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Interactive comment on “Spectral solar irradiance and its entropic effect on Earth’s climate” by W. Wu et al.

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As editor I wish to make some remarks on this manuscript, before I have to make a decision. Since I am not a radiation physicist, I am reliant on the reviewers' comments on the originality of the analysis; nonetheless, it appears that the authors have not taken the second reviewers' comments to heart. In particular:

1. The authors have not effectively responded to comment 1(b), namely that equation (1) is not original to the authors' 2010 paper, but was preceded by Rosen in 1954 and in a later work by Essex & Kennedy, amongst others. The manuscript still reads as if equation (1) is original to the authors' 2010 work. If the reviewer is correct, this must be corrected throughout the manuscript, and the primary reference must be cited correctly. If the reviewer is incorrect, the authors must make a clear rebuttal of this claim.

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2. While the authors may have responded to comments 2(b)-(c), on the precision of terminology used, the manuscript as a whole is unnecessarily opaque and difficult for a non-specialist to follow. Expressions such as :

"the specific solar energy received at the TOA is the same as that radiated at the Sun's surface and incident radiation is isotropic within the cone of the solid angle to the Sun subtended by any point at the TOA",

which is repeated again and again by the authors, become utterly meaningless due to their length, lack of punctuation ("lack of breath") and the unfamiliar acronym, and should never be expressed in this manner.

3. Given the density of language, it is difficult for a non-specialist to appreciate the purpose of the manuscript. What is the main result, in one sentence ? What is the significance of this result? Since it concerns radiative entropy transport (or flux), how does it relate to studies of the entropy production, the main theme of the special issue ? These questions should be immediately obvious from the abstract, which is twice or three times as long as an abstract should be.

In this regard, it greatly improve the readability of the manuscript if the authors could draw a diagram to indicate the physical meaning of the quantities used; and then, on the same or in separate diagrams, illustrate the physical meaning of assumptions I and II. These assumptions should be explained carefully, and thereafter, should only be referred to as assumptions I or II, without the long statements. Why do these assumptions give such wildly disparate estimates of the solar entropy flux (pp 14-15)? - this should be clearly explained. Further, the authors need to be more consistent in terminology: "specific energy intensity" should not suddenly change to "specific solar energy intensity", etc, unless the meaning has changed.

Unless these changes are made, I fear that the manuscript will be inaccessible to the broader entropy production community (or wider scientific community) and its contribution will be forgotten.



To help me make a decision, I have requested a third review from a specialist in radiation physics, with particular attention to the validity and originality of the analysis, as well as to the style of the manuscript. I will defer the decision until this review is received.

Interactive comment on *Earth Syst. Dynam. Discuss.*, 2, 45, 2011.

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2, C213–C215, 2011

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