Short Review of

Spectral solar irradiance and its entropic effect on Earth climate

by W. Wu, Y. Liu, and G. Wen

Having kindly been asked by the editor to review the present manuscript with particular attention to the validity and originality of the analysis, as well as its style, I must first confess that at this juncture I am unable to spend the time to share in a detailed review my views and modest knowledge of the same problem as the one treated by the authors.

Nonetheless, I will attempt a sketch of my opinion about the article, particularly with the editor's points in mind.

I begin with the last one, concerning the style. I agree to the editor's interactive comments on it as of June 15, in which he deplores the use of lengthy and repetitive sentences full of concatenated nouns and of the typical acronyms that seem to infest modern articles in atmospheric science. An abominable custom but in no way a reason for me as someone familiar with the topic to set aside the work on this count. In spite of its somewhat aimless drifting, I had no problem in following the line of argument towards the conclusion that somewhat may be learned from calculating the entropy irradiance of our atmosphere. For the uninitiated reader that lesson may remain perplexing. However, sentences with words like "*substantial impact is possible*" and "*expected critical to determining the Earth system's thermodynamic quantities*", and of something being "*somewhat evident by the fact that*" make the reading an excruciating experience.

As to originality, I cannot help being clear: there is none as far as the theoretical foundation is concerned. What I do find to be original is the fact that a well developed if badly publicized theory was applied to recent observed spectra, and the figures did arouse my theoretician's curiosity, being based on real data. I may also add that since the theory is not widely known and errors have crept into the recent literature, there is a merit to an account like theirs, as an attempt to set the record straight. And as some of the interactive comments show, ignorance of former work has lead to a need for clarification of the entropic role to be attached to radiative processes. Thus I applaud the effort of the authors.

Following the example set by one reviewer, I feel compelled to indulge in bad taste by pointing out that I myself have dealt at length with all the formulae presented in the manuscript (see my book of 1995: *Entropieerzeugung eines strahlenden Planeten: Studien zu ihrer Rolle in der Klimatheorie*. [Entropy production of a radiating planet: studies of its role in climate theory]. Verlag Harry Deutsch, 206 pp. My apologies for not having either translated that book or published the solar radiation parts, although there is an inkling including absorption of solar radiation in the Appendix to my paper of 1994: Towards an accurate estimate of the entropy production due to radiative processes, *Meteorol. Atmos. Phys.*, **53**, 1-17.) It has been a discouraging experience that so many later authors have not seen fit to take any notice of much of the previous work on the entropy of radiation (not even Goody and Abdou did in 1996, although Goody used to be careful with his foreign citations). They leave out contributions that have partly prepared the ground for much of the recent (flawed) work. Of the seminal papers that were published from 1984 on, I feel strongly urged to mention that of Ulrich Callies on the entropy production by scattering of polarized radiation, which goes far beyond, in scope and technical detail, of what has been published since (Callies, U., 1989: Entropy production by atmospheric scattering of light. *Beitr. Phys. Atmosph.*, **62**, 212-226).

I have read and enjoyed part of Planck's work and can say that Eq. (1) in Wu et al.'s manuscript is anything but a generalization of what can be found in Planck's publications from 1900 on, as well as in the first edition of his famous textbook (1906). As their approach is, like mine, phenomenological, Planck will always remain first to have discussed the more general formula for polarized radiation, of which (1) is but a specialized form. The claimed or implied originality does not correspond to the historical facts.

As to the validity of the analysis, I have rather bad news for much of the paper. The authors are grappling with some definitions but they do not seem to be entirely conversant with radiative transfer. One of the basic tenets of that theory is that the radiance (or Planck's specific intensity or the astrophysicists' intensity) is independent of the distance from the source, unless the radiation (the photons) interact with matter, while it is the flux density (irradiance) that diminishes according to the inverse square law. Therefore, in my view, Eqs. (8) and (9) are simply wrong, and the entire "scenario I", with a poorly defined assumption of "isotropic hemispheric incident radiation" is invalid. Formula (3) or (4) suffices for their purposes. Formula (5) is an excellent approximation if the deviation of the real incoming radiance from a blackbody spectrum at 5770 K is as small as it is shown by the authors to be. Everything dealing with assumption I can be dispensed with, as it obscures the whole of the presentation of the incoming entropy radiation flux density. The authors sense the awkwardness of that scenario when they write in the Summary: "It is worth mentioning that in reality the Earth's incident solar radiation probably does not behave as the assumption I of isotropic hemistropic (sic!) incident solar radiation that requires the space is full of scattering particles." The sentence makes little sense, grammatically as well as semantically, since "probably" the space between Sun and Earth is not "full" of scattering particles. And if it were, the incoming radiation from the sun would not have a radiance reduced by the "travelling distance" but by extinction. Of course the values calculated under this assumption become useless. The high values found (more than unity) cannot be true and the discrepancies seen by commentators disappear. It is Stephens and O'Brien's value of 0.08 $W/m^2/K$ that gives the right order of magnitude. Also, in their Summary they mention a difference between what follows from assumption I and from II to be 0.23 $W/m^2/K$, but that seems to have no relation to the values previously calculated.

Section 5 was the most interesting to me even though I cannot see the point of calling the comparison of qualitatively different spectra a sensitivity study. If the article would just confine its results to comparing the observed entropy spectrum with that of an energy-equivalent blackbody or a deviant spectrum, it would be of interest to casual readers. I have my doubts that the observed spectrum has a higher flux density than the equivalent blackbody spectrum, whereas I expected the "sinusoidal" spectrum to carry more entropy, as it indeed does, according to their value of 1.4 $W/m^2/K$. But this value is based, as far as I can see, on the flawed assumption I.

As it stands in my understanding, I cannot recommend the paper for publication. I would, however, welcome a highly condensed version which does justice to previous work and subsequently shows the differences existing in entropy import from the Sun between the blackbody spectrum at the Sun's temperature and the observed spectrum. For the entropy production of the whole Earth that quantity matters. In any case, the authors can draw from previous work that is ready to hand and should not make such a fuzz of a single formula that was introduced by Planck more than a century ago. I had a similar experience with the recent paper by Wu and Liu where one and the same formula for diluted blackbody radiation is rewritten over and over again. Less is more in this case, and we mortals should strive more than ever for clarity and concision.

I am not proud of having to reach this conclusion, as I know the authors to have been working with engagement and great personal effort on a subject that I earnestly welcome: Helping others to understand the role of radiation entropy in atmospheric science. And it bears repetition when I say that there is a merit to their attempt to draw attention to the correct formulae, as there have been already too many articles with wrong formulae, ignoring past work to the detriment of a healthy development of science.

Joachim Pelkowski
KlimaCampus,
JRG Dynamical Systems,
Universität Hamburg