Absurdity of Gerard 't Hooft Chief Editor Foundations of Physics

A reply to a rejection by FOOP

http://groups.google.ru/group/sci.physics.electromag/browse_thread/thread/0bf031457f58b566#

It took a lot of chutzpah to write the following in response to submitting of my paper **"Experiment concerning electrodynamics' nonlocality" [0]**:

"The approach chosen does not befit the described phenomenon. The electron is of an utterly quantummechanical nature, and can therefore not be described by classical means."

In reality, this paper offers to interfere two laser beams, which pass through two half-wave plates, one of which is divided into two parts concentrically. Each of the parts of the plate can be rotated manually independently. Because of such a rotation, the torque works on the plate, and the light frequency changes. It results in a movement of interference fringes. Observing of this movement makes it possible to determine, where the torque is applied. No electrons are considered

Besides the electron, Gerard 't Hooft uses his stock phrases in this rejection:

"Before entering a submission to the reviewing process, we check whether it obeys criteria such as the following: - Is the topic of research suitable for this journal?
- Does the paper contain original ideas and new results?
- Are the arguments and calculations accurate and correct?
- Is the exposition sufficiently well organized, and worded well?
- Does the overall quality agree with our very tough standards?

I regret to inform you that the editors had to conclude that this work is not suitable for publication in Foundations of Physics. I would like to thank you very much for forwarding your manuscript to us for consideration and wish you every success in finding an alternative place of publication. With kind regards, Gerard 't Hooft Chief Editor"

So, I appealed this rejection.

Dear **Gerard 't Hooft**:

Your messages of May 04, 2010 and May 19, 2010 convince me that my paper FOOP1819 **"Experiment concerning electrodynamics' nonlocality"** merits publication in FOOP because, unfortunately, its content is helpful even for editors and reviewers. Please read the following analysis. Note, I am forced to consider reviewers' opinions (red) as your opinion because of the anonymity of the reviewers.

The submitted manuscript is about the angular momentum density of circularly polarized light beams. It can be shown within Maxwell formalism that the angular momentum of a circularly polarized electromagnetic field contains a (spin) term that is a function of the radial gradient of the field (as given in Eq. 4), so this term vanishes in uniform regions -where the field gradient vanishes-.

You are right, but in part. You cannot affirm that this angular momentum is spin. Electromagnetic fields of a circularly polarized light beam [16],

$$\mathbf{E} = \omega\exp(ikz - i\omega t)[\mathbf{x} + i\mathbf{y} + \frac{1}{k}\mathbf{z}(i\partial_x - \partial_y)]u(x,y,z) = \omega\exp(i\varphi + ikz - i\omega t)(\underset{\rightarrow}{\rho} + i\rho\underset{\rightarrow}{\varphi} + i\frac{1}{k}\underset{\rightarrow}{z}\partial_\rho)u(\rho,z)\,, \quad (2)$$

($\underset{\rightarrow}{\rho}, \underset{\rightarrow}{\varphi}, \underset{\rightarrow}{z}$ are *covariant* coordinate vectors) is an eigenfunction of the *orbital*, not spin, angular momentum operator $-i\hbar\partial_\varphi$.

Further, according to [2,4], z-component of the angular momentum volume density is

$$j_z = -\varepsilon_0\omega r\partial_r|u(r)|^2/2\,. \quad (4)$$

This density is proportional to the radial gradient of the light beam intensity while the energy volume density in the beam, w, depends on the intensity itself:

$$w = \varepsilon_0\omega^2|u|^2\,. \quad (5)$$

So, the ratio between the densities,

$$\frac{j_z}{w} = -\frac{r\partial_r |u(r)|^2}{2\omega |u(r)|^2}, \quad (6)$$

changes from place to place considerably while the ratio between spin density and energy density of photons is constant, $1/\omega$. This fact gives an alternative: either density (4) is not a spin density, or the electrodynamics is not local. In the paper, we propose an experiment to detect this ratio.

In general, the total (integral over space) angular momentum per unit of energy is inversely proportional to the monochromatic beam frequency. The question of the gradient form of the spin density was considered, for instance, in the 70's in Ref.[5], pointing that a field can be uniform and not vanishing only within a finite region. Therefore the transverse profile of a circularly polarized light beam will necessarily decreases somewhere introducing field gradients and leading to spin angular momentum.

