Response to Reviewer #1

I thank the reviewer for the constructive and helpful review. In this response, I respond to all points raised and describe how I will accommodate these in the revision.

In the following point-by-point response, I will state the reviewer's comment in *italics*, followed by my response and description of the action that I will take in the revision.

Major comment: *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.*

Response: Thank you for this assessment. I agree that providing more background of the criticisms will be helpful.

Action: In the revision, I will extend the part in the introduction on thermodynamic optimality principles (third paragraph), particularly regarding the criticisms that these have received. I will also add further references as suggested by the reviewer.

Specific comments

Comment: *ll. 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...*

Response: Thanks for the suggestion, and I agree that this place would be a good place to give references to the entropy budget. Since the detailed evaluation of entropy production is not the main focus of the manuscript and because it is already quite long, I would prefer to keep it relatively short and focused on practical applications and estimates.

Action: I will add a paragraph in the introduction and briefly describe entropy budgets with some representative references. I will include mentioning the planetary entropy budget also in Section 2.5, and make connections to this budget at the end of the three examples in Section 3.

Comment: *l. 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...*

Response: Thank you for this suggestion. I agree that this is useful to add.

Action: I will extend part of the introduction and mention briefly the proposed MEP principle and its discussion, with some representative references.

Comment: *l. 130 and ll. 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?*

Response: The relevance of going down to the microscopic scale is that entropy is not just about heat, or thermal energy, but that there are other forms of entropy that describe radiation and chemical compounds. These aspects can be understood when describing how quanta of energy are being distributed at the microscopic scale, and how this results in the different forms of entropy. This picture is relevant when dealing with Earth system processes and their entropy production, particularly when it comes to deriving work from the solar radiative forcing. While the atmosphere derives work from entropy differences associated with differential heating, photsynthesis and photovoltaics are able to utilize the entropy of solar radiation, that is, before it turns into heat during absorption. Because of this, photosynthesis and photovoltaics could, in principle, derive much more work than atmospheric heat engines from the solar forcing (and, in fact, solar panels already derive much more free energy from solar radiation). This part is aimed to provide the basis to understand this critical difference when entropy is being discussed.

Action: I will rewrite parts of this section to clarify this aspect.

Comment: *l. 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.*

Response: This part of the text describes the distribution of energy at the microscopic scale and thermodynamic equilibrium associated with the different forms of entropy. It does not deal with macroscopic phenomena like diffusion.

Action: I will rewrite this paragraph to clarify.

Comment: *l. 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;*

Response: What is often not recognized is that Earth system processes deal with different types of entropy, not just with the concept of entropy in classical thermodynamics. In quantum physics/ statistical physics, this is very well established. This is what I aim to describe in this paragraph (and which is illustrated in Figure 2), although apparently, it was not clear to the reviewer.

Action: I will rewrite this paragraph and clarify, making it more specific what the change in the form of entropy is reflected in. For the sentence on line 152, this can be extended to be made more specific like this:

"When solar radiation is absorbed and heats the surface, it is not just the energy that changes its form from radiative to thermal energy. Also, the entropy changes its form, from entropy representing how photons are being distributed over wavelengths (radiative entropy) to how energy is distributed over the random vibrations and motions of molecules (thermal entropy)."

Comment: *l. 172: maybe "entropy change"?*

Response: No, it is not about that entropy does not change, but that free energy has no entropy. When, for instance, two reservoirs of heat have different temperatures, the total entropy of the two reservoirs is below its maximum. This maximum is reached when heat is transferred and the temperature difference is levelled out. The system could derive work from the initial temperature difference, which, if it is dissipated within the system, raises the entropy. In other words, the free energy that was generated has no entropy and only enhances the entropy of the system when it is being dissipated. This description is synonymous with exergy, although with the added notion that free energy is energy without entropy.

Action: As reviewer 2 also stated that this section is not specific enough, I will rewrite this paragraph.

Comment: *l. 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?*

Response: Ok, see explanation above.

Action: See action above.

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

Response: Given the previous comments and responses, and also those of Reviewer 2, I hope that this will become clearer in the revised version of the manuscript.

Action: See action above.

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

Response: Well, when I look at textbooks (Feynman lectures on physics, Kondipudi and Prigogine) that I am familiar with, the Carnot limit is derived from a specific thermodynamic cycle, usually inferring the limit from specific steps of adiabatic expansion and isothermal heating and cooling. The derivation used in the manuscript starts directly from the laws of thermodynamics that do not invoke a specific thermodynamic cycle.

