Reply to Referee (#2) Dr. Christopher Essex: Comments on "Spectral Solar Irradiance and Its Entropic Effect on Earth's Climate"

Thank you very much for your careful reading and useful comments. Based on your comments, we have carefully revised our manuscript, adding some context, re-writing some parts and expanding some explanations. We hope we have addressed your concerns. Detailed reply to your comments is listed in the following.

I dislike it when referees point to their own papers but remain anonymous. This topic is particularly awkward for me in this regard because for about ten years, beginning in the early 80's I was one of very few writing on radiation and the global thermodynamics of the atmosphere. My paper, C. Essex, Radiation and the Irreversible Thermodynamics of Climate, The Journal of the Atmospheric Sciences 41 (1984), 12 1985-1991, was the first to make clear that previous attempts at global thermodynamics had left out the entropy of the radiation field, and furthermore that from a thermodynamical point of view this was a significant oversight. Beforehand the major discussion was in terms of fluid dissipation and there was little grasp of the mechanics of entropy radiation. It was a significant change in thinking that the entire entropy production rate of the Earth was representable in terms of the radiation field alone. Afterward things changed substantially. Beforehand, it was a difficult struggle to get referees to even agree to publish such a paper because they had little or no knowledge of radiation entropy or how to work its mechanics. Radiation was just energy in their minds, and nothing more. I will try to keep myself citations to a minimum, but I apologize in advance if I get carried away with it. So with this slightly embarrassing context in mind I will forge ahead with points that ought to be dealt with before this paper is published:

Reply:

We deeply appreciate your letting us know you reviewed this paper, which helps us understand your comments better based on our knowledge of your publications. We agree that Essex (1984) is one of the earliest papers which correctly employed Planck expression instead of other formulae (e.g., radiation energy flux divided by absolute temperature) for estimating Earth's radiation entropy flux, and we appreciate the significance of these pioneering papers for advancing our understanding on the Earth's radiation entropy flux and the Earth's entropy production rate. In fact, this has been explicitly discussed in our recent review paper on Radiation Entropy Flux and Entropy Production of the Earth System (Wu and Liu, 2010 in Rev. Geophys., page 2: paragraph [6]). In view that this context has been included in our review paper (Wu and Liu, 2010 in Rev. Geophys.), we felt that it is not necessary to include the historical background again in this paper. However, after reading your comment, we felt a brief history is helpful. So, we added a brief history into Introduction.

1. History and Priority: There are several issues of history and priority that should be addressed.

(a) If the authors think that the entropy of the radiation field is important to climate, which is the premiss of their paper, they really should in fairness cite the paper which opened the field. That JAS paper is still being cited by other authors despite its age. You should certainly feel free to look at and cite papers by Wildt in ApJ in the 60's as tendered by one remark, but correct as they

are, they were neither in the atmospheric science literature, nor 1 are they concerned with the dynamical implications of nonequilibrium processes, being think pieces on classical grey atmospheres. I published objections to Wildt's approach in C. Essex, "Minimum Entropy Production in the Steady State and Radiative Transfer", The Astrophysical Journal 285 (1984) 279-293, but this is beside the point of the manuscript in question. In any case, the Journal of Atmospheric Sciences paper mentioned above is the point, so it should be mentioned in any revised manuscript.

Reply:

As mentioned above, we added some context of the history into Introduction, for the purpose of helping readers recall the advancement on calculating the Earth's radiation entropy flux and the Earth's entropy production rate. Please check the details in the current version.

As we replied to another Referee, Eq. (10) in this manuscript is the result of simply substituting specific solar energy intensity I_{λ}^{r} into Planck expression for calculating specific entropy intensity. This Eq. (10) has little to do with detailed processes of radiation transfer involving radiation absorption or scattering as in those early papers by Wildt (1956, 1966 and 1972).

(b) It seems from the manuscript that equation (1) was deduced in the general case in 2010. While I am not so sure that Planck did not do so, he was hampered by his dependence on ensembles of oscillators. He really got surprisingly far given that limitation. I will have to have a look. But that is really not a crucial point, because it was done for sure in a modern way using photon counting in 1954 by Rosen of EPR fame. I believe it was in Physical Review. So this result is not new and not in any way a modern insight circa 2000's. The combinatorics are easy to do, so authors just deduce it on the spot often. A simple modern treatment in momentum space can be found in C. Essex, and D. Kennedy, Minimum Entropy Production of Neutrino Radiation in the Steady State, J. Stat. Phys. 94 (1999) 253-267.

Reply:

Eq. (1) in this manuscript is a generalized formula of Planck expression for calculating specific entropy intensity (defined as spectral radiation entropy flux per unit frequency per unit solid angle) by using specific energy intensity (defined as spectral radiation energy flux per unit frequency per unit solid angle). The history of generalizing Planck expression for any radiation field was explicitly addressed in our recent review paper (Wu and Liu, 2010 in Rev. Geophys, page 5: paragraph [18]). In addition, we also provided a new derivation to demonstrate the generalization (Section A3.2 in Wu and Liu, 2010). Please check the details in our review paper (Wu and Liu, 2010).

(c) Brightness temperature T is completely unnecessary to the derivation, which was a matter brought up by one referee. But since the quantity was brought up in the manuscript too, it is worth mentioning that this temperature, unlike many other nonequilibrium pseudo temperatures used to discuss nonequilibrium systems actually behaves like a genuine thermodynamic temperature. That is because photons are always in a meta-equilibrium when in the absence of

matter. Here is a paper on that C. Essex, D. Kennedy, and R. S. Berry, "How hot is radiation?" Am. J. Phys. 71(2003) 969-978.

