11 July 2023

Response to Re-Review of my manuscript "Working at the limit: …"

Dear Gabriele,

Thank you (and the reviewers) for another round of reviews.

I have now revised the manuscript according to these comments - they mostly dealt with additional clarifications. The response to the comments raised in your letter as well as the comments by reviewers #1 and #3 are addressed separately point-by-point on the following pages.

In the revised manuscript, the modified text is highlighted in cyan. In addition to text modifications to address reviewer comments, I have also added a further, new reference on maximum power that was just published (Ghausi et al. 2023, PNAS, https://doi.org/10.1073/pnas.2220400120). I added this new reference for two reasons: first, it shows a much better agreement with observations at seasonal scales (that is, if more detail is added). Since the "more detail" part is not the main focus of the manuscript, I simply refer to it in Section 2.4 (lines 426ff). Second, this study also shows potential aspects for refinement of the maximum power limit, as it takes horizontal heat advection into account and demonstrates its importance in modifying the limit. This part is mentioned in the discussion section in Section 4.1 (lines 974ff).

With these revisions, I hope that this will help to satisfy the reviewer comments (although, honestly, I am not so sure if that is at all possible with reviewer #3).

Best,
Axel

Response to Editor

Editor: *The reviewers have diverging opinions on the revised manuscript, and I believe that some further refinements may be needed before taking a final decision on the manuscript.*

Response: I thank the reviewers for their efforts on reviewing the revised manuscript.

Action: I have addressed all comments in the responses below, mostly by providing more explanations. The new modifications in the re-revised manuscript are marked in **cyan**.

Editor: *In particular, one of the reviewers, although recommending acceptance, argues that this is more of a perspective paper than a review, since it favours specific viewpoints as opposed to providing an unbiased overview of the field. This is not necessarily a problem per se, and on the contrary may contribute to the value of the paper, as long as this is made clear to the reader - which is not always the case in the current text.*

Response: Agreed.

Action: I added a few sentences on this after line 105 where the aims of this review are being described and why I think that this biased view may be beneficial.

Editor: *The same reviewer also argues that some of the edits you have implemented add text without necessarily providing clear definitions of the concepts you are describing, notably in the case of free energy. While I do not advocate a heavy mathematical treatment of the topic, since this is not the style of this study, some precise definition may aid understanding (e.g. along the lines of how Tailleux (2013) defined exergy in terms of the relatively simple eq. 5).*

Response: It is not as simple as expressing free energy in one equation. If one only deals with heat and pressure work, then free energy (or exergy) can be expressed by one equation. But the Earth system deals with different forms of free energy, and some conversions do not involve entropy, or involve different forms of entropy. The free energy in radiation, for instance, is similar to the one for heat, but it includes modifications due to photon pressure, which is different from the pressure of an ideal gas. Hence, the expression for free energy is different as well. The kinetic energy of motion that can be converted further into electric energy using wind turbines (another conversion process) is, again, different.

So instead of providing an equation that may be misused because it is applied to contexts for which it has not been derived for, I think a clarification on the need for a more general, yet qualitative description of free energy is more helpful in the text.

Action: As the reviewer did not find the previously revised text helpful, I revised it again (after line 244) and shortened it along the lines described in the response.

Editor: *The third reviewer recommends again rejection. While this study is a review of past results from the literature, this does not mean that such results may not be partly at odds with the literature in other subfields, as some of the reviewer comments on e.g. the vertical energy transport argue.*

Response: Actually, the results are mostly *not* at odds with literature, particularly regarding the example of vertical energy exchange. In that particular example, the difference concerns the perspective on whether one focuses on the inside of the system, or on the whole system that includes its boundary conditions. The importance of the latter can easily be seen by looking at a heat engine: the laws of thermodynamics tell us the limit of energy generation irrespective of the details inside the engine, but these limits follow from the boundary conditions, not from the details inside. Likewise, the limits of power are not about adiabatic temperature profiles within the atmosphere, but rather about the entropy exchange at the system boundaries. The reviewer, however, seems to only focus on the interior, and not at the boundaries of the system.

Action: In the revision, I aimed to clarify these aspects further on lines 430ff.

Editor: *Relating back to the comment above on a perspective versus review paper, it is important to highlight clearly cases where the proposed principles, presented in peer-reviewed studies, may not fully agree with other equally peer-reviewed results.*

Response: Again, it is less a lack of agreement but rather the lack of a system's perspective that may lead to contrasting interpretations. When it comes to stomatal functioning, for instance, interpretations from plant physiologists typically focus on the scale of a leaf, and not on the larger scale. At the larger scale, the environmental controls on evaporation are very well described and established in the context of the Budyko framework. As this larger scale is the focus of the manuscript, the latter is more applicable here.

