Reply to Reviewer 1

The stimulating criticisms raised by this Reviewer have been attentively examined. Most of them are of minor importance because they refer to questions that do not regard the main subject of the manuscript. Nonetheless, they contain fundamental questions that deserve attention. Instead, the final criticisms (i.e., arbitrary component of the electric field and unexplained opposition to monopoles) are crucial because they are against the rationale of the work. The rebuttal is organized by replying to each criticism in a detailed manner.

Reviewer 1 writes:

In the introduction the author interprets the zero-point energy of the electromagnetic field in terms of half photons. It has a different origin. The author should read Glauber's 1963 paper on coherent and incoherent states of the radiation field

Author's reply:

Before any comment on Glauber's paper, it must be made clear that it is not this Author who interprets the zero-point energy of the electromagnetic field in terms of half photons. But, even if the Reviewer was right, the Author would be proud of being in very good company. Indeed, <u>Nobel laureates interpret the zero-point energy of the electromagnetic field in terms of half photons</u>!!! It suffices to read the abstract of one of the well-known Haroche's papers (Brune et al., Phys. Rev. Lett. 72, 3339-3342 (1994)). In the abstract, the Reviewer can read: "*The vacuum induced Lamb shift, equal to the light shift of "**half a photon**," has been observed, providing a direct and absolute measurement of the zero-point field fluctuations in a cavity mode*". Therefore, I am confused. Should I follow the 2012 Nobel laureate Prof. Haroche or the Reviewer? Despite the confusion, it seems that <u>distinguished and recognized physicists incline to the Haroche's position</u>. For instance, a collaboration between two groups from Yale and Berkeley Universities came out with the following acknowledged paper: Bergeal et al., Nature Phys. 6, 296-302 (2010). In the main text the Reviewer can read: "*Assuming thermal equilibrium with temperature T, each port is fed at its input with **half a photon** of noise arising from vacuum fluctuations*". Other examples include books and public lectures, but the above-mentioned examples are enough to support the claim that the Author of the submitted manuscript followed the stream of physicists who have used the term "half a photon". Nonetheless, in the revised manuscript, the term "half a photon" has been eliminated.

Next, coming to the zero-point energy in the suggested Glauber's paper, I am well aware of the Glauber's approach to the subject of coherent and incoherent states of the radiation field (Phys. Rev. 131, 2766-2788 (1963)). Unfortunately, the Reviewer has forgotten that the zero-point energy is related to Fock states [or photon-number states, or states of the quantum harmonic oscillator: see section 10.3, "Canonical quantization of the transverse field", pages 473-475 in L. Mandel, E. Wolf, Optical Coherence and Quantum Optics, Cambridge University Press, Cambridge, (1995)]. But Fock states are different from the coherent and incoherent states treated in the mentioned Glauber's paper!!! And the lowest Fock state is the vacuum that gives rise to the criticized "half a photon". In the submitted manuscript, Fock states are considered (note the integer value of the occupation number n) in demonstrating the equivalence between Eq. (1) and Eq. (24). Therefore, Glauber's 1963 paper is not appropriate for the context of the submitted manuscript.

In more detail, a closer inspection of the suggested Glauber's paper may help to strengthen my opinion on the Reviewer's criticism. Indeed, the mention of zero-point fluctuations occurs three times (a first time on page 2771, left column, four lines below equation 3.30; a second time on page 2772, left column, sixth and seventh line from the top; a third time on page 2779, right column, seventh line from the top). In all cases, the origin of zero-point fluctuations is treated in reference to coherent states only. To prove this, we do not have to go far. It would be enough to read the title of the paper that refers to coherent and incoherent states. However, we can use Glauber's words. On page 2771 of his paper, after the mention of the amplitudes of zero-point fluctuations appearing in equations 3.29 and 3.30, Glauber writes that “the fact that the wave functions for the coherent states have this elementary structure (cf., amplitudes of zero-point fluctuations of equations 3.29 and 3.30) should be no surprise in view of the way they are generated in Eq. (3.13), by means of displacements in the complex alpha plane”. It results that Glauber relates the appearance of zero-point fluctuations of coherent states to the application of the displacement operator that shifts the coherent state away from the vacuum. But, the displacement operator is limited to coherent states and these only!!! Under this restriction, Glauber emphasizes that zero-point fluctuations of coherent states emerge through the correspondence with the quantum harmonic oscillator (see equation 3.27 combined with equation 3.13) that characterizes Fock states. Therefore, Glauber does not explore and neither investigate the nature of the zero-point fluctuations that are associated with Fock (or photon-number) states. Instead, Glauber clarifies that the zero-point fluctuations of coherent states are the consequence of zero-point fluctuations of Fock states. That is all. To conclude, the Reviewer has erroneously placed the origin of zero-point energy in the space of coherent states. Instead, the origin is found in Fock states that represent the reference states of the electromagnetic field treated according to the principles of the quantum harmonic oscillator. This is written in any of the referenced textbooks, but I suggest the one of highest level [section 10.3, “Canonical quantization of the transverse field”, pages 473-475 in L. Mandel, E. Wolf, Optical Coherence and Quantum Optics, Cambridge University Press, Cambridge, (1995)]. The zero-point energy emerges in the Hilbert space of harmonic oscillator operators that operate on Fock states or number states!!!

