Response to Reviewer #3 (Jonas Nycander)

I thank the reviewer for the critical review. In this response, I respond to all points raised. Most of these points can relatively easily be addressed by further clarifications. It should also be noted that this manuscript does not describe novel results, but summarizes previously published papers on the topic that were reviewed by experts of the respective disciplines and brings these together into one bigger picture in this review. So I think that the reviewers recommendation to reject is unjustified.

In the following, I tried to disentangle the review into separate points, addressed these points by clarifications, and describe how I will accommodate these in the revision. In the point-by-point response, I will state the reviewer's comment in *italics*, followed by my response and description of the action to be taken.

Major Comment 1a: *Thus, the solution is exactly half way between the two extremes mentioned above. Clearly, the optimization principle is equivalent to the choice $c_2 = 1/c_1$ in eq. (2). However, no motivation is given in the manuscript for why this particular choice should be universally valid.*

Response: I agree that the mechanism by which this maximum can be obtained is not included. The maximization is likely achieved by the way that frictional dissipation arranges itself within the atmosphere - a process that is currently described by semi-empirical parameterizations in climate models. This is consistent with earlier studies, e.g., Kleidon et al. (2003, GRL) with a simplified GCM that showed that one could get a state of Maximum Entropy Production (MEP) by adjusting the friction time scale. As I describe in this manuscript, MEP is very similar to maximizing power and dissipation.

Furthermore, the dynamics to maximization likely involve two contrasting feedbacks that operate at different time scales and that are modulated by the intensity of friction (Kleidon, 2016, Cambridge Univ. Press): If we perturb a system that is initially at rest, a fast, "power enhancing" positive feedback would enhance perturbations that yield more power, kinetic energy, more heat transport, thereby feeding back to yield more power; and a slower, negative "gradient depletion" feedback by which more heat transport leads to a greater temperature depletion, hence less power, kinetic energy and heat transport. The result is then that at the maximum power state, the state is governed overall by a negative feedback (as also described by Ozawa et al., 2003, Rev. Geophys.).

I would also like to point out that I do not claim that this choice is universally valid. It simply is a thermodynamic limit, so that the atmosphere cannot generate more power than this. For the present Earth, it seems that atmospheric motion operates at this limit, particularly when it comes to dry convection over land. It thus represents a highly relevant constraint that can provide closure to the surface energy balance. But other constraints, such as those imposed by the angular momentum balance, could reduce this limit to lower power and dissipation. This has, for instance, be shown by idealized GCM simulations by Pascale et al. (2013, Planetary Space Sciences) in which the rotation rate was varied.

Action: In the revision, I will add a subsection in Section 2 that describes the feedbacks involved in the maximization, I will clarify that this is a limit which is not necessarily reached, and add the reference to Pascale et al. (2013) in the discussion (Section 4.1) where the role of rotation rate is being discussed.

Major Comment 1b: *In fact, the external fluxes and the transport between the boxes are usually independent physical processes.*

Response: If I understand the reviewer correctly, I disagree. It is well known that the magnitude of atmospheric heat transport is reflected in the radiative exchange at the top of the atmosphere - they are not independent from each other. With no heat transport, thermal emission would balance solar absorption, but observations show very clearly that tropical regions absorb more solar radiation than they emit, while polar regions emit more than they absorb.

Action: I will add this explanation at the beginning of Subsection 3.1.

Major Comment 1c: *This certainly true in the cases considered in the manuscript, with the external fluxes given by Planck's law and the transport by the turbulent dynamics in the atmosphere, and it is easy to imagine changes that would affect one but not the other.* ***For example, a faster planetary rotation should decrease the horizontal transport in the atmosphere without affecting the radiative fluxes, thus decreasing c_2 while leaving c_1 unchanged.***

Response: With a faster planetary rotation rate and less heat transport, there is certainly a change in the radiative fluxes, as the tropics get warmer and the polar regions get colder. This is consistent with the simulations by Pascale et al. (2013) mentioned above.