Again, you cannot affirm that the spin density must be of the gradient form. Why spin must be connected with gradient of energy density and not with the energy density itself? Note, a moment of the linear momentum, $\mathbf{j} = \mathbf{r} \times (\mathbf{E} \times \mathbf{B})$, that is an *orbital* angular momentum, is connected with the gradient (4).

This argument is generally accepted also to explain torque effects of plane waves shining on birefringent objects. An interesting question was raised ten years ago in Refs.[11], about the rotation of an absorber consisting on an internal disk fitting within an annulus when shined by a circularly polarized wave. This question was answered, for instance, in Ref. [12] in 2002, with an argument based on a field decomposition like in Eq.(11) of the submitted manuscript. This and other arguments -the author refers to Feynman and Beth- consider the question of local spin effects from different angles always predicting spin transfer (torque on shined objects) also in the central flat area of these beams, in spite of the fact the spin density for the beam (not absorbed/refracted by any object) is vanishing. As a matter of fact, contrary to the author claims (lines 29-30 of pg.3 of the submitted manuscript), also the field decomposition proposed in [12] leads to this conclusion.

Again, how can the spin be transfered into the central area of the beam from its surface where $\partial_r(u^2)$ (4) is not zero? Note, there is no radial angular momentum flux in the beam.

How can the internal disk experience a torque if the angular momentum, according to (4), is in the beam surface only? Note, Professor Robert H. Romer valiantly published my question [11] and resigned.

The Allen & Padgett's answer [12] on my question [11] was criticized in [9]. Note, no one is against the criticism. And really, Allen & Padgett [12] represent our beam as the superposition of two parts,

$$u(r) = u_{in}(r) + u_{out}(r), \quad (11)$$

such that the radius of the inner part is approx $r_1 < R$ and the outer part looks like a thick-wall tube located approx between r_1 and R. The authors conclude the inner part of the absorber does perceive a torque because $\partial_r(u_{in}^2)$ of (4) is not zero. However, I think we must take into account the both components of the decomposition and the interference between them. Then we obtain zero for the torque density (4) at any point of inner or outer part of the absorber, except the surface of the beam. Indeed, according to (4),

$$j_z = -\varepsilon_0 \omega r \partial_r |u(r)|^2 / 2 = 0$$

at any point of inner or outer part of the absorber except the surface region, and the zero will hold when the decomposition (11) is substituted into (4). I have already explained that a decomposition of the beam field into inner & outer parts cannot create a torque despite Allen & Padgett's belief [12].

Contrary to the Allen & Padgett's zero, the concept of Feynman and Beth predicts the torque at the central part of the absorber. The aim of my experiment is to observe torque (4) at the edge of the beam and the spin torque at the central part.

Here the author considers again this question presenting it as an open one, in spite of many experiments with tweezers where circularly polarized beams with different spatial profiles always rotate trapped particles across the beam. The author's proposal is to shine a circularly polarized beam on an object with the same disk-annulus geometry as in Ref.[11] but being now birefringent instead of absorbing. The proposal is to measure the ratio of spin angular momentum density and energy density through a frequency shift (Righi experiment) detected with an interferometer.

Yes, circularly polarized beams rotate trapped particles, but there is no experimental determination of the angular momentum *distribution* across the beam section. Such an experiment is brought forward in this paper.

It is not clear if the object is rotated by light or manually (as mentioned -only- in the abstract). The last sentence suggests the possibility to use Gaussian beams that actually are already well-known to be able to rotate birefringent particles placed on axis.

The last sentences show that the author understands nothing in this experiment essence. We offer to interfere two beams, which pass through two half-wave plates, one of which is divided into two parts concentrically. Each of the parts of the plate can be rotated manually as in the Righi experiment, but independently. Because of such a rotation, the torque works on the plate, and the light frequency changes. It results in a movement of interference fringes. Observing of this movement makes it possible to detect, where the torque is acted.

Finally, this paper reports, not always precisely, on previous results, and contains a very specific proposal of a rather cumbersome experiment with a half-wave plate with a disk-annulus geometry to observe local spin effects already largely studied, for instance, in tweezers. The presentation is not very accurate and rather misleading, starting from the title. Therefore I do not recommend it for publication in Foundation of Physics.