Reviewer 1 writes:

on page 6 the author writes: “A detector cannot follow the fast time oscillation of the electromagnetic field" This general statement is wrong. There are the so-called Schumann waves with less than 100Hz.

Author’s reply:

This criticism shows a typical example of how a sentence made in a specific context is inaccurately used in a completely different context. Indeed, the criticized sentence has been made in the context of quantum optics. But, the Reviewer has taken an example belonging to geophysics!!! Indeed, the Schumann waves are created by lightning discharges in the cavity formed by the Earth's surface and the ionosphere. By borrowing the Reviewer’s logic, one could say that quantum mechanics is wrong because the moon does not have quantized energies that describe its orbit around the earth. In this sentence quantum physics is contrasted with an example of geophysical meaning. But, the sentence is clearly false because the comparison is meaningless. Despite this, the Author has modified the sentence in order to clarify the context of quantum optics, even though this change is redundant in comparison with the matter treated in the submitted manuscript that has nothing to do with electromagnetic fields of geophysical interest (of which Schumann waves are an example)!

Reviewer 1 writes:

“on page 7 the author writes: "...the electric field rather than the vector potential that is never measured." Again wrong. The gauge-invariant part of A can be measured in a Bohm-Aharonov setup.

Author’s reply:

The Author confirms the criticized sentence. The sentence is common knowledge in physics. For example, the Reviewer can find it in the well-known book by Griffiths (D. J. Griffiths, Introduction to Quantum Mechanics, Prentice Hall (1995). Section 10.2.4 entitled “The Bohn-Aharonov effect”, page 343), it is written that “*in classical electrodynamics <u>the potentials (phi and A) are not directly measurable</u>, the physical quantities are the electric and magnetic fields*”. Therefore, the criticized sentence, made in the context of classical electrodynamics, is correct and agrees with Griffiths. In addition, what is measured in a Bohn-Aharonov set-up is a phase difference in signals of real observables. This is so true that can be easily verified by reading one of the feature articles published by Tonomura (Physics Today, Sept. 2009, pages 38-43), the scientist who gave the first clear evidence of the effect in 1986. Different sorts of signals are used to measure a phase difference that is interpreted in terms of the path integral of the vector potential. Therefore, it is an overstatement to affirm that the vector potential can be measured. It is however true that the phase difference of a meaningful signal is used to determine the effect of the path integral of the vector potential. This becomes evident as soon as one reads fundamental experimental works on the Bohn-Aharonov effect. A couple of examples are Tonomura et al. Phys. Rev. Lett. 56, 792-795 (1986) where the phase difference in electron interference patterns is measured, or the much-acclaimed work by Bachtold et al. in Nature 397, 673 (1999) where magnetoresistance measurements are made to determine the Bohm-Aharonov effect. The measurements are made by using signals that are related to the vector potential through the path integral. Therefore, the Author of the submitted manuscript is right: the vector potential is never measured in the classical context of the manuscript (classical electrodynamics).

Reviewer 1 writes:

worst of all, the author writes down an arbitrary component of the electric field...

Author's reply:

The component of the electric field is not arbitrary. It surprises to realize that the Reviewer does not know that equation 16 corresponds to solution of the Helmholtz equation (see equation 15) for the empty space described by the spherical system of coordinates. This is a fundamental result of mathematical physics. See the well-known books about mathematical methods in physics. Here, the Reviewer finds some renowned suggestions: pages 557-559 in Arfken and Weber, Mathematical Methods for Physicists, Elsevier (2005, 6th ed.) or pages 314-315 in Courant and Hilbert, Methods of Mathematical Physics, Interscience Publishers (1953, Volume 1). In particular, in the more recent book by Arfken and Weber, the Reviewer finds table 9.2 where very general solutions are reported. For the spherical symmetry of our problem set in the empty space, we have the solution given in the manuscript. However, it is rather ambiguous the contrast between the rejection of the component of the electric filed in Eq. (16) (see also the criticism below) and the general acknowledgement of the correspondence of Eq. (8) between the classical amplitudes and the amplitudes of the creation and destruction operators. Indeed, the correspondence of the conventional quantization procedure is realized by forcing the agreement between classical and quantum energies. No demonstration is given in this respect. We can only postulate the quantum-classical correspondence of the conventional quantization and this is the origin of the amplitudes that multiply the creation and destruction operators in Eq. (8). Along the same lines as the correspondence of the conventional quantization, we could write down the electric field component that satisfies the Helmholtz equation and, at the same time, realizes the correspondence with Eq. (1). If Eq. (8) is designed to realize the conventional agreement between the classical and quantum energies, one could equally accept that Eq. (16) is designed to realize the same agreement. Nonetheless, Eq. (16) can be proven without forcing the correspondence.