Action: I think that the addition of the Pascale et al. (2013) reference, as described above, should address this point in the revision.

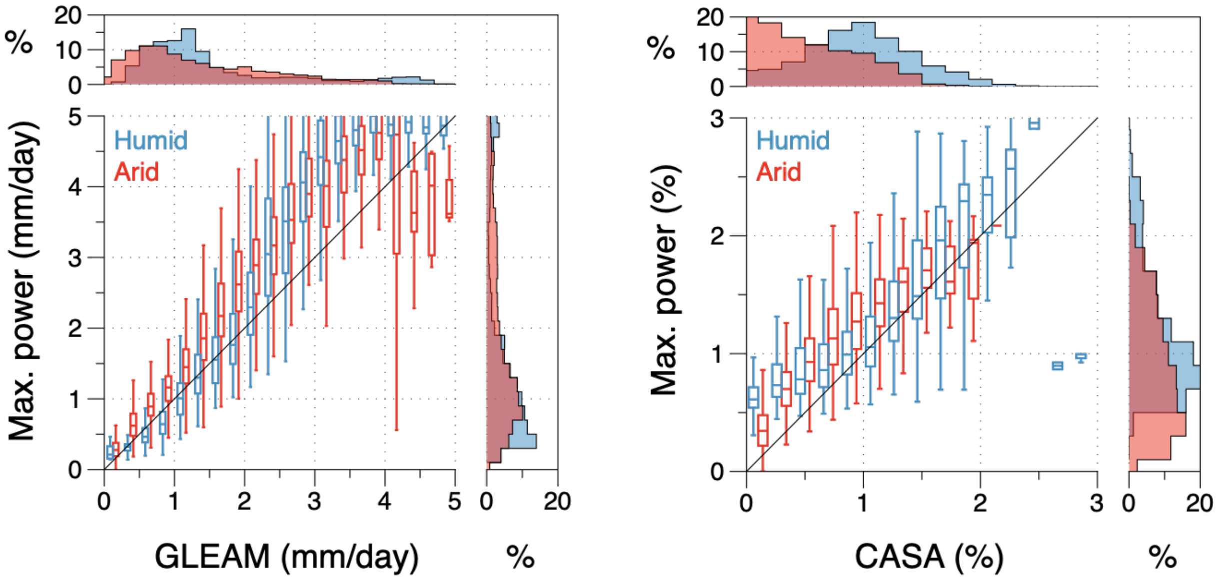
Major Comment 1d: *In the absence of physical motivation, the only support for the choice $c_2 = 1/c_1$ comes from the agreement with observations. However, in the case of horizontal transport in the atmosphere, the agreement is not very convincing. According to Fig. 8, J is approximately (2/3) J_0, rather than (1/2) J_0.* ***This very approximate agreement in only one data point could well be a coincidence****, and if there is a fundamental reason for it, this is not given in the manuscript.*

Response: There is not just the approximate agreement in terms of the heat flux, but also in terms of the power generated, which is similar in magnitude to the generation rate of kinetic energy in the Lorenz Energy Cycle and which serves as another means of comparison. Since power, heat transport, and temperature difference are closely interconnected, the agreement cannot be a coincidence. Furthermore, there are previous studies which have obtained very similar results from applying the maximization of entropy production (MEP), as described in the text.

Action: I will add this clarification to the end of Section 3.1.

Major comment 1e: *In the case of vertical energy transport in the atmosphere, no direct observations of this are given. Instead the energy transport is translated into evaporation by using the psychrometric constant, and the evaporation compared to observations.* ***However, the theoretical prediction is first combined with a precipitation dataset to account for water limitation, and it is most likely the precipitation data that are mainly responsible for the seemingly good agreement in Fig 3b****.*

Response: No, this is not correct. The thermodynamic estimate works equally well in humid regions where water availability does not constrain evaporation rate (i.e., precipitation rates are greater than potential evaporation), so that the rates in these regions are not determined by precipitation. This is shown in the revised figure 10b below, in which the individual gridpoints were separated according to water limitation into humid (blue) and arid (red) grid points. Since there are roughly as many humid (54.5%) and arid (45.5%) grid cells in the dataset, thermodynamics acts to constrain and set the evaporation rate in many regions on land, while less than half are determined by precipitation rate.



