March 5, 1991

The Editor(s)
Physical Review A
1 Research Road
Box 1000
Ridge, New York 11961

Dear Mssrs. et Dames,

I am enclosing various materials originally accepted by you for active consideration for publication in Physical Review A on November 30, 1981. I have not made any changes in these materials in the hope that they might be published with the original submission date.

I am also enclosing a copy of the referee's original notes. The referee's comments are the classical view of electromagnetic propagation for wave structures larger than one wavelength. The whole of this paper deals only with electromagnetic structures of sub-wavelength dimensions. It is, in fact, the primary point of the paper to propose a new sub-wavelength structure for electromagnetic radiation.

As with a standing wave in a string, energy may indeed be zero at discrete points along the axis of propagation. And, as with a physical string, energy is a more useful term than energy density.

With a propagating (non-standing) wave in a string, the (kinetic) energy values are non-zero (in fact are maximal) for points where the string crosses the axis of propagation; whereas energy values calculated by means of any classical technique using Maxwell equations (including the integral of energy density) results in zero energy within a wavelength at the same points.

The point of the paper is that this discrepancy may be eliminated by the superposition of a mass wave in energy contraposition to the electrical and magnetic field waves.

At the time this paper was submitted, very few physicists (and therefore, I would imagine, referees) were intellectually familiar with, or politically comfortable with, Michael Green's work in London. (I myself was unaware of it, and indeed, much of it had yet to be published.) His work on the unification provided by superstring theory was not published until 1984.

Today, of course, superstring theory is arguably the most popular unification theory among physicists. By retaining the original date of submission, I would hope to make it clear where Resonance Theory lies in the chronology of superstring theory.

As for the comments by the referee that the general level of the paper is elementary, he(she) is of course correct. It is the point of the paper to present a simple description of a straightforward application of conservation theory in a universal geometry defined by the axis of electromagnetic propagation.

I would not propose that the language of the paper is as current in jargon or even scientific politics as might be expected of an author seeking to be published in the Review, but the ideas in this paper are not in any way naive. I say this having discussed these ideas at the time with Roger Penrose, David Bohm, David Boulware, Joseph Cronin, John Cramer, William Bender, William Matheson and others, some of whom I have acknowledged on p. 24.

I do not, of course, know who the referee was, but I assumed that the credibility of these individuals was such that he (she) would give greater credence to the ideas expressed in this paper than the simple rejection of physical string mathematics for the propagation of electromagnetic radiation.

I would like to add that I understand how strange this approach must seem when compared to the classical model, although the progress made in superstring theory in the last ten years has turned this from a revolutionary concept to a (more) politically correct one. In addition to the above discussions of the ideas contained in the paper, I have submitted the paper in its entirety to Bill Matheson for review, and spent many hours discussing these and derivative equations with William Bender, professor emeritus of physics at Western Washington University. Fairly stated, their opinion was that the theory was successful from the point of view that 1) it did not contradict any current experimental knowledge, and 2) that it was internally consistent. I have left the third theoretical test of providing experimental predictions for a later paper on the application of this approach to thirteen major applications of force laws.

In other words, I have done considerable additional work beyond the scope of the current draft to assure that this is indeed a practical and worthwhile path to follow. Contrary to the referee's comments, I would state in the most confident manner that the Maxwell equations are indeed 'vulnerable' on this and other issues. (As I am sure you are aware, Jim Brittingham of the Lawrence Livermore Nuclear Lab provided a radically new set of Maxwell solutions in 1987.) I would add that there are additional benefits to be drawn from the definition of mass wave and charge as defined by this Resonance Theory, not the least of which is an improved physical definition of empty space.

If there are true deficiencies in the logic or physics of the paper, I of course would like to be apprised of this in order to make the required changes.

I realize that this is an unusual situation, and would appreciate your assistance in completing the process of bringing this material to publication.

Thank you.

Sincerely,

Mark R. Anderson

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