COMMENT

Comment on "What is Negative Refraction"

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ARTICLE HISTORY

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ABSTRACT

In a recent review entitled *What is Negative Refraction* [JMO 56, 1727-1740 (2009)], McCall has criticized my previous work on the subject. This and some other aspects of McCall'spaper deserve commentary. Specifically, I will discuss below the following three points: (1) the definition of negative refraction; (2) McCall's argument regarding the form of the Poynting vector; and (3) McCall's claim of overwhelming experimental evidence for the physical reality of negative refraction.

KEYWORDS

Negative refraction; Poynting vector; Magnetic poles

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1. Definition of negative refraction

It is logical to start with the definitions. In my previous work [1], I have stated that negative refraction in a passive medium occurs if and only if the transmitted wave vector $\mathbf{k}_t$ satisfies the condition $\mathrm{Im}k_t^2 < 0$. This condition is applicable to general anisotropic and nonlocal media. The definition of a passive medium is the following. Assume that the half-space $z > 0$ is occupied by a passive medium and the half-space $z < 0$ is vacuum. Then any monochromatic plane wave transmitted from vacuum (where the sources of radiation are located) into the medium experiences exponential decay away from the interface, i.e., when $z \to \infty$. In an active medium, such plane waves experience exponential growth. The condition for the negative refraction in active media is reversed and reads $\mathrm{Im}k_t^2 > 0$. Below, I will discuss only the case of passive media. In particular, in local isotropic media the condition simplifies to $\mathrm{Im}(\epsilon\mu) < 0$, where ϵ and μ are the permittivity and permeability of the medium evaluated at the frequency of the transmitted wave.

In his review [2], McCall writes on pp. 1733-1734: "Note also that with regards to the claim that that the inequality on the right of Equation (24) rules out NPV [negative phase velocity - V.M.] propagation, the criterion to which this refers, i.e., Equation (12), is based on the $\mathbf{E} \times \mathbf{H}$ form of the Poynting vector. When $\mu < 0$, $\mathbf{E} \times \mathbf{B}$ is oppositely directed to $\mathbf{E} \times \mathbf{H}$, so that even were one to take $\mathbf{E} \times \mathbf{B}$ as the

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preferred Poynting vector, one would then need to revise the NPV criterion anyway, before drawing conclusions about its occurrence." Here the equation references (12) and (24) apply to the McCall's review rather than to this paper. Specifically, Equation (12) is

$$\mathrm{Im}(\epsilon\mu) = \epsilon'\mu'' + \epsilon''\mu' < 0$$

and the right-hand side of Equation (24) (which is of relevance here) is

$$\omega|\mathbf{E}_0|^2 \mathrm{Im}[\epsilon(\omega)\mu(\omega)] \exp[-2\,\mathrm{Im}(\mathbf{k}\cdot\mathbf{r})] > 0 \ .$$

All the statements quoted above are incorrect. Firstly, I have never equated negative refraction with negative phase velocity. These two phenomena, although not unrelated, must be considered separately. For example, Bloch waves in photonic crystals can easily be characterized by negative phase velocity. On the other hand, the term "negative refraction" is only applicable to electromagnetically homogeneous media in which effective parameters can be reasonably introduced, as is the term "refraction" in general. Secondly, even in the case of electromagnetically homogeneous media, the condition $\mathrm{Im}k_t^2 < 0$ for negative refraction is absolutely independent of the definition of the Poynting vector or of the relative directions of the phase and group velocities. Therefore, McCall's statement that "... even were one to take $\mathbf{E}\times\mathbf{B}$ as the *preferred* Poynting vector, one would then need to revise the NPV criterion anyway, before drawing conclusions about its occurrence" is both irrelevant and incorrect. Since the misunderstanding seems to persist, I will now explain in detail the reasoning behind the condition $\mathrm{Im}k_t^2 < 0$.

