

Earth Syst. Dynam. Discuss., referee comment RC1
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Comment on esd-2022-38

Anonymous Referee #1

Referee comment on "Working at the limit: A review of thermodynamics and optimality of the Earth system" by Axel Kleidon, Earth Syst. Dynam. Discuss.,
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The manuscript "Working at the limit: A review of thermodynamics and optimality of the Earth system" by Axel Kleidon features a review of several mechanisms involving optimality principles and thermodynamics constraints in energy conversions throughout the climate system. In particular, three examples are provided, about the atmospheric mean meridional circulation, the hydrological cycle and the productivity of the terrestrial biosphere. The specimens, though self-consistent, are related in order to discuss the different degrees of efficiency in energy conversion and work production, and comparisons between proposed conceptual models and observations are provided.

Overall, I think that the review is well written, focused on the aim of stimulating further research on the investigation of the climate system (in the wide sense, including the biogeochemical "layer") from the point of view of thermodynamic constraints. I do acknowledge, though, that there has been a lot of criticism on the motivation behind and the application of these approaches, and I believe that they should be more extensively addressed in this manuscript. I also think that this review might be a valid reference on the topic, that is why I recommend that the manuscript is accepted for publication, provided that a more robust set of references is included, and that the mentioned criticisms are taken into account, along with the specific comments and technical corrections proposed below.

Specific comments

- II. 35-37: here and elsewhere in the text, I believe that a more complete framework of the research literature on methods of computation of the entropy budget in the climate system. I can think, among others, of Goody 2000, Raymond 2013, Bannon 2015, Bannon and Lee 2017, Lucarini and Pascale 2014, Lembo et al. 2019...

- I. 50: as the manuscript here proposed is a review, when the MEP principle is introduced, I think it is also worth informing the reader that the concept has not been unanimously accepted by the community in their theoretical derivation. I can think of Dewar 2007 or Grinstein and Linsker 2007, as examples of this ongoing debate...

- I. 130 and II. 142-143: not sure I get the point here. Of course, it is impossible to evaluate the entropy production of the system in a microscopic sense, as jumps of quanta of energy. Is it relevant at all in this context?

- I. 148: when I think of heat transport in the atmospheric medium, I do not see molecular diffusion as a mean of transport that is relevant in the macroscopic scale.

- I. 152: it would be interesting to know a bit more about what the author means when talking about "forms" of entropy, as it is not entirely clear at this point of the manuscript;

- I. 172: maybe "entropy change"?

- I. 173: related to my previous comment, if we are talking of "free energy" as a form of energy that is converted with no change of entropy, that would surely not be dissipation. That is why I am a bit confused by the whole definition of "free energy" that is proposed in this context. Could the author clarify on this point?

- Figure 3: not sure I understood what is included in the term "Generation", although it is somewhat described in the text;

- I. 206: as far as I understand it, there are several more general ways to describe a thermodynamic cycle.

- II. 241-242: this is in general not true, I believe. Despite the fact that you can of course have frictional dissipation anywhere, the contribution to the energy reservoirs is almost negligible, and I cannot think how it can affect the transport, at least in the atmosphere.

- II. 306-307: I think that a similar formulation for the Carnot cycle within the climate system was provided in Pauluis and Held, 2002.

- I. 369: I think it is a bit misleading to suggest that the use of conceptual models is

meant to facilitate proving a point in front of a reader. There is an illuminating communication by Isaac Held on the importance of establishing a hierarchy of models in order to understand how the climate system works, that might be of interest in this context (Held, 2005).

- II. 436-438: as stated at the beginning, I am not against speculative arguments, but I find it hard to agree with this sentence, just because observations are in rough agreement with the proposed conceptual model.

- I. 449: if this was the aim, it would have been useful to give numbers in order to compare the different contributions. Maybe some table would have been helpful.

- I. 464: it seems to me that you are rather arguing here that the conceptual model that has been designed in order to be consistent with observations is maximizing the energy conversions based on some thermodynamic constraints. Still, there is a missing step before one can claim that the atmosphere is actually operating at its maximum.

- II. 469-470: but this is something you can also achieve by computing the material entropy production in the Lorenz Energy Cycle, as shown elsewhere (cfr. Lucarini et al. 2014, Lembo et al. 2019).

- II. 551-553: shouldn't the phase changes also be taken into account here? (cfr. Pauluis and Held 2002).

- II. 607-609: I have nothing against these approximate calculations, but there are so many assumptions here that I really have the feeling no conclusions can be easily drawn here.

Typos and technical corrections

- I. 17: incredible -> incredibly;

- I. 95: add "were" between "thermodynamics" and "developed";

- I. 527: missing reference;

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