Action: I will update Figure 10b in the manuscript with the separation between humid and arid regions (as shown above) and the associated text. For consistency, I will update Figure 12b as well.

Major comment 2a: *In the section about photosynthesis, the rate of CO_2 assimilation (usually called GPP, 'gross primary production') is obtained by multiplying the estimated evaporation by a typical value of the water use efficiency (WUE).* ***Thus, no optimality assumption for the photosynthesis itself is involved****.*

Response: This is correct - there is no optimality assumption in here, and the low efficiency of photosynthesis is related to the bottleneck of gas exchange. Hence, the magnitudes of photosynthetic carbon uptake reflect the thermodynamic limit of atmospheric heat engines indirectly because these set the rates of evaporation and, therefore, gas exchange.

The application of optimality/maximization to photosynthetic carbon uptake is being described at the end of this section (lines 638ff). One primary example regards the role of rooting zone depth. In seasonal climates with prolonged dry periods, like in much of the seasonal tropics, a sufficiently deep rooting zone allows for seasonal soil water storage to contribute to evaporation from the surface. This effect is well known and documented in publications (see text for some references). This effect of rooting zone depth can enhance the power generated by terrestrial vegetation by 12%, as recently estimated by Kleidon (2023, Ecology Economy and Society - the INSEE Journal, https://doi.org/10.37773/ees.v6i1.915).

So there are aspects by which terrestrial vegetation can maximize the free energy generation further, as described in the manuscript.

Action: I will clarify the role of where optimality comes in and extend the description of the example of rooting depth.

Major comment 2b: *Instead, the result is a 'by-product' of the rate of evaporation obtained by using the optimality assumption for the vertical transport in the atmosphere. The agreement with data is not surprising, given that WUE is known to vary only moderately, and that the evaporation estimate is constrained by precipitation,* ***but is hard to see what this proves****.*

Response: As described above - evaporation is more than constrained by precipitation, and it needs a more differentiated picture to describe actual evaporation rates. This, after all, is one of the holy grails in hydrology to figure out the partitioning of precipitation into evaporation and runoff. So the notion that evaporation is simply precipitation is not correct. I refer to the explanations above regarding humid regions and the importance of sufficient seasonal soil water storage.

Action: See actions described above.

Major comment 2c: *There is hardly a direct causal relation, since excessive evaporation is typically detrimental to the plants, while a surplus of water is simply transported away as run-off. From the text the idea seems to be that the downward transport of CO_2 is governed by the same dynamics as the upward transport of water vapor, and that GPP is therefore another 'by-product' of the optimality assumption for the vertical transport in the atmosphere. However, the main resistance to CO_2-transport is not in the lower atmosphere,* ***but in the stomata of the plants, and it is caused by the need to save water****.*

Response: No, this is not correct. As described in the text, current stomatal optimization theory describes their function to maximize the carbon uptake for a given water loss (Medlyn et al. 2011, cited in the text). Additionally, the agreement of evaporation rates to those derived by thermodynamic constraints (as shown by the diagram above) suggest that stomata play very little role in controlling evaporation rates (see also Conte et al. 2019, cited in the text).

Action: I will include further clarification in the paragraph as there is an apparent need to clarify some of the basics.

Major comment 2d: *Thus, CO_2 limitation and water limitation are two sides of the same coin, and increasing turbulent transport in the atmosphere is unlikely to increase GPP. The most natural explanation for the agreement seen in Figure 12 is that both the theoretical estimate and the real GPP are limited by the precipitation.* ***This adds nothing to the common knowledge in the field.***

Response: Again, the reviewer has a too simplistic view on the controls of evaporation (see responses above).

Action: See above.