

Dear Editor,

I am immensely grateful for the opportunity you have given to my work and I accept your decision of rejection. Nonetheless, I want to underline the inadequacy of the criticisms raised by your Reviewers. Therefore, I kindly ask you to take a few minutes to read what follows. It will be shown how these Reviewers have again falsified the argument developed in my work.

First of all, Reviewer 1 associates the solution used in the manuscript with a plane wave. But there is no truth in this association. The multipole expansion of classical electromagnetism never appears to be an expansion of a plane wave (see Sect 9.6 in J. D. Jackson, *Classical Electrodynamics*, Wiley, New York, 1999, third edition). Indeed, the plane wave is the limit of very large distances of a spherical wave, the latter being the exact association with my work. Is this so hard for a Reviewer to understand? At a more general level, pages 557-559 in Arfken and Weber, *Mathematical Methods for Physicists*, Elsevier (2005, 6th ed.) make a manifest disproof of what this Reviewer affirms.

In addition, Reviewer 1 criticizes the limit of $kR \gg 1$ as though this limit determines “special conditions on the electromagnetic field”. But, the limit of very large sizes (or very large R values for a finite k) is typical of the canonical quantization. This is written everywhere and this is the reason behind the common change from a discrete distribution of modes to a continuous distribution. Therefore, if “special conditions on the electromagnetic field” apply to my work, the same can be argued for the canonical quantization where “we imagine the electromagnetic field to be contained in a very large cube of side L and... at a suitable stage in the calculation we then allow L to tend to infinity” (quote from page 467 in L. Mandel, E. Wolf, *Optical Coherence and Quantum Optics*, Cambridge University Press, Cambridge, 1995). Very large sizes of the quantization volume are common in quantum optics!!! Therefore, this Reviewer is in remarkable contradiction.

But the best example of the prejudice I have to contrast in defending my work is provided by the second Reviewer, who forces my work into reaching conclusions that are not present in the manuscript. Indeed, the Reviewer affirms that the most important mistake “is the fact that present paper predicts that a cavity with a single node would only have one excitation”. But, my work refers to the free space, or empty space!!! Why does this Reviewer falsifies my work by applying its results to cavity electrodynamics that implies the presence of mirrors at very short relative distances? Even worse, this Reviewer wants to stretch the truth by arbitrarily adjusting my work to the special (and most extreme) case of cavity quantum electrodynamics of one node, the latter being a research field where I made several contributions in the past (together with Prof. De Martini in Rome and later with our beloved Prof. H. Walther at the Max-Planck Institute of Quantum Optics in Garching, see for example Marrocco, M. Weidinger, R. T. Sang, and H. Walther, *Physical Review Letters* 81, 5784 (1998); or De Martini, Marrocco, Murra, *Physical Review Letters*, 65, 1853 (1990); or De Martini, Marrocco, Mataloni, Crescentini and Loudon, *Physical Review A* 43, 2480 (1991)).

In conclusion, I regret to accept your rejection. I had hoped that more favorable comments would have been written this time in comparison to the former submission (PLA-D-15-00904) regarding which I have already shown the prejudices of your Reviewers (see my replies to them). But, it seems that the prejudice is the persistent guidance of your Reviewers. Their new comments are contradictory and easily disputable up to the point that their inconsistency does not require much effort to be shown.

Once again, I have appreciated your support.

Your sincerely,

Michele Marrocco