in the appearance of the surface current. It is neither necessary nor correct to add any additional terms to the right-hand side of Eq. (C5). Incidentally, the last integral in Eq. (C4) is identically zero because it is evaluated over a surface which completely encloses the body. To avoid ambiguity, this surface must be drawn in free space where $\mathbf{M} = 0$ by definition. Consequently, no additional terms need to be added in Eq. (C5) in order to understand Eq. (C4).

Next, Marques offers a discussion of the point-wise equivalence (or the lack of it) of expressions (C1) and (C2). To quote from the Comment, "It may seem however, that Eq.C1 does not give correctly the heating ratio [rate? - V.M.] in a differential volume of the body, since the surface current density Eq.C6 is not present inside the body. However, this interpretation is also incorrect ..." It is not clear to me, which interpretation Marques is referring to. The fact that expressions (C1) and (C2) in which the current **J** is computed according to Eq. (C5) are not point-wise equal is not an interpretation but a mathematical theorem. I must conclude therefore that what Marques really means here is that the use of the definition (C5) in Eq. (C2) is incorrect. This is supported by the reminder of the sentence quoted above which reads "...because the volume current density $\mathbf{J}_v = c\nabla \times \mathbf{M}$ is not enough for properly characterizing a *physical* portion of the body." And then "The surface current Eq.C6 can not be ignored in order to properly describe any small portion of the magnetized body." But again, this is incorrect. The term $c\nabla \times \mathbf{M}$ contains both the volume and the surface contributions. Of course, the surface contribution is non-zero only at the true boundary of the medium, not at an imaginary surface surrounding a "physical portion of the body". Even the unit vector $\hat{\mathbf{n}}$ can not be defined on that surface because it is not possible to tell into which of the two neighboring "small portions of the body" it should point. It is therefore not clear to me how Eq. (C6) can be of any use for describing a "small portion of the magnetized body".

Now, the central argument of the Comment is that the use of Eq. (C5) contradicts the interpretation of the vector **M** as the magnetic moment per unit volume. But this interpretation is neither necessary, nor essential, nor generally correct. The use of the symbol "≡" in Eq. (C7) suggests that Marques thinks that the magnetic moment $\delta\mathbf{m}$ of the volume δV is, by definition, $\int_{\delta V} \mathbf{M} dV$. However, the correct definition is $\delta\mathbf{m} \equiv (2c)^{-1} \int_{\delta V} \mathbf{r} \times \mathbf{J} dV$. It is well known that this expression is unambiguously defined only in some special cases. Indeed, under an arbitrary shift $\mathbf{r} \to \mathbf{r} + \mathbf{a}$, the magnetic moment transforms as $\delta\mathbf{m} \to \delta\mathbf{m} + (2c)^{-1} \mathbf{a} \times \int_{\delta V} \mathbf{J} dV$. Therefore, $\delta\mathbf{m}$ is a meaningful physical quantity only if the last integral is zero. For example, in magnetostatics the integral is zero when evaluated over the whole body. Therefore, the total magnetic moment of a statically magnetized body is a useful quantity. But the differential density of magnetic moment is still not a well-defined quantity in the macroscopic theory. This is why in many standard textbooks the surface integral in equations similar to Eq. (C7) is evaluated in free space over a surface that encloses the whole body, as Marques has correctly pointed

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out. But there is no difficulty here at all. I challenge Marques - and the Reader - to provide an example of a physically meaningful calculation which starts from the macroscopic Maxwell's equations for homogeneous media and in which it is essential to know that **M** is the density of magnetic dipole moment (derivation of the Maxwell-Garnett formula and similar mixing rules or the Ising model and its generalizations do not qualify for obvious reasons).

Although this is not stated directly in the Comment, it seems plausible that the Marques' critique of the definition (C5) of the total current is inspired by the model of magnetization as a lattice of closed-loop currents. The magnetic dipoles Marques mentions are these closed loops and the "physical portion of the body" is a region of space containing several such loops (which are viewed as completely isolated). Although this model may have some pedagogical value, it should not be taken too seriously. There are many reasons why the model may be misleading but it should suffice to say that even if these closed-loop atomic currents exist, they are microscopic (and immensely complicated) quantities that obey quantum rather than classical laws of motion and which can not be used in macroscopic classical expressions such as Eq. (C2). This last statement is especially evident for magnetism which is caused by alignment of intrinsic magnetic moments of elementary particles, although these types of magnetism are of only marginal interest for this discussion.

But how can I deny the reality or usefulness of the closed-loop current model if such currents can readily be excited in the so-called split-ring resonators (SRRs)? The answer is simple. Unlike the microscopic currents, the current in the metal of an SRR obeys the classical laws of motion (this distinction is very important) and therefore it is proportional to the macroscopic electric field as is described by the formula $\mathbf{J}_\omega = -i\omega[(\varepsilon_\omega - 1)/4\pi]\mathbf{E}_\omega$. Now, if the magnetic resonances can be excited in SRRs forming a periodic array, this means that the macroscopic electric field oscillates (changes direction) on scales smaller than one period of the composite. Such a strongly oscillating field can not converge in any reasonable norm to a smooth wave that oscillates on a much larger scale and therefore the medium can not be viewed as electromagnetically homogeneous. Introduction of effective parameters for such a medium is misleading.

I have studied the antisymmetric resonances in subwavelength particles, of which the SRR resonance is a special case, in the series of papers [3–5]. It can be shown from the results of Ref. [5] that the maximum value of the magnetic polarizability α_m (for purely magnetic resonances) can be estimated as $|\alpha_m| \sim 0.5V(kR)^2/[\omega/\sigma + (kR)^2(k^3V)/6]$, where σ is the conductivity of metal (which is assumed to be Drudean), $k = \omega/c$, ω is the resonance frequency, R is the characteristic size of the particle (radius, in the case of SRR), and V is the volume occupied by metal. In order to observe a significant magnetic response, $|\alpha_m|$ must be at least of the same order of magnitude as the elementary cell volume (which, in the case of SRRs, is much larger than V). If at some value of R this condition is satisfied, the medium can not be viewed as homogeneous for the reasons stated above. And when R is decreased, the value of $|\alpha_m|$ decreases rapidly until the magnetic resonance is suppressed, at which point the medium can be viewed as homogeneous. This is consistent with the result obtained by Tsukerman [6] which sets the lower bound on the parameter qh above which negative dispersion of Bloch waves in photonic crystals is possible (q - the Bloch wave number, h - period of the crystal).

In conclusion, it can be shown with mathematical certainty that expressions (C1) and (C2) differ point-wise if the expression in the right-hand side of Eq. (C5) is used for the total current **J**. The suggestion made by Marques that the expression for the current which is used in Eq. (C2) must be modified by more correctly accounting for the surface currents is mathematically and physically unsound. The most general form of the macroscopic current that can flow in magnetics is given in Eq. (C5) (assuming that no more than one spatial and one temporal derivative of the fields can enter the formula) and mixing microscopic and macroscopic quantities in the same macroscopic classical expression (C2) is fundamentally incorrect.

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