Let a monochromatic plane wave with the wavenumber $\mathbf{k}_i = \hat{\mathbf{x}}k_x + \hat{\mathbf{z}}k_{iz}$ be incident from vacuum onto a half-space $z > 0$ occupied by a passive medium. Here $\hat{\mathbf{x}}$ and $\hat{\mathbf{z}}$ are unit vectors in the directions of the X- and Z-axes. Thus, k_x is the projection of the incident wave vector onto the interface and the plane of incidence is XZ. We will assume that the quantity k_x is purely real. Otherwise, the incident wave is evanescent in the direction orthogonal to the interface $z = 0$ which necessitates the presence of some additional interface; such a possibility is not considered here. As is well-known, the transmitted wave vector lies in the same plane XZ and its projection onto the interface is conserved. Therefore, we can write the transmitted wave vector as $\mathbf{k}_t = \hat{\mathbf{x}}k_x + \hat{\mathbf{z}}k_{tz}$. Now square the vector $\mathbf{k}_t$ and compute the imaginary part of the result, taking into account the fact that $\mathrm{Im}k_x = 0$. We obtain

$$\mathrm{Im}k_t^2 = \mathrm{Im}(k_x^2 + k_{tz}^2) = \mathrm{Im}(k_{tz}^2) = 2\mathrm{Re}k_{tz}\mathrm{Im}k_{tz} \ . \qquad (1)$$

Since the medium is passive, we have $\mathrm{Im}k_{tz} > 0$. This is the condition for exponential decay of the transmitted wave into the medium. Note that the decay takes place irrespectively of definitions or knowledge of the Poynting vector and/or the group velocity. We are then left with two possibilities: the quantity $\mathrm{Re}k_{tz}$ can be either positive or negative [1]. If it happens so that $\mathrm{Re}k_{tz} > 0$, the wave is refracted normally or, as is often said, *positively*. Then and only then we have $\mathrm{Im}k_t^2 > 0$. If, on the other hand, the inequality $\mathrm{Re}k_{tz} < 0$ were to hold, we would observe the phenomenon of negative refraction. This can be seen, for example, by graphically representing the

[1] If it happens so that $\mathrm{Re}k_{tz} = 0$, it is not possible to tell whether refraction is positive or negative. The equality $\mathrm{Re}k_{tz} = 0$ can hold in anisotropic crystals for certain discrete values of k_x, typically, for just one single value. For our purposes, such occurrences can be safely ignored.

real part of the vector $\mathbf{k}_t$ with an arrow: in the case of negative refraction, the arrow points towards the interface. Of course, this condition is equivalent to $\mathrm{Im}k_t^2 < 0$. It should be stressed that the inequality $\mathrm{Im}k_t^2 < 0$ is also the necessary condition for the operation of the so-called "superlens" and is, therefore, physically relevant [3]. Also, it is not necessary to consider an infinite half-space; consideration of a finite slab with the correct (Sommerfeld) radiation boundary condition at the infinity would suffice. All this is discussed in our recent publication [3] and, briefly, in my reply [4] to the comment by McCall and co-authors [5].

To summarize, the criterion $\mathrm{Im}k_t^2 < 0$ is absolutely independent of the group velocity or of the Poynting vector. The conditions based on consideration of these two quantities are not irrelevant and have been discussed, for example, in References [6, 7]. However, these conditions are less general, not always applicable or physically-relevant and, most importantly, their use tends to obscure the very elementary physics of negative refraction. Given the ample simplicity of the mathematical arguments leading to the inequality $\mathrm{Im}k_t^2 < 0$, the persistent appearance in the literature of lengthy, convoluted discussions of what negative refraction "really is", such as the one offered by McCall [2], is quite shocking.

