

**Review of**  
**“Spectral solar irradiance and its entropic effect on Earths climate”**  
**by W. Wu, Y. Liu, and G. Wen**

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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. Before hand 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. Before hand, 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 my self 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:

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

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.

- (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.
- (c) Brightness temperature  $T_\nu$  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.
- (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.

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

- (a) The idea of “travelling distance” makes no sense to me whatsoever. The discussion around equations (8) and (9) attempts to explain with a simple  $r^2$  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 doesn't need  $r^2$ '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 doesn't and it can't 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.
- (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  $\pi$  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  $\pi$  just does not work here unless there is another assumption; namely, that the factor  $\pi$  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.

- (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?

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.