Action: I will make this sentence more specific by adding the references to the textbooks.

Comment: *ll. 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.*

Response: What I describe here are two extreme cases that are being considered where the dissipated heat is added. It is true that the heat being added is small, yet one can easily show that this assumption affects the temperature in the denominator in the Carnot efficiency. This has, for instance, be done previously by Renno and Ingersoll (1996) and Bister and Emanuel (1998). This distinction affects the work output of hurricanes, as shown by Emanuel (1999).

Action: I will clarify the text and state that these are two extreme cases being considered and that it affects the Carnot efficiency term, and add the reference to Emanuel (1999) to illustrate its relevance. I will add an Appendix with that derivation.

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

Response: Pauluis and Held (2002) used the Carnot limit, as did previous work, e.g., by Renno and Ingersoll (1996). The important difference here is that the heat engine is coupled to the energy input from the surface, which results in the limit of maximum power.

Action: I will clarify the relation to previous work and the difference in the revision.

Comment: *l. 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).*

Response: Thanks for making this point.

Action: I will add the reference and refer to the importance of having a hierarchy of models.

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

Response: I am not sure what the reviewer means by "speculative arguments" in the context of this paragraph. The calculation shows how much work can maximally be derived from differential solar radiative heating, and the resulting estimate compares well with observations, with respect to the power needed to sustain the large-scale circulation as well as the associated poleward heat transport. This was done in a two-box model, which, quite clearly, is very simple, although it similar to previous models that have been used before to demonstrate MEP with a very similar outcome. In my understanding, this is consistent with "Occam's razor" - a scientific explanation of a phenomenon based on physical principles with the fewest number of parameters needed.

There are certainly some limitations that were not considered in this extremely simple model, which I can add to highlight some of the uncertainties (e.g., the contribution by the Hadley cell and seasonal variations that I would take from the discussion in Kleidon 2021a). Yet, these would not affect the interpretation that this consistency implies that the atmosphere works close to its limit, that is, as hard as it can.

Action: I suggest I add a few sentences of the uncertainties.

Comment: *l. 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.*

Response: Very good point, thank you.

Action: I will add a table with the estimates of entropy production to support this difference between power and entropy production and extend the description to describe it more specifically.

Comment: *l. 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.*

Response: I do not understand which missing step the reviewer is referring to. It is not "some thermodynamic constraint", but rather power - that is, the work needed to sustain motion against friction. I can imagine that the reviewer may be referring to the potential mechanism of how this maximization may be achieved, an aspect that Reviewer 3 alludes to as well.

Action: I will add a subsection in section 2 to outline how the maximization of power may result from two contrasting feedbacks, as described in Kleidon (2016).

Comment: *ll. 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).*

Response: Yes, sure. As is described in the previous paragraph, the maximization of power, frictional dissipation or entropy production yield about the same result. The difference is that with power, we get a physical interpretation, which allows us to distinguish processes that perform work from those who do not (such as diffusion or radiative transfer), while entropy production is associated with every process.

Action: I will add references of MEP applications to the previous paragraph.

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

Response: Actually, phase changes do not produce entropy in this equilibrium partitioning approach, because it occurs at saturation and is hence reversible. The irreversibility is associated with the mixing of the saturated surface air with the unsaturated near-surface air, but this entropy production by mixing does not affect the partitioning between sensible and latent heat.

Action: I will add this clarification in this discussion part and also include a more general description of the contribution of evaporation to the entropy budget (see also response to first specific comment above).

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

Response: Actually, these are not many assumptions - the approaches that this calculation is based on are standard in vegetation modelling and land surface hydrology, and the numbers represent well-established values. Even if the values deviate, e.g., the water use efficiency is twice as high, it still results in an overall efficiency that is still much, much lower than the theoretical limit derived from radiation conversion.

Action: I will extend this estimate with additional references to support the numbers, and add the uncertainty estimate.

Typos and technical corrections

Comment: *l. 17: incredible -> incredibly;*

Thanks, corrected.

Comment: *l. 95: add "were" between "thermodynamics" and "developed";*

Thanks, added.

Comment: *l. 527: missing reference;*

Thanks, corrected.