2. Form of the Poynting vector

The discussion of the correct or "preferred" form of the Poynting vector is offered by McCall on p.1733 of his review [2]. A similar but much more detailed discussion can be found in Reference [8] and, in a somewhat abbreviated form, in Reference [5], all co-authored by McCall. In Reference [2], which is the subject of this comment, McCall writes:

"The comment by Favaro et al. ... pointed out that $\mathbf{E}\cdot\mathbf{J}_b$ [$\mathbf{E}$ is the total electric field and $\mathbf{J}_b = \partial\mathbf{P}/\partial t + c\nabla\times\mathbf{M}$ is the total current induced in the medium - V.M.], is not the only way that the work done by the fields on the bound charges can be expressed from Maxwell's equations. For a Poynting theorem written in terms of $\mathbf{E}\times\mathbf{B}$, it is true that the total heating rate is expressed as $\mathbf{E}\cdot\mathbf{J}_b$ as used by Markel; however, for a Poynting theorem based on $\mathbf{E}\times\mathbf{H}$, the magnetic part of the heating is incorporated differently. Kinsler et al. [43] showed that in this case we have

$$-\nabla\cdot\langle\mathbf{E}\times\mathbf{H}\rangle = \left\langle\mathbf{E}\cdot\frac{\partial\mathbf{P}}{\partial t}\right\rangle + \mu_0\left\langle\mathbf{H}\cdot\frac{\partial\mathbf{M}}{\partial t}\right\rangle \quad \text{[Eq.(25) of McCall's review]}$$

The terms on the right-hand side of Equation (25) respectively correspond to the work done on the bound electric current and the equivalent monopole current in magnetised matter." Quote ends. Note that the brackets $\langle\ldots\rangle$ in Equation (25) of McCall's review denote time averaging.

The reader should be warned from the outset that by "equivalent monopole current" McCall means a current of magnetic monopoles which do not exist in nature. Analogously, he interprets the term $\langle\mathbf{H}\cdot\partial\mathbf{M}/\partial t\rangle$ as work done by magnetic field on moving magnetic monopoles. None of this has any grounding in physical reality. The electric current $c\nabla\times\mathbf{M}$ is not a "convenient fiction" which can be arbitrarily replaced by a current of magnetic monopoles, $\partial\mathbf{M}/\partial t$, as McCall and co-authors have claimed in Reference [8]. This can already be seen from the fact that all magnetics have nonzero gyromagnetic ratios. Therefore, a magnetized object has also a nonzero angular momentum. The classical gyromagnetic effects include the Barnett effect (when a body gets magnetized upon mechanical rotation) and its inverse, the Einstein-de Haas effect (when magnetization or de-magnetization of a freely suspended body result in its mechanical rotation). To quote S.J.Barnett [9], "Everyone who has predicted the possible discovery of any gyromagnetic effect has based the prediction on the assumption of the validity of the celebrated hypothesis of Ampere and Weber, according to which the magnetic element in a magnetic substance consists of a permanent molecular or intramolecular whirl of electricity endowed with mass and inertia."

Thus, the only source of magnetic field that exists in nature is the electric current. On several occasions, I have come across a common misconception that a single quantum spin, such as a free electron, can produce magnetic field without any electric currents. This statement is incorrect. There exists a non-zero quantum-mechanical expectation of electric current associated with the electron spin in addition to any current which is due to orbital or translational motion. The mathematical expression for this expectation is $\mathbf{j} = c\nabla\times\mathbf{m}$, where $\mathbf{m} = \langle\psi|\hat{\mathbf{m}}|\psi\rangle$, $|\psi\rangle$ is the complete wave-function of the electron, $\hat{\mathbf{m}} = -(\mu_B/s)\hat{\mathbf{s}}$, μ_B is the Bohr magneton, s is the spin magnitude ($s = 1/2$ for the electron) and $\hat{\mathbf{s}}$ is the operator of spin [10, \$ 115]. Similarly, in a macroscopically magnetized body, there exists a macroscopic electric current $c\nabla\times\mathbf{M}$; any other interpretation is unphysical and incorrect.

