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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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Title: Quantifying energy use efficiency via maximum entropy production: A case study from longleaf pine ecosystems Reviewer 1: Alex Kleidon Reviewer's comment: 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,

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Discussion paper

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.

Authors' response: Thank you for your comment, you are correct, and acknowledge the wrong use of the term dS/dt. For the revisions we changed this calculation method and instead now focus on the entropy outputs and inputs and internal entropy production to quantify if dS/dt = 0 holds in our systems, which we have added to the revisions of the manuscript.

Reviewer's comment: 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 long- wave 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 - Rl- down/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.

Authors' response: Thank you for pointing out the mistake. We have adjusted our calculations following the Brunsell et al. (2011) approach using incoming longwave radiation as follows: Rl,in x (1/Tsrf-1/Tsky). We acknowledge that calculating the Rl,up/Tsrf and Rl,down/Tsky will estimate the incoming and outgoing entropy transfer associated with longwave radiation, but not the entropy produced due to absorption of longwave radiation and conversion to heat during this process (as shown in Brunsell et al. 2011). We have also changed our analysis to using half-hourly gross fluxes of GEE and Reco

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following your comment.

Reviewer's comment: 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.

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Authors' response: Thank you, we have added an analysis and discussion of energy fluxes and the sites' energy balances to show the novelty of the entropy approach and to highlight that entropy production gives more insights about the energy efficiencies and resilience to drought at our sites.

We have included soil moisture content and rainfall in our analysis to quantify changes in entropy fluxes and entropy production due to changes in soil moisture and rainfall, but an analysis of the entire water budget was beyond the scope of this research project. We kindly disagree with the reviewers comment that the relevant flux for ecosystems is solely the carbon flux. For ecosystems (encompassing not only plant organisms), the partitioning of heat fluxes plays a significant role in their function, because the physical and biological processes are interconnected. LE in particular plays a large role in the maintenance of the surface temperature in ecosystems and is one of the largest contributors to entropy export in our ecosystem. However, we have adjusted our analysis according to your comment and are now using gross fluxes of GEE and Reco, to estimate the entropy change due to metabolic processes at the three sites. 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.

Authors' response: We will adjust the discussion and methodology accordingly, to show that entropy production indeed gives more insights about differences in resilience at the three longleaf pine sites, in contrast to solely using energy fluxes. In our revisions we will focus more on the entropy import and export, as well as the internal entropy production, to quantify how close these systems are to a thermodynamic steady state. This could not be accomplished using solely energy fluxes.

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Discussion paper

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. Authors’ response: Thank you for your comment. We will adjust the sentence accordingly. 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.

Authors’ response: In the papers we cited (Patzek et al. 2008 and Steinborn and Svirezhev 2000) the energy efficiency of agricultural management practices was determined, amongst other things using entropy metrics. Patzek et al. determined this unsustainability using gross and net primary productivity measures, as well as biomass estimates. The change in entropy was determined by quantifying anthropogenic energy inputs (fertilizers, herbicides, pesticides, fossil fuels, electricity, etc.) and energy production and respiration rates by the crops, and comparing it to a system which the agricultural practice displaced (here a prairie system). So here the “maximum entropy production” was equal to the productivity of an unmanaged ecosystem. The authors note that: “Excess entropy generated in an agrosystem manifests itself mostly as soil degradation by chemical and mechanical means, and toxic effluent runoff.” This basically implies that intensive agriculture requires energy from “outside the boundaries” (i.e. fertilizer production elsewhere) to meet increasing production demands and to balance soil degrading processes, due to the intensive management. Steinborn and Svirezhev (2000) provide similar measures, showing that a decrease in energy inputs from anthropogenic sources, or an increase in biomass production at similar energy inputs could decrease the excess of entropy production and therefore make the system more sustainable. We expand on this in the revisions of the paper as well.

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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.

