

Appraisal of the spin tensor

A letter from Tony van Oosten

On 9 March, 2001, after my publication,

R.I. Khrapko. True energy-momentum tensors are unique. Electrodynamics spin tensor is not zero.

- <http://arXiv.org/abs/physics/0102084>,

I received the letter from TonyVanOosten [oosten@chem.rug.nl]

Dear Dr Khrapko

Your submission to the lanl preprint server is interesting. You have a point when you say that there is something wrong with the treatment of electromagnetic spin in present theories of electromagnetism. In fact there is something wrong with the concept of gauge invariance. Just to save time and effort, please find the attached article which addresses these points.

The problem you discuss is real, but does not adversely affect the predictions of the theory, because workarounds were found. It originates from the unnecessary requirement of gauge invariance. You can't go against such a massive consensus and still expect to be published. The abolition of gauge invariance, as it appears, is totally inconceivable to the physical community.

Very few physicists appear to be willing or able to rethink fundamentals, they prefer to be part of a community and this implies accepting the consensus. Each physicist is personally responsible for the correctness of his sources, obviously. Whoever does so, unfortunately, is a dissident. <...>

Some advice: word your articles as conservative and diplomatic as you possibly can. That means: much more diplomatic and conservative than the paper you submitted. Even if you get published that does not imply recognition ...

Good luck

A.B. van Oosten

A Referee's Comments (JMO)

When submitting the paper

“Mechanical stresses produced by a light beam” J. Modern Optics, 55, 1487-1500 (2008)

<http://khrapkori.wmsite.ru/ftpgetfile.php?module=files&id=9>,

I received the Referee's Comments to Author:

Reviewer: Comments to the Author

This is a difficult paper to judge. It attempts to clarify and correct some questions in one of the 4 or so century-old controversies in classical electrodynamics, perhaps the major one of interest in modern optics. I think the paper, almost in the present form, would be a useful addition to the research literature on the topic, and I'm willing to recommend publication with minor changes. This is despite the paper being in error, in my opinion. The paper is on a topic where the literature is literately riddled with error, confusion, and dispute. The topic is of interest in practical issues in optical micromanipulation and of theoretical interest in the foundations of field theory and classical electrodynamics. Given the confused situation of the literature on this topic, I'm prepared to recommend the paper for publication despite the errors - it won't make things worse, and does make, in my opinion, a positive contribution.

Below, I list changes that I believe are required before the paper can be published, and changes that I believe could usefully improve the paper. I also believe that the editor is well within his or her rights as editor to insist that some of my recommended changes be mandatory.

The main error in the paper, in my opinion, is one of double-counting. The angular momentum transport by a light beam can be dealt with, in most cases, either in terms of the moment of the Poynting vector, or by the spin + orbital angular momenta, as done by Humblet. For example, there is a page of problems in Jackson, 3rd ed, devoted to this point. The author adds the two together, which is wrong. However, I don't think this will lead readers into error, so I don't see this as a real obstacle to publication. (But see recommended change 2).

Required changes:

- (1) A lengthy list of the rejections of the author's work doesn't help his case, and it isn't appropriate for a journal paper. Delete.
- (2) Rather than just manifesting eqn (1.6) - the spin tensor - from nowhere, it is necessary to introduce it more concretely. Furthermore, since refs 5 and 6 also introduce spin tensors, the author needs to describe explicitly how his spin tensor differs from theirs. I'm assuming it is different, since if it doesn't, what would be the point of the current paper? This needs to be made clear.

Recommended changes:

- (1) Many of the figures are taken from published sources. Perhaps the author could produce conceptually equivalent, but new, figures to avoid troublesome copyright issues.
- (2) The author errs by double-counting the angular momentum. Perhaps it would be better to not consider the edge effects by only considering the case of an infinite plane wave. Focussing on this would lead to the heart of the controversy, wherein a sizable body of opinion claims that a circularly polarized plane wave carries no angular momentum (despite the work by Sadowsky, cited and summarised by Beth).
- (3) The paper could be written in a less polemic style, to advantage.
- (4) The author could usefully widen the literature review, the papers Zambrini and Barnett in *JMO*, van Enk's papers, the paper by Crichton and Marston in *EJDE*, etc.

My answer to the Reviewer

Dear Reviewer:

The orbital torque and the spin torque are separated by space. So, I think, a double-counting is possible only in the case of a violation of the localization principle.

Sorry, I could not find van Enk's papers

Next Referee's Comments (JMO)

My next submission to *JMO* was "**Experiments for Determination of Angular Momentum Flux Density**" (see <http://khrapkori.wmsite.ru/ftpgetfile.php?id=46&module=files>)

I received the Referee's Comments:

I have had the "pleasure" of reviewing a large number of papers by this author on his alternative theory of optical angular momentum. At one stage, I recall, his manuscripts **proudly** proclaimed the long list of journals that had rejected his work. The author believes that there is an additional spin angular momentum for the photon, that is not present in standard (Maxwell-based) theory and all of his papers that I have seen are based on this, shall we say "dubious" idea. The conventional (Maxwell and Poynting - based) theory of optical angular momentum is in excellent agreement with all recent experiments and there is no need nor evidence for any correction of the type **envisaged** by the author

My answer to the review

Dear Professor **Marangos**,

There is a puzzle.

The question is: whether the reviewer has not read my paper **proudly** or he is not inclined to **envisage** results of the Righi experiment. In any case this type of comments is inadmissible. There is no word concerning the content of my paper in the comment. So I attach a paragraph from my paper.

“Both the results mean a considerable nonlocality of the electrodynamics. Really, if the inner part does not perceive a torque, spin angular momentum of a photon is absorbed on peripheries of the body while energy of the photon is absorbed on the inner region. If the inner part does perceive a torque, Maxwell stress tensor of electromagnetic field does not have a local sense because, according to (4), [4,5], this tensor provides no tangential forces in the inner part”.

The reviewer of my paper *JMO* 2008, 55, 1487 was less proud than this reviewer.

A Referee's Comments (OL)

My submission to Optics Letters was "**The Beth experiment is under review**".

I received the Referee's Comments to Author:

Reviewer 1: The result, dealing with matters at the heart of the rather confused matter of electromagnetic angular momentum, is interesting and merits publication. The paper is concise, and likely as clear as possible, given the subject matter. <...>

The submission was rejected