Action: I added clarifications around line 831ff and described the relationship to the Budyko framework more explicitly.

Editor: *Finally, a large part of the ESD readership is likely not to be familiar with plant physiology. It is therefore important to clarify some basic concepts there, and avoid statements that may be understood as being at odds with basic physiological processes.*

Response: I refer to the previous two responses, as this comment aims at these.

Action: See previous comments.

Editor: *When revising the manuscript, I would encourage you to try to formulate your replies to the comments by the third reviewer in as clear and detailed a fashion as possible, copying in your replies the whole text provided by the reviewer. One of the salient features of ESD is the possibility for the community to follow the review process and discussion between the authors and reviewers, and the format of the replies should facilitate this process.*

Response: The responses to the reviewer comments is included below.

Action: See below.

Response to Reviewer #1 (Anonymous)

Reviewer: *I carefully read through the revised version of the article "Working at the limit: A review of thermodynamics and optimality of the Earth system" by Axel Kleidon, submitted for consideration to Earth System Dynamics. I found that the suggestions and comments by the other reviewers' and myself have significantly improved the quality of the manuscript.*

Response: Thank you. I am glad to see that the modifications are seen as an improvement.

Action: No action taken.

Reviewer: *I still think that* ***some of the figures are a bit too qualitative****, and something could be done to* ***make them more self-explanatory*** *and* ***suitable*** *for a scientific publication. Given that I do not have specific advice on how this shall be accomplished, I leave it to the author to decide whether they are fine like this or they want to work a bit more on that.*

Response: I am somewhat confused by this comment. While some figures are taken from previous publications, the ones that are not from publications (Figs. 1, 4, 5, 9, 11, 13, 15) are used to either convey concepts or supplement model formulations described in the main text.

Action: I made minor modifications to the captions to describe the purpose of the schematic figures and aimed to better link them to the text. I also slightly adjusted Figure 16 to better match the description in the text. I hope with these modifications, the figures are more self-explanatory.

Response to Reviewer #3 (Jonas Nycander)

Reviewer: *Some changes have been made in response to my comments, but none of them address my main objections. I therefore still recommend rejection. Much of the text consists in lengthy qualitative descriptions of how entropy is produced as a result of various energy transformations. These descriptions are correct, but unsurprising, since everything said follows immediately from basic thermodynamic principles. They also do not lead to any quantitative predictions.*

Response: It is nice to see that the author thinks that the "lengthly qualitative descriptions" are correct but unsurprising. My experience over the last decades has been the opposite - that these aspects of entropy and thermodynamics are neither widely known nor recognised.

Action: No action taken.

Reviewer: *The scientific core of the paper is a consideration of a simple model with two boxes. All radiative energy fluxes are assumed to be either given externally or determined by the temperature of the boxes. There is also a turbulent energy flux J between the boxes. … The function f describes the radiative fluxes and the energy budget, while g describes the turbulent flux. Thus, the two functions describe independent physical processes. But, of course, the turbulent flux affects the temperatures, and thereby indirectly the radiative fluxes, as a part of determining the state of the whole system.*

Response: It may come down to splitting hairs when it comes to define what an independent process is. The presence of turbulent fluxes on land are clearly intimately linked to surface temperature, because it is mostly buoyancy developing at the surface that drives them, and buoyancy on the other hand develops because the surface is warmer than the overlying air. One can formulate these separately, but they are clearly not independent from each other.

Action: None taken.

Reviewer: *Turbulent transport is in general a complex process that is difficult to describe. The author claims that this is not needed, and that J can instead be determined by maximizing the free energy production, subject to the Carnot constraint (the 'optimum principle'). If the ΔT is much smaller than the absolute temperature, this means that the product $J\Delta T = Jf(J)$ should be maximized.*

Response: The author does not claim anywhere in the manuscript that turbulence is a simple process. As a matter of fact, it may be so complex that in the end, the only relevant constraint is how much free energy is generated to drive turbulent dissipation, because this limit is not set by turbulence, but rather by the boundary conditions of the system. The Carnot limit explains why the outcome is then very simple: This is because the limit of free energy generation is only described by the conditions at the system's boundary. The derivation of the Carnot limit does not require any information of the complexity of the interior of the heat engine that it deals with. Similarly, one can understand the success of maximum power - that the dynamics are governed by the constraints imposed by the boundary conditions, for which the surface plays a pivotal role.