While McCall's excursion into the realm of magnetic monopoles has no physical standing, it is instructive to consider the mathematical arguments involved, as this would allow us to trace the historical origins of the classical Poynting's formula which involves the cross product $\mathbf{E}\times\mathbf{H}$. If we are only interested in electric and magnetic fields *outside* of material objects, then magnetization can be *mathematically* described either by the electric current $c\nabla\times\mathbf{M}$ or by a current of magnetic monopoles, $\partial\mathbf{M}/\partial t$. I will refer to these two approaches as to the First and the Second Model, respectively. The effects of electric polarization are described in both models by the same vector field $\mathbf{P}$. Let the electric and magnetic fields be $\mathbf{E}_1$, $\mathbf{B}_1$ in the First Model and $\mathbf{E}_2$, $\mathbf{B}_2$ in the Second Model. Then the Maxwell's equations take the following forms. In the First Model, the equations are

$$\nabla\times(\mathbf{B}_1 - 4\pi\mathbf{M}) = \frac{1}{c}\frac{\partial(\mathbf{E}_1 + 4\pi\mathbf{P})}{\partial t}\ , \tag{2a}$$

$$\nabla\times\mathbf{E}_1 = -\frac{1}{c}\frac{\partial\mathbf{B}_1}{\partial t}\ , \tag{2b}$$

and in the Second Model, the equations are

$$\nabla\times\mathbf{B}_2 = \frac{1}{c}\frac{\partial(\mathbf{E}_2 + 4\pi\mathbf{P})}{\partial t}\ , \tag{3a}$$

$$\nabla\times\mathbf{E}_2 = -\frac{1}{c}\frac{\partial(\mathbf{B}_2 + 4\pi\mathbf{M})}{\partial t}\ . \tag{3b}$$

It is easy to see that, if both sets of equations are solved with the same boundary conditions at infinity, one has $\mathbf{E}_1 = \mathbf{E}_2$ and $\mathbf{H}_1 \equiv \mathbf{B_1} - 4\pi\mathbf{M} = \mathbf{B_2}$. Thus, the electric fields in the two models coincide everywhere in space. The magnetic fields coincide in free space but differ by $4\pi\mathbf{M}$ inside the material.

Note that the quantities $\mathbf{P}$ and $\mathbf{M}$ are the same in both models but the constitutive relations which express $\mathbf{M}$ in terms of the fields, generally, differ. Thus, if we view $\mathbf{M}$ as a function of the magnetic field, then we have $\mathbf{M} = F_1[\mathbf{B}_1]$ in the First Model and $\mathbf{M} = F_2[\mathbf{B}_2]$ in the Second Model, where $F_1[\cdot]$, $F_2[\cdot]$ are general functionals, possibly, nonlocal, nonlinear, and multivalued. It is, however, required for consistency that, for the true solutions of both systems of equations, $\mathbf{B}_1$ and $\mathbf{B}_2$, the equality $F_1[\mathbf{B}_1] = F_2[\mathbf{B}_2]$ holds. In the case of local and linear constitutive relations, this condition is satisfied by taking $\mathbf{M} = [(\mu - 1)/4\pi\mu]\mathbf{B}_1 = [(\mu - 1)/4\pi]\mathbf{B}_2$ and, in this case, $\mathbf{B}_1 = \mu\mathbf{B}_2$.

It is interesting to note the following. If we accept the Second Model as physically correct, we can still solve the system of equations (2) (which describe the First Model) and obtain the correct results for the fields everywhere in space, as long as we assign the fundamental physical meaning to the vector field $\mathbf{H}_1 \equiv \mathbf{B}_1 - 4\pi\mathbf{M}$. Thus, if the Second Model was accepted, the true magnetic field would be given by $\mathbf{H}_1$ rather than by $\mathbf{B}_1$. Of course, in this case the expression for the Poynting vector would contain the cross product of the two fundamental fields, $\mathbf{E}_1 \times \mathbf{H}_1$, or, equivalently, $\mathbf{E}_2 \times \mathbf{B}_2$. Also, if magnetic monopoles are present, the magnetic field is allowed to do work on these monopoles and this work is expressed as $\partial\mathbf{M}/\partial t \cdot \mathbf{B}_2 = \partial\mathbf{M}/\partial t \cdot \mathbf{H}_1$.

