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Interactive comment on "Quantifying energy use efficiency via maximum entropy production: A case study from longleaf pine ecosystems" *by* Susanne Wiesner et al.

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Summary:

This manuscript describes a study in which entropy production is used to assess the resilience of ecosystems. To do so, this study diagnoses entropy fluxes and entropy production and describes these for three observational sites. In principle, I think that the approach is certainly unusual and novel. Yet, they way that entropy fluxes and production are diagnosed shows some major deficiencies which will certainly impact the results their interpretation. Also, I am not at all convinced that there is extra insights gained by looking at entropy production rather than a conventional analysis of

the energy, water, and carbon balances. Hence, I cannot recommend publication in this form.

Major comments:

1. Diagnosis of entropy fluxes and entropy production

I think that there are some major flaws in the methodology of how entropy fluxes and entropy production are being diagnosed. I understand that the authors use formulations from previously published papers, yet, as I will describe, I think that these are incorrect.

First, the entropy balance is used in Eq. 9, stating that the "overall change in entropy production (S) over time (t) in kJ m-2 K-1 of the ecosystem [is estimated] by adding entropy flux and entropy production". This is incorrect. What Eq. 9 formulates is the entropy balance. It balances the change in entropy on the left hand side of the equation (dS/dt) with the sum of all entropy exchange fluxes (J) and all entropy production terms (σ). This balance is typically assumed to be zero in a steady state, i.e., dS/dt = 0, which then allows one to diagnose entropy production from the difference in entropy exchange fluxes. This is in fact what the authors do to diagnose entropy production in Eqs. 3.6 and 3.7 to diagnose entropy production by absorption of radiation. Yet, the authors later use dS/dt in Eq. 4.8 to derive an efficiency. This efficiency should be zero, otherwise they did not do the balancing correctly. So there is a major inconsistency in the methodology that needs to be resolved.

Second, entropy production by absorption of longwave radiation is estimated using net longwave radiation at the surface (Eq. 3.7). What is the justification for using net longwave radiation, rather than gross fluxes? After all, the downwelling longwave radiation of the surface adds an entropy flux of Rldown/Tsky, while the emission of radiation from the surface exports entropy at the rate of Rlup/Tsrf. Using the difference of these two fluxes (assuming that dS/dt=0) yields an entropy production of σ = Rlup/Tsrf - Rldown/Tsky, which is not the same as (Rlup - Rldown) * (1/Tsrf - 1/Tsky). The authors should correct this, or explain why their expression is justified.

The same reasoning applies to the application of net ecosystem exchange, where I think that also gross fluxes should be used, not net fluxes.

2. Additional insights gained from entropy fluxes and entropy production

The authors link their entropy-based analysis to rather general concepts such as resilience and energy use efficiency. Yet, I do not see the additional insights gained by using entropy production, rather than an analysis based on the entropy, water, and carbon balance. Why does the entropy-based analysis provide more or novel insights that cannot be obtained by just an interpretation based on fluxes? The authors do not really answer this question within the manuscript and do not use the results to show this, as they only focus on an entropy-based analysis.

In terms of interpreting the observations, I think that there is a critical step missing that relates the observed differences to an interpretation of processes, and this cannot be gained by just looking at entropy. For instance, temperature changes result from changes in the energy balance, as temperature is a measure for heat content. Yet, the energy balance is not even shown or discussed. Likewise, to understand changes in evaporation, I would expect a water balance being discussed. Instead, this study directly diagnoses entropy fluxes and thereby skips this process-based level of interpretation. It does not show and interpret the fluxes of the energy, water, and carbon balances separately, and does not demonstrate that something else can be learned by looking at entropy.

By lumping all aspects of the land surface into entropy production, I think that this neglects those aspects that are relevant for ecosystems from those that are irrelevant. The relevant flux for ecosystems is primarily the uptake of carbon, as this provides the chemical energy for terrestrial ecosystems. Plants live from the energy they fix during carbon assimilation, and, quite frankly, care little about the entropy production of other processes.

For this manuscript to provide more solid insights, I think it needs a more process-

based interpretation using the available data, it needs to be more specific regarding those terms that are really relevant to ecosystems, and it needs to at least discuss why there is more to be gained by looking at entropy-based diagnostics.