Action: I have added this interpretation to the end of Section 2.4 (lines 440ff). I have also added and emphasized that the likely interpretation of how this maximization is achieved is through the complexity in turbulence (Section 2.5, line 475).

Reviewer: *The only support for the optimum principle is that it gives good agreement with observations. In the case of meridional heat transport in the atmosphere we have, according to the manuscript, $\Delta T_{max} \approx 60$ K, which gives the prediction $\Delta T \approx 30$ K. This should be compared to the observed value 20 K. I don't find this agreement very impressive, given that we know a priori that ΔT lies between 0 K and 60 K. Moreover, it is clear that many factors, for example the planetary rotation rate, affect the turbulent transport, and therefore the function g, without affecting the function f. Thus, changing the rotation rate will change the state of the system, while the proposed optimum solution remains the same.*

Response: The example given in Section 3.1 is not the only support for the optimum principle - section 2.4, 3.2, and 3.3 provide further support, as well as the new added reference to Ghausi et al (2023). Further support comes from climate model simulations that have been done previously (e.g., on the related MEP hypothesis, Kleidon et al., 2003, 2006 - cited in the text in Sections 2.5 and 3.1). In Section 3, however, I have written explicitly that (lines 514ff) "*I will specifically use models that are as simple as possible to describe these examples. The motivation for this simplicity in formulation is to provide transparency and accessibility to the reader, and to not obscure outcomes with complex mathematical formulations.*" In other words, the manuscript favours insights over precision, and this is clearly stated in the beginning of the Section.

Regarding the role of rotation rate, the manuscript already mentions rotation rate in both, section 3.1 and in the discussion section. The thermodynamic limit remains valid irrespective of rotation rate, but for certain conditions, the maximum power limit may not be reached. For Earth, however, this does not seem to be the case.

Action: I have slightly reworded the text towards the end of Section 3.1 (Lines 607/608), and added a further reference to support maximum power (Ghausi et al, 2023). The discussion on the role of rotation rate has been clarified (Lines 967ff).

Reviewer: *The idea that you can determine the state of the system without knowing anything about the mechanisms of turbulent transport is scientifically unsound. This can clearly be seen in the case of vertical energy transport. Here, ΔT_{max} is the temperature difference given by a pure radiation balance. … This result is different from the established theory, which says that the vertical temperature gradient is close to the adiabatic lapse rate. The mechanism is that the turbulent transport is negligible if the atmosphere is stably stratified, and increases very rapidly if the temperature decreases upward faster than the threshold for convective instability. Translating this to the box model used in the manuscript, this means that the function $g(\Delta T)$ in eq. (2) is essentially zero for $\Delta T < \Delta T_{conv}$ (where ΔT_{conv} is the threshold for convective instability), and increases very rapidly for $\Delta T > \Delta T_{conv}$. The solution of the problem is then essentially that ΔT equals the smallest of ΔT_{max} and ΔT_{conv}. Thus, if $\Delta T_{max} > \Delta T_{conv}$ (as in the real troposphere) the state is entirely determined by the mechanism of turbulent transport, which is ignored by the optimum principle. The simple theory outlined above is a corner stone of climate science since many decades, and it is based on an clear physical mechanism. If you want to replace it by another theory, which lacks physical motivation, it is not enough to show that its prediction of ΔT agrees roughly with observations. You must explicitly compare it with the established theory, and show that it gives a better prediction.*

Response: The claim that the approach is unsound is not justified. The Carnot limit of a heat engine can be derived without the detailed knowledge of the inside of the heat engine and how it actually works. This is because the Carnot limit follows directly from thermodynamic constraints at the system's boundary, irrespective of how the engine functions. The details that the reviewer describes, e.g., regarding the adiabatic lapse rate, deal with the interior organization of the atmospheric heat engine that are not being considered in the maximum power approach. The maximum power approach uses the radiative boundary conditions of the surface-atmosphere system (i.e., the radiative heating at the surface and the radiative cooling of the atmosphere), in addition to the strong interaction with surface temperature, but not with the interior organization of the heat engine within the convective boundary layer. Furthermore, it seems that the reviewer neglects the fact that the presence of unstable vs. stable conditions - at least on land - are predominantly caused by the presence or absence of solar radiative heating of the surface. In other words, if these established concepts are viewed in a more holistic way that encompasses the whole system, then one notices that there is no contradiction between what the reviewer writes and the applicability of the maximum power limit.