Now we can see why the expression involving $\mathbf{E}\times\mathbf{H}$ was historically used to represent the Poynting vector. In the nineteenth century, when Poynting wrote his classical paper [11], the nature of magnetism was uncertain and displacement of magnetic poles was as plausible a hypothesis as anything else. Besides, the system of equation (3) is more symmetric as it treats the effects of electric polarization and magnetization on exactly the same footing. Correspondingly, all expressions containing the fields $\mathbf{E}$ and $\mathbf{H}$ are more symmetric and easy to handle than the expressions containing $\mathbf{E}$ and $\mathbf{B}$. For the same reason, the field $\mathbf{H}$ is known as the *magnetic field* and the field $\mathbf{B}$ as the *magnetic induction*, even though, in reality, the magnetic field is given by $\mathbf{B}$ rather than by $\mathbf{H}$; it is the $\mathbf{B}$-field which appears in the expression for the Lorentz force, for example.

J.H.Poynting himself did not give much of a thought to the physical model of magnetization nor to the energy flow *inside* a magnetized material. From some casual remarks found in Reference [11], it can be inferred that Poynting viewed magnetization as polarization of magnetic poles. For example, he defines the "electromotive intensity" (the electric field in the modern terminology) as the "force per unit of positive electrification which would act upon a small charged body" and the magnetic intensity (that would be the magnetic field) as "the force per unit pole that would act on a small north-seeking pole". Likewise, Poynting states that the energy "moves at any point perpendicularly to the plane containing the lines of electric force and magnetic force". Here the magnetic force is, evidently, the force created by the magnetic field on a unit magnetic pole. Beyond that, I could not find any discussion of magnetization of matter either in Reference [11] or in other scientific works of J.H.Poynting.

J.C.Maxwell also viewed electric and magnetic polarization of matter as conceptually similar phenomena which result from "displacement" of electric and magnetic "fluids". Of course, Maxwell understood that while the electric "fluids" could be separated, so that a given body could contain a nonzero net amount of electricity, the magnetic fluids could not, at least not in the experiments known to Maxwell. This constraint, however, was perceived by Maxwell as an "after-thought to explain a particular fact which does not grow out of the theory" [12, p.7].

These are the historical reasons for viewing the field $\mathbf{H}$ as fundamental and for using it in the expression for the Poynting vector. For many practical problems, this choice is inconsequential. However, whenever fundamental questions of electrodynamics are to be answered, we must remember that only the First Model represents the physical reality and that the true magnetic field inside matter is $\mathbf{B}$ rather than $\mathbf{H}$.

Now, there is a logical inconsistency in McCall's arguments which goes even beyond choosing an unphysical model for magnetization. Namely, in Reference [8], McCall and co-authors write (in the abstract): “The Poynting vector is an invaluable tool for analysing electromagnetic problems.” Yet, later in the text, McCall and co-authors insist that there is no single correct expression for the Poynting vector but, rather, there are four different expressions any of which is “no more or less correct” than the others. These two points of view are irreconcilable. Either the Poynting vector is not measurable and has no physical significance at all, so that the discussion of its different definitions is but a purely academic musing, or the Poynting vector must be defined uniquely. In the latter case, the different definitions can not be simultaneously correct. How is it possible for McCall to ignore such an obvious logical contradiction is unclear to me.

As for myself, I take here the middle ground. It is my understanding that the role and significance of the Poynting vector is often exaggerated. The Poynting vector is not directly measurable; the Maxwell’s equations can always be solved without invocation or knowledge of the Poynting vector; and the action of the fields on matter is fully described by the Lorentz force. On the other hand, I think that the *divergence* of the Poynting vector is a physically measurable quantity. Therefore, if different definitions result in different results for the divergence, only one of these definitions can be correct. If one wishes to retain a connection to physical reality, there is no choice but to accept the definition which follows from the First Model, namely, the one containing $\mathbf{E} \times \mathbf{B}$.