Reviewer 1 writes:

This is not physics, this is only a normalization property of the harmonics. Equation (16) is meaningless.

Author's reply:

The Reviewer should be more cautious about what is or is not physics. Indeed, Eq. (16) is in strong analogy with free propagation of quantum mechanics. The argument developed in the context of classical electrodynamics of the submitted manuscript is not formally different from what is concluded in quantum mechanics where for the free particle an analogous Helmholtz equation is established as a direct consequence of the Schrödinger equation (or the eigenvalue problem) written in spherical coordinates for the kinetic term only. In this circumstance, the corresponding quantum-mechanical eigensolutions are made of spherical Bessel functions that multiply spherical harmonics and it is straightforward to verify that the combination of these special functions reproduces the structure of Eq. (16) of the submitted manuscript. Three examples are given below and many others could be cited: 1) A. Messiah, Quantum Mechanics, Dover Publications (1999). Chapter 9, page 357. 2) R. Shankar, Principles of Quantum Mechanics, Plenum Press (1994). Chapter 12, page 349. 3) D. J. Griffiths, Introduction to Quantum Mechanics, Prentice Hall (1995). Chapter 4, page 132. The list could go a long way, but these examples are enough to show that it comes as no surprise to discover that the peculiar structure of the criticized Eq. (16) is in close analogy with quantum mechanics of free particles or free waves. Besides, taking the famous book by Griffiths in the example 3, let's have a look at the lines below equation 4.51 [the equivalent of the criticized Eq. (16)]. It is written that each energy level is (2n+1)-fold degenerate and this degeneracy is what appears in Eq. (18) of the submitted manuscript!!! Maybe, the Reviewer does not like the result of this work, but it is a fact that physics is often made of math! By the way, the normalization property of the spherical harmonics is physically important in many branches of physics: classical electrodynamics, classical optics, acoustics, geophysics (Refs. 8-11 of the submitted manuscript), and beyond these examples of classical physics, the normalization is central to the quantum-mechanical determination of orbital angular momenta (see Ref. 3, pp. 519-523).

Reviewer 1 writes:

Equation (16) is meaningless. One could also divide it by sqrt[(2n + 1)] and there would be no (n+1/2) in the final result.

Author's reply:

It is impossible to fit a remark of this sort into the rigorous references that support the calculation explained in the text of the manuscript. Let us take Ref. 8. In particular, page 433, Eq. (9.136) of Ref. 8. There, the normalization of the spherical harmonics is used and leads to the physical result that "*the total energy in a spherical shell in the radiation zone is thus an incoherent sum over the multipoles*" [quotation from Jackson, see the lines after Eq. (9.136) cited earlier]. The incoherent sum reduces to the 2n+1 factor of the manuscript because, in the empty space, each multipole contributes in the same manner. Therefore, the suggested factor of sqrt[(2n + 1)]) is unreasonable. Once again, by borrowing the peculiar logic of this Reviewer, we could use the same arbitrary factor of sqrt[(2n + 1)] in the above-mentioned textbooks of Messiah, Shankar and Griffiths to criticize the basics of quantum mechanics of free particles (or free waves) that are characterized by the identical spatial dependences of Eq. (16).

Reviewer 1 writes:

Coming up with monopoles out of the blue sky will make the paper even worse.

Author's reply:

This criticism is not supported by an explanation. Why do monopoles make the manuscript worse? The information is missing. Given this deficiency, the Author makes the effort to find a possible answer. First of all, the introduction of the monopole does not come "out of the blue sky". It appears within the discussion about the interpretation of the quantum number n and, for n=0, we have the monopole. This corresponds to the fundamental solution of the multipole approach and its introduction is coherent with the flow of the text. Secondly, in a problem of electrodynamics where the multipole approach is used, the presence of the monopole is very common (see J. D. Jackson, Classical Electrodynamics, Wiley, New York, 1999). Why this should be worse is not known to the Author.