Minor comments:

Abstract: "Our study provides foundational evidence of how MEP can be used to determine resiliency across ecosystems globally" - I am not at all convinced and doubt this conclusion. The authors provide no discussion why a diagnosis based on entropy fluxes yields more or better insights than the diagnosis of energy, water, and carbon balances. I see this as a critical missing bit in this manuscript.

Introduction, page 2, line 16: MEP is referred to as a principle in the text. At best, it is a "proposed" principle, or better hypothesis, as it is not generally being accepted.

page 2, line 24: How can agricultural systems exceed MEP if MEP already describes the maximum? This does not make sense. What I can imagine is that agricultural systems maintain a different state because of nutrient inputs, but then, the boundary conditions are changed because there are additional exchange fluxes across the system boundary. Also, why would this excessive entropy production be unsustainable? As long as the nutrient input can be maintained, I see no reason why it should be unsustainable.

page 3, line 9: What are entropy efficiency ratios? In thermodynamics, efficiency is used to describe the conversion efficiency of one form of energy into another, and this involves entropy (like the well-known Carnot limit). But to speak of efficiency for entropy does not make sense to me.

page 6, line 4: How can two unknowns (GEE and Reco) be estimated from one equation? I think there is some information missing here.

page 6, line 8: The authors convert the units from W m-2 K-1 to kJ m-2 K-1. The unit should be kJ m-2 K-1 month-1 (i.e., the time is missing, throughout the whole manuscript), since entropy production refers to a rate, and not to an amount. But I do not understand the motivation for not keeping the units of W m-2 or W m-2 K-1 that are much more standard. Some explanation why this has been done would be helpful.

page 6, line 14: Radiative entropy production actually includes a factor of 4/3, as it does not deal with heat, but with radiation (the additional contribution of 1/3 is due to radiation pressure). I think it needs a brief explanation why this factor was omitted.

page 7, line 2: What do you mean by "to calculate the change in entropy of the metabolic system". Do you refer to entropy production? If you want to estimate entropy production, this would relate to dissipation of carbohydrates, which in turn relates to respiration. So I do not understand why NEE is being used.

page 7, line 14: Why is net longwave radiation being used to calculate entropy production? The entropy fluxes of longwave radiation are Rl,down/Tsky and Rl,up/Tsrf as the authors write earlier in the manuscript. But this is not the same as Rl,net * (1/Tsrf - 1/Tsky). (See major comment above)

page 7, line 20: dS/dt refers to the change in entropy with time, not change in entropy production. It should be zero in steady state, otherwise one cannot calculate entropy production from entropy fluxes. (See major comment above)

page 7, line 29/30: Why are these expressions referred to as MEP? I see no connection to MEP. They just formulate radiative entropy production. Also, what's the difference to Eq. 3.6 and 3.7?

page 8, line 3: "an ecosystem maximizes its entropy production when it converts all incoming Rs and Rl into work". This is not correct. First, work is something different than entropy production. Second, it is impossible to convert all incoming radiation into work, as it would imply that there is no energy left to maintain a temperature that is greater than T = 0K.

page 8, line 3: "... MEP.. is often negative or 0". No! Entropy production must always

be greater or equal to zero, otherwise there is something wrong in the formulations! Spontaneous reductions in entropy are only possible at the microscopic scale during extremely short time periods but are practically irrelevant at the scale of ecosystems.

page 8, line 7: "maximum entropy of metabolism". What do you mean by this?

page 8, line 13: You express the efficiency as the ratio of the entropy flux associated with net ecosystem exchange to the energy flux of GEE. Should this not compare gross energy fluxes, rather than net exchange to gross exchange?

page 8, line 16: This expression merely describes a radiative entropy flux, but not entropy production, or a maximum in entropy production.

page 8, line 18: This expression does not give an efficiency, because in steady state (a condition needed to estimate entropy production from fluxes), dS/dt = 0 so this expression is zero as well.

I stop here with commenting, because I think that the methodology has a number of flaws that I wonder how much these impact the results. In addition, as expressed earlier, I think that the overall motivation for this entropy-based analysis needs to be improved.

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