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Discussions

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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Response to reviewer 2 Major Comments Reviewer's comment: The authors need to provide a more detailed overview of the concept of entropy. Are these concepts definable for biological systems? What are the caveats? How do they fit in with the second law of thermodynamics (and concepts of disorder and free energy)?

Authors' response: Thank you for this comment. We will add more background information on the concept of entropy, specifically tailored towards biological systems, with our revisions.

Reviewer's comment: The framework presented in this study is built on Stoy et al., 2014, which in turn used a formulation by Holdway et al., 2010. These essentially simplify the concept of entropy to temperature normalization of fluxes of energy, carbon and water exchange. While a temperature normalized index for these quantities is likely to be highly useful in itself, does it warrant invoking entropy? Moreover, there are several inconsistencies, and not adequate explanation for how entropy for different fluxes is estimated. For instance, eq 4.6. which the authors define as the entropy efficiency of metabolism, is essentially a ratio of NEE:GPP. This has been previously identified as carbon use efficiency and extensively studied (for. e.g. see DeLucia et al., 2007 and references therein). In many instances, it is unclear how energy and entropy are related. It would be useful to present side-by-side comparisons.

Author's response: You are correct. The concept of entropy applied in our study is essentially a normalization of energy fluxes to temperature. We are now using half-hourly gross fluxes of GEE and Reco to quantify the change in entropy of metabolic processes. As these fluxes occur at different temperatures (GEE during the day and Reco during day and night); this will go beyond an analysis of the carbon use efficiency.

Three examples Reviewer's comment: 1. Page 3, line 3: how does the entropy dissipation through sensible heat relate to energy dissipation? These concepts need to be clarified. 2. Fig. 4. Why look at JLE instead of LE fluxes? What is additionally learned from this? 3. Page 10, line 31. JNEE not being related to soil moisture. This claim (I say claim since data is not shown) would be highly interesting if it is contrasted with the NEE response to soil moisture. There are more rigorous formulations (e.g. Wu et al., 2017) as well as critical discussions (e.g. Volk and Paulus, 2010).

Authors' response: An analysis of entropy fluxes is preferable in ecosystems which are exposed to different environmental variables, as differences in surface and air temperatures can affect entropy production of energy fluxes of LE and H. For example, two systems could have similar magnitudes of LE, but differ in JLE due to differences in air or surface temperatures. For the ecosystem which maintains a higher surface/air

temperature, the entropy flux would be lower, suggesting that it is less efficient in exporting entropy across its boundaries. By calculating the difference between entropy outputs and inputs, as well as internal entropy production, one can estimate how close an ecosystem is to a thermodynamic "steady state" and therefore how organized it is. This could not be accomplished by looking only at the energy balance. We will add a more thorough introduction and discussion of why entropy metrics can be more useful in describing energy use efficiency. In our revised manuscript we will include figures for all results. Reviewer's comment: Another cause for concern is that that inferences are not quantitatively supposed. There are several instances where analysis is restricted to 'eyeballing' relationships between different curves, and correlation coefficients are not presented. In some occasions this leads to the authors making inferences that are not backed up by the data that is presented. Author's response: We will add tables of Type III effect summaries for all models as supplementary tables, as well as add indicators to the plots where differences between factors were significantly different.

Reviewer's comment: The writing is overly descriptive, and often disconnected with the conclusions. Is this study describing entropy fluxes and efficiency ratios and how these vary with different environmental conditions, or is it trying to use these variables to understand site differences? The result is an unclear combination of the two. I would recommend the authors to stick to a storyline that is supported by the data.

Authors' response: Thank you for this valuable comment. We included the effects of environmental variables to understand changes and differences in entropy production and fluxes and thus changes in energy efficiency at our three sites. In our revisions we will make this more clear.

Reviewer's comment: Finally, there are several instances where the authors discuss the effect of soil moisture and rainfall on various fluxes/processes in the text (e.g. lines 13,19, 31 on page 10, line 25 on page 11) but do not choose to show these data. In my opinion these data are critical and need to be discussed (since it is a drought recovery study). Authors' response: You are correct, and we will add figures showing their

effects, as we did include these variables in our models. However, rainfall often had no significant effect on the response variables by site or as a simple effect. Reviewer's comment: In light of these observations, I would not recommend this manuscript in its current form for publication in Biogeosciences. I think the authors provide very valuable observations, but should consider either re-framing the study or provide a more critical discussion on the concept of MEP, as well as consider extensive revisions on the writing as well as presentation of data.

Authors' response: Thank you. We will add a more thorough discussion. Figures

Reviewer's comment: There are several instances where curves are classified as significantly different, but do not appear significantly different from each other at all (Fig. 1d, for instance). The authors need to expand figure captions, since in the current form it is hard to infer what is being shown. E.g. Figure 4 has three time series (one for each site in most panels) but only one for sub panels b and e. It is unclear what data are presented. There are similar issues with Figs. 5-7.

Authors' response: We will add a supplementary file with all type-3 tests of effects for all models included in this manuscript to show significant differences among the independent variables where applicable. For Figure 4, 5 and 7 we kindly refer to the Figure captions, where we note that when only one black line is shown, the interaction with site was not significant. For example, for Figure 4, we wrote: "For (b) and (e) the interaction with site was not significant."

Reviewer's comment: I also feel that the authors rely on too much on summarizing data and do not explain how or why this is done (again, eg. Fig 4b and d). What are the data that are presented in these analyses?

Authors' response: Following your suggestion, we are changing our analysis to estimate entropy from mean half hourly energy fluxes for daily time-steps (W m-2 K-1) to look at differences in energy and entropy metrics.