3. Experimental evidence for the reality of negative refraction

McCall has expressed an opinion on p.1734 of Reference [2] that the experimental evidence for the physical reality of negative refraction is overwhelming. With this opinion I also disagree.

To be sure, there have been a lot of experiments in which observation of negative refraction was claimed in one form or another. However, it is my opinion that all these experiments have alternative interpretations which are quite mundane and do not involve negative refraction. For example, some experiments in which negative deflection of a beam was observed [13, 14] can be explained by the action of anisotropy without invocation of negative refraction. The conceptual difference between the two phenomena was recently discussed by us in Reference [3]. Otherwise, an apparently negatively deflected beam can be one of the several diffraction lobes when the medium in question is too coarse to be considered electromagnetically homogeneous. Naive and poorly substantiated homogenization theories are often used to interpret the experimental results in situations when homogenization is not really possible. Quite often a negative index of refraction is “retrieved” from transmission and reflection data taken at normal incidence [15] (in the latter reference, the reflection data were measured at a small angle away from the normal to avoid the overlap of the incident and reflected beams). This approach was recently criticized in Reference [16]. The conclusion of the above reference is quite pessimistic, namely, it is concluded that a typical “metamaterial” used in negative-refraction experiments can not be reasonably characterized by effective medium parameters in the spectral region in which the antisymmetric (“magnetic”) resonances are excited. In this situation, retrieving effective parameters from normal incidence is a meaningless procedure. In the experiment of Reference [17], the angle of incidence was varied and it was found that, while the angle of refraction of a beam was, approximately, consistent with a certain negative value of the refractive index of a prism, the transmitted intensity was not. This suggests that the sample in question could not be adequately described by effective medium parameters. Additionally, it is not clear to me whether the refractive index of the prism reported in the experiments of Reference [17] was independent of the apex angle, and the transmitter and receiver horn antennas had 3-dB beam widths at 14 degrees, which is inconsistent with high angular resolution. Yet in one other experiment, negative refraction was claimed based on the ability to create a small spot of high electric field intensity [18], even though the possibility to create such spots is not an exclusive property of negative refraction, the size of the spot was only marginally smaller that what was termed the "diffraction-limited" size (which can be defined in more than just one way and, therefore, can vary by a factor of the order of unity), and no meaningful image of any object has been recorded.

It is not possible for me to review all claims of experimental observation of negative refraction which, perhaps, can now be counted by the hundreds. However, what seems to be much more important and relevant here, is the following. All the experiments in question are not of the type of a physics experiment in which something fundamentally unknown is tested, different hypotheses are validated and alternative theories are carefully compared. No one really doubts that all phenomena associated with negative refraction are fully understandable within the framework of the macroscopic Maxwell's equations. Further, no one doubts that refraction in any given constituent of any "metamaterial" is positive. Therefore, these experiments are not fundamental tests of nature but rather engineering attempts to manufacture a device with given properties. Both the standards and the criteria for success which are applicable to such experiments are different. It is neither required nor expected, for example, that engineers report any failures or any results or interpretations suggestive of failure because this would constitute no new scientific knowledge.

But, on the other hand, the criterion for success in this type of experiments is or should be more stringent. The success can only be judged by the degree of progress towards the declared practical goals. For example, a colleague of McCall at Imperial College, J.Pendry, has written in 2001 [19] that the applications of negative refraction materials would include "DVDs that could store 100 times more data than at present, optical lithography in the semiconductor industry with a resolution 10 times better than the current standard, and MRI scanners an order of magnitude cheaper than current models" and also that "it would be surprising if some of these applications were not realized." While I did not follow these fields of study closely, it is my impression that after a decade of very intensive investigation by multiple research groups, very little progress has been made towards these or any other previously declared practical goals, at least, not with the aid of negative refraction materials. Under the circumstances, it is not unreasonable to question whether this can be explained by technical difficulties such as high Ohmic losses in the metal (as Pendry had suggested in Reference [19]) or a more fundamental limitation is at work.

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