Authors' response: Thank you for your comment, we refer to these ratios as entropy efficiency ratios, as what they are describing in relation to maximum entropy production are how close these systems are to MEP. In the revised version we describe this more effectively to eliminate possible confusion. However, following your comment we have changed the section concerned with the ratio of all ecosystem fluxes. We are now quantifying how close these systems are to a steady state by estimating dS/dt using entropy inputs and outputs and internal entropy production. The revised version will solely quantify the ratio of internal entropy production of radiation to MEP.

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

Authors' response: Thank you for your comment, this method was described in more detail in other published studies from this lab following common approaches to partitioning eddy covariance data. We now add a more detailed description of how these estimates are obtained.

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

Authors' response: We have adjusted the units accordingly. 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. Authors' response: We avoided this factor previously, due to the controversy surrounding this factor (see Ozawa et al.

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2003; Kleidon and Lorenz, 2005; Fraedrich and Lunkeit, 2008; Kleidon, 2009; Pascale et al., 2012) and because we assumed that the incoming and outgoing radiation does not assert radiation pressure. We will add more explanation of why we chose to omit this factor to the revisions.

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. Authors' response: For the revised paper we have adjusted our analysis accordingly and are now estimating the change in entropy due to metabolic processes using the half-hourly gross fluxes of Reco and GEE.

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)

Authors' response: Please see our response 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) Authors' response: You are correct, and we acknowledge the wrong use of dS/dt. As noted above, for the revisions we will change this calculation method. 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?

Authors' response: Thank you for your comment, as we note following Eq. 4.3 under an ideal case MEPRL would be zero, if the system transfers all energy from available radiation into LE, rather than H. To avoid confusion, we will revise this section and make it clearer that we are talking about an assumption or an empirical maximum

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entropy production (as shown in Stoy et al. 2014). Even if this assumption does not necessarily reflect reality, it still gives us a means to compare different ecosystems or sites with each other with respect to how they reflect, absorb and emit radiation.

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.

Authors' response: As noted above, we will change the dS/dt section, which will exclude this analysis. Instead we will quantify the sum of entropy imports and exports, as well as entropy production, to determine dS/dt.

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. Authors' response: We note that this sentence refers to the empirical maximum entropy production of shortwave and longwave radiation. MEP of longwave radiation is usually small when considered as part of the whole system. Here we assume that an ideal system partitions all incoming energy into LE (and M and G), rather than H, such that the temperature difference between surface temperature and the temperature of the overlying air mass would approach zero (Tsrf = Tair, ref. Stoy et al. 2014). We have added a better description of that assumption to the methods.

page 8, line 7: "maximum entropy of metabolism". What do you mean by this? Authors' response: We indeed intended to refer to the maximum decrease of entropy due to C assimilation of plant organisms in our systems, calculated by quantifying energy uptake as Ein from GEE and the simultaneous decrease in entropy. We have changed our analysis to using gross fluxes of GEE and Reco and we are now calculating the

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change in entropy of metabolic processes from the time of day when these fluxes occur. We will elaborate on this more in the revised document.

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.

Authors' response: In the revised paper we are now estimating the entropy decrease due to C assimilation during the day and the entropy increase through Reco during day and night, to calculate the change in entropy of metabolic processes. page 8, line 16: This expression merely describes a radiative entropy flux, but not entropy production, or a maximum in entropy production.

Authors' response: Thank you for your comment. As we altered the section including the whole ecosystem entropy budget, this section was omitted.

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.

Authors' response: As noted above, we are altering the calculation method to describe ecosystem efficiency in terms of entropy fluxes and production by focusing on the ratio of entropy outputs to inputs, as well as the internal production of entropy. 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. Authors' response: We will clarify the motivation of this study, as it was not as transparent as we intended. With this study we wanted to show that sites exhibit differences in their energy use efficiencies due to differences in energy partitioning and entropy production, which in part is due to differences in surface, air and sky temperatures. The Intermediate site for example maintained higher surface and air temperatures compared to the other sites (except for the years 2012 and 2013), which for example lowered its

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entropy production and the entropy flux of LE.

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