Reviewer's comment: The authors need to include sub panels in the text (Fig. 4a, b etc.).

Authors' response: Thank you for this suggestion. For our revisions we are adding sub panel information to the text.

Reviewer's comment: Figure 1 has inconsistent units for temperature. For instance, subpanels c and e are plotted in units of Kelvin but d and f are in deg. C. Also, VPD is plotted in Figure 1 but not discussed at all amongst other discussions of Fig. 1 (Sec. 3.1).

Authors' response: We used air temperature in degrees C for our model formulations, but for the calculation of entropy we converted air temperature to Kelvin. We will change Figure 1 to have consistent units. We will also add a more thorough discussion for VPD.

Reviewer's comment: Fig 2. Why are monthly means shown here, while the rest of the paper annual means are presented?

Authors' response: We used monthly means of entropy transfers and production for all of our analyses. As we are now using mean daily estimates, we will revise this in the text.

Reviewer's comment: Table 1: Please provide LAI estimates (if available) and also disturbance history, since this is a key component of your overall conclusions.

Authors' response: We are adding LAI data when available (for the mesic and xeric site), as well as disturbances history for all sites to the Table.

Minor comments Reviewer's comment: Page 2 Line 1-2: Turbulent exchange of... specify (for e.g. momentum, heat, gases). Line 3: Maybe just use examples related to terrestrial ecosystems? Are these examples of the butterfly effect in terrestrial ecosystems?

Authors' response: Thank you for your comment. We have added heat fluxes to the

sentence, as part of our story was focusing on the partitioning and entropy transfer through LE and H. We will also add examples of the butterfly effect in ecosystems.

Reviewer's comment: Page 5 Lines 5-9: This assumes energy balance closure. Please describe why you closed the energy balance.

Authors' response: To accurately describe the entropy balance for ecosystems, we are required to have a closed energy balance. Following this and the comments from reviewer 1 (Alex Kleidon) we will add a more detailed description of the energy balance and energy partitioning at the three sites, which will include a better description of why we chose to close the energy balance using the Bowen method.

Reviewer's comment: Page 6 Eq. 2: Describe briefly how NEE was partitioned into source and sink terms.

Authors' response: Thank you for your comment. We will add a more detailed description on how we partitioned NEE into GEE and Reco following Whelan et al. (2013) and Starr et al. (2016).

Reviewer's comment: Page 7 eq. 3.6. and 3.7: Unclear why net fluxes are used. Line 23: Are periods of rainfall excluded from the analyses? Where is this described? eq 4.1 and 4.2: Why is 4.1. formulated using incoming radiation whereas as 4.2 using net fluxes? Authors' response: We have revised this section and are estimating entropy production as follows:

and

Reviewer's comment: Page 8 eq. 4.8 is essentially carbon use efficiency (see major comment above).

Authors' response: We are revising our analysis accordingly, and we are now using half-hourly gross fluxes of GEE and Reco to quantify the change in entropy of metabolic processes. As these fluxes occur at different temperatures (GEE during the day and Reco during day and night), this will go beyond an analysis of the carbon use efficiency.

Reviewer's comment: Page 9 Line 11. Subpanels missing. Lines 21-24: temperatures differences do not appear to be significantly different across sites in Fig. 1. Authors' response: We kindly note that even though these figures appear to not show significant differences in temperature across the sites, our statistical results indicate that there were in fact significant differences in temperature. We are adding the type-3 tests for all models to show this.

Reviewer's comment: Page 10. Sec. 3.2. Methods for this analysis are not presented. I think this section should be merged with Sec. 2.1. (site description), as it doesn't appear to be a result of this study (unless methods are presented). Line 14: Soil moisture data seems important here (and in other places). Line 15: VPD effects are discussed first but EVI figure shown first in Fig. 4. Line 23: This is not correct according to Fig. 4. Line 23: See major comment above.

Authors' response: Thank you for your comment. We have added a description of the methods that were used to calculate understory biomass for the three sites. We are also adding graphs for the independent variable of SWC. We are changing the description of VPD and EVI according to the order shown in the figures and will make sure that this is consistent throughout the text as well.

Reviewer's comment: Page. 13 Line 1: What does 'preservation' on LE mean? Again, these are hard to interpret in the absence of absolute fluxes (see major comment above). Line 8: Ecosystems do not 'experience' LE (or JLE), but rather the interactions between the ecosystem and the overlying atmosphere determines the LE flux. Line 13: Clarify what this means.

Authors' response: Thank you for your comment. As noted above, we are adding an analysis of the energy balance to show absolute fluxes. We will make sure to not "personalize" ecosystems throughout this manuscript.

Reviewer's comment: Page 14 Line 8: should read "at the more biodiverse site (i.e. mesic)" Line 11: What was the contribution of the C4 understory photosynthesis to

overall ecosystem photosynthesis? Did you measure this? Lines 25-30: This is incorrect. Annual (and monthly) changes in EVI do not reflect changes in biomass. Biomass includes the carbon stored in the trunks, branches and stems of trees (among other pools), which do not fluctuate in forests at these timescales. Instead, at these timescales EVI is a measure of canopy greenness that is related to net photosynthesis (see Sims et al., 2008). Authors' response: We have changed the sentence on line 8 accordingly. Unfortunately, we did not measure differences of the C4 understory at our sites over the course of this study. However, in Wiesner et al. (2018) we showed that the understory contributes about 50% to Reco, using soil respiration data. We will correct the definition of EVI. What EVI reflects is indeed the change in LAI (canopy

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