No, the experiment is not cumbersome. The experiment is a simple modification of a simple experiment performed in 19-h century [13]. Local spin effects were not studied in tweezers. The Beth experiment [3] and many experiments on micro particles with tweezers confirm a presence of an angular momentum in a circularly polarized light beam. Unfortunately, now we have no experimental determination of the distribution across the beam section.

While the problem the paper adresses is very interesting, I think that the paper could be considered for publication only after a major revision. I think that papers aimed to clarify some controversial issue, has to be as clear as possible from the mathematical point of view, placing in simplest experimental situation conceivable. This paper, as it stands, fails to meet this requirement, actually generating more confusion.
For example, just in the beginning of the paper it is not told the equation to which the function u(x,y) (probably the dependence on z in formula (1)-(3) is a misprint) has to satisfy, and this is crucial to decide if u=cont inside a circle is acceptable or not (I suspect not because probably u(x,y) has to be harmonic, and so it cannot be taken piecewise constant). So from the mathematical point of view the procedure becomes unclear, and the paper confused.

The fields

$$\mathbf{A} = \exp(ikz - i\omega t)(-i\mathbf{x} + \mathbf{y})u(x,y,z), \quad (1)$$

$$\mathbf{E} = \omega \exp(ikz - i\omega t)[\mathbf{x} + i\mathbf{y} + \frac{1}{k}\mathbf{z}(i\partial_x - \partial_y)]u(x,y,z), \quad (2)$$

$$\mathbf{B} = \exp(ikz - i\omega t)[-ik\mathbf{x} + k\mathbf{y} + \mathbf{z}(\partial_x + i\partial_y)]u(x,y,z), \quad (3)$$

satisfy the wave equation in the paraxial approximation, which is widely used [7,2]. The paraxial approximation implies $\partial_z u << ku$ and $\partial^2_{xx}u + \partial^2_{yy}u + i2k\partial_z u = 0$. A wide class of beams satisfies the paraxial conditions. We consider, together with [1,5,6], a wide beam and, in accordance with [5], we have made $u = u_0$ constant over a large central region of the beam and confined the variation of the function u from u_0 to zero to lie within a "skin" which lies a distance R from the axis (see[5, Fig. 9.3]; [6, Fig.1]; our Figure 1a).

I suggest the author to simply take a plane wave, for which is obvious that the component of the angular momentum of the field in the propagation direction vanishes, solves (numerically or analytically) the problem stated by Feynmann in its lectures (bound electron in the field of the wave, on the Landau book it is solved the free electron problem), and check if the total angular momentum (field + electron) is conserved or not. I suppose the momentum will be conserved, because for what concern the electric field one has to consider also the Coulomb field of the electron. This taken into account, the angular momentum of the field in the direction of propagation turns out probably not vanishing, so that the contradiction disappears. If it was not the case the result would be of paramount importance.

Feynman did not consider a free electron. A free electron is beyond the topic. Feynman and Beth considered a dielectric medium. Respectively, I use such macroscopic quantities as components of the Maxwell energy-momentum tensor & their moments. The fact of paramount importance is that there is no flux of torque to the inner part of the absorber in the frame of Maxwell electrodynamics. My experiment is aimed to measure the flux of torque to the inner part of the absorber.

But, a part the previous suggestion, if the mathematical analysis of the problem is not clear, whatever experiment we perform, we do not gain any knowledge, because we don't know how to interpret the result in the frame of our theory. So, before suggesting any experiment, one would correctly solve the equations for the propagation of the electromagnetic field in this context, and derive what are the consequences to be observed. This is the part that, in my opinion, is insufficient in this paper. If the author will improve substantially this part, the paper would be acceptable for publication.

All equations for the propagation of the electromagnetic field in this context are correctly solved in the frame of the paraxial method, and the consequences to be observed are derived in the smallest details.

Improvements should be: first of all write down the equations of motion which describe the problem (classical or quantum electrodynamics, microscopic as in Feynmann approach, or macroscopic as in reference [1]). Quantum physics is different from the classical one, and it is not clear that in this context the answers turns out to be equal. Then give the exact solution (if possible) or more likely an approximation to such a solution (with a discussion aimed to show how good this approximation is). Finally, compute from this solution, the magnitude of the expected effect to compare with the experimental findings.
I believe that after a revision along the lines suggested, the paper would be suitable for publication.

The program is fulfilled.

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