Reply:

Thanks for this information. Brightness temperature in this manuscript is used for determining incident specific solar energy intensity, which is a key quantity for calculating incident specific solar entropy intensity by Planck expression. Also, brightness temperature is the quantity for determining the blackbody Sun's radiation entropy flux by using the conventional approach proposed by Stephens and O'Brien (1993). We adjusted our writing to make this point clearer.

(d) The paper by Stephens and O'Brien cited in the manuscript is closest to the manuscript. The authors must discuss this paper by comparing and contrasting with this previous work. There is every reason to put one's work into its proper context for the good of the reader and the field.

Reply:

It appears there is some misunderstanding here, because the suggested comparison is one of the foci of our work. As described at the beginning of Abstract, the main thrust of this manuscript is examining the magnitude and spectral distribution of the Earth's incident solar radiation entropy flux by comparing the Earth's incident solar radiation entropy flux estimated by Planck expression with that estimated by a conventional approach proposed by *Stephens and O'Brien* (1993). Maybe our writing about this is not clear enough. We adjusted our writing to make this clearer.

2. Physics: There are a few physical issues that must be dealt with.

(a) The idea of "travellling distance" makes no sense to me whatsoever. The discussion around equations (8) and (9) attempts to explain with a simple r2 scaling from a point source. This suggests that there is some confusion about the purpose of going to irradiance in radiative transfer. Going to irradiance is at the heart of radiative transfer, because it eliminates the geometry of configuration space so that one can dwell in what is in effect the momentum space of the photons. There is no "distance" there. In principle working with irradiance handles this problem completely. That is why radiative transfer people do it. One doesnt need r2's in radiative transport. The idea goes over completely for entropy radiation. There is an entropy irradiance, which also has no "travelling distance". . .And there is a radiative entropy flux too. This goes over easily because once in timeless photon momentum space, entropy, is a straightforward combinatoric problem. It is not clear whether the authors think that some kind of irreversible process happens as part of radiation transport or not. They need to be clear that it doesnt and it cant without violating relativity: radiation has no rest frame, so it cannot have internal irreversible processes in the absence of interactions with matter. Photons don't measure time. That is one of the coolest things about this subject. So the authors either mean something completely different, in which case some considerable explanation is required, or what they say is just wrong and needs to be removed.

Reply:

We do not quite understand this comment. The fact that solar irradiance (in unit of W m^{-2}) is only a function of solar distance (i.e., 'traveling distance' in this manuscript) has been demonstrated in many references (e.g., Goody and Yung, 1989, Atmospheric Radiation, second edition, page18). Maybe our writing about this is not clear enough. We adjusted our writing to make sure those statements relevant to 'traveling distance' are clearer.

(b) A related problem is that the authors run flux and intensity (irradiance, and radiance) together frequently as if these distinct things were the same. I am not sure how to fix what they have done in any simple manner because it is so muddled up. They seem to attempt to bridge the difference with their ! factor, but this is not appropriate for this topic. Equation (1) is an example. It is described as sort of about some kind of flux but its form is that of an irradiance. Of course irradiance or intensity is just flux per solid angle, but they are distinguished in that one is a moment integral of the other. The moment integral does not go through the nonlinear logs, so ! just does not work here unless there is another assumption; namely, that the factor ! implies a half isotropic flow. When it comes to an "exact" calculation of entropy flows (which is the sales point of this paper I believe) each beam path is a distinct radiative entropy transfer problem in its own right. Or alternatively each cell in momentum space is distinct and not interacting with others. Clear distinctions between fluxes and intensities must be drawn throughout. Furthermore, assumptions of how one relates to the other must be spelled out clearly for this to be acceptable for publication. Reproducibility becomes impossible without a clear path of what has been done. I am concerned because of the "travelling distance" issue that there is a greater problem than can be fixed with some more careful writing. But let's see.

Reply:

We defined 'specific energy intensity' (or 'specific entropy intensity') as spectral radiation energy (or entropy) flux per unit frequency (or wavelength) per unit solid angle on page 8, below Equation (2). As described in the manuscript (Section 2), the way we use Planck expression to calculate the Earth's incident solar radiation entropy flux is

1) First, substituting incident specific solar energy intensity into Planck expression [i.e., Equation (2) in the manuscript] to get incident specific solar entropy intensity;

2) Then, integrating incident specific solar entropy intensity over all the wavelengths through a surface with a known zenith angle and solid angle [i.e., Equation (4) in the manuscript] to get the Earth's incident solar radiation entropy flux.

Based on your comment, we adjusted our writing to make the definitions clearer.

(c) The final major physical issue I have with this paper is another case of mixing up language in a way that causes significant problems, either for the reader to understand or for the underlying merit of the paper. The authors mixup four completely different physical things: entropy, rate of

entropy change, entropy flow and entropy production. One cannot determine which they are talking about where, or whether it is understood that these things are actually quite different. But the difference is crucial, as only one of these has any thermodynamics significance in it itself in an open system. Which one do you think it is? And if this paper is not aimed at thermodynamic significance, what is it about?

Reply:

Based on your comment, we carefully checked and adjusted each individual use of entropy-related phrases in the whole manuscript, including *entropy*, *entropy flux* and *entropy production rate*.

There is actually quite a lot to do to this paper before I would recommend it for publication. To the authors, I am sorry to be so full of criticisms. Please don't be too discouraged, but there is a kernel of material that can stand on its own here, which is the detailed entropy flux/intensity (please decide which it is) as a function of frequency. I don't believe that has been done before if I understand correctly. It is interesting to see the shape of the black body functions and how the spectral lines alter it. It is not necessary to make claims of new thermodynamics, or climate "impacts". The role of thermodynamics in a dynamical system like the Earth's atmosphere and ocean remains an open topic. We are not ready for such claims. I wish the authors the best of luck with their revisions and look forward to seeing what they can do to make this a much better paper.

Reply:

Thanks.