Action: I have added an extra paragraph at the end of Section 2.4 along this explanation (lines 430ff). I have also added another reference (Ghausi et al., 2023, https://www.doi.org/10.1073/pnas.2220400120) in which the maximum power limit is evaluated at greater detail and which shows how remarkably well this approach can describe observed temperature variations.

Reviewer: *The discussion of photosynthesis seems to imply that the evaporation somehow drives the photosynthesis. That is a strange idea, since increased evaporation is in general detrimental for plant growth. The text is not very clear on this, but in the reply to my previous comments the author rejects my comment that the main resistance to CO_2 transport is in the stomata, and is caused by the need to save water. He also states that "stomata play very little role in controlling evaporation rates".*

Response: The carbon uptake by plants is intimately connected to water loss and evaporation - they are not independent processes. I am not sure why this should be a strange idea. It relates to the need of plants for carbon dioxide, which plants need to take up from the air. This is very well established. It is also very well established in hydrology that land surfaces typically evaporate near their potential rate set by energy availability when unconstrained by water availability, as reflected in the common Budyko framework. Stomata are surely the way by which the gas exchange takes place, and stomata react when soil water becomes limiting to protect the plants, but their primary purpose is not to save water.

Action: I have rewritten and added text (lines 827ff) to clarify that climatological evaporation rates and therefore gas exchange are set mainly by physical factors, not stomata.

Reviewer: *Restricting water loss while at the same time allowing CO2 to enter the leaves is the reason for the existence of the stomata. This can be seen in any text book, for example 'Plant Physiology', 3rd ed, Lincoln Taiz and Eduardo Zeiger, p. 59 (open source). A quick web search reveals similar statements in the abstract or opening paragraph of a large number of scientific articles. Here are two examples: "Almost all water used for plant growth is lost to the atmosphere by transpiration through stomatal pores on the leaf epidermis. By altering stomatal pore apertures, plants are able to optimize their CO2 uptake for photosynthesis while minimizing water loss." (L.T. Bertolini et al, Front. Plant Sci., 2019, vol. 10, https://doi.org/10.3389/fpls.2019.00225). "In order for plants to function efficiently, they must balance gaseous exchange between inside and outside the leaf to maximize CO2 uptake for photosynthetic carbon assimilation (A) and to minimize water loss through transpiration. Stomata are the 'gatekeepers' responsible for all gaseous diffusion, and they adjust to both internal and external environmental stimuli governing CO_2 uptake and water loss. " (T. Lawson and M.R.Blatt, Plant Physiol., 2014, vol.164, pp 1556–1570.)*

Response: Even if this is published in the scientific literature, these statements are incorrect. Plants do not simultaneously maximize CO2 uptake while minimizing evaporative loss. This incorrect perception may result from the lack of a system's perspective when focussing on the gas exchange of an isolated leaf. When looking at the broader scale of the land-atmosphere system, it is well known and confirmed with observations that land surfaces essentially evaporate at their potential rates if water availability does not act as an additional constraint. This is, for instance, reflected in the Budyko framework (see previous comment).

Action: I have added text to clarify that this is not the case (see also previous comment). I have also added a few explicit sentences on this misinterpretation (lines 905ff): "What this does not imply, however, is that vegetation maximizes carbon gain while minimizing water loss, as it is sometimes incorrectly stated in the plant-ecophysiological literature." I have, however, decided to not include the references provided by the reviewer. This misconception is widespread, and I do not want to single out one or two references for this notion.

Reviewer: *This means that the plants need to restrict the flux of CO_2 when access to water is limited. The facts that the water use efficiency is fairly constant across ecosystems, and that the energy efficiency of photosynthesis is far below the theoretical limit, imply that this regime of simultaneous water and CO_2 limitation is normal. The plants handle this by regulating the size of the stomata openings.*

Response: Yes, plants do reduce their stomatal conductance in the presence of water stress. In the climatological mean and for natural ecosystems, this water limitation is accounted for by limiting evaporation to the precipitation rate in water-limited regions. As can be seen by the evaluation in Figure 14, this works rather well - the thermodynamic efficiency of terrestrial ecosystems can be predicted very well in both, humid and arid regions, irrespective of water limitation and specific stomatal functioning. Stomatal effects thus do not need to be taken into account for this insight.

Action: As this review is not about the role of stomatal functioning in the presence of water stress, I have decided to not include additional text to clarify this point.