

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

Anonymous Referee #2

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Overall summary

Wiesner et al. analyze energy use efficiencies for three different forested ecosystems along a moisture gradient using the framework of maximum entropy production. This is an interesting approach but needs a far more critical overview and careful analyses than what is presented currently.

Major Comments

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)?

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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.

Three examples

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).

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.

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 dif-

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ferences? 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.

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).

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.

Figures

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.

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?

The authors need to include sub panels in the text (Fig. 4a, b etc.).

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).

Fig 2. Why are monthly means shown here, while the rest of the paper annual means

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are presented?

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

Minor comments

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?

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

Page 6

Eq. 2: Describe briefly how NEE was partitioned into source and sink terms.

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?

Page 8

eq. 4.8 is essentially carbon use efficiency (see major comment above).

Page 9

Line 11. Subpanels missing. Lines 21-24: temperatures differences do not appear to be significantly different across sites in Fig. 1.

Page 10.

Sec. 3.2. Methods for this analysis are not presented. I think this section should be

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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.

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.

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).

References

DeLucia, E.H., Drake, J.E., Thomas, R.B. and Gonzalez-Meler, Miquel., 2007. Forest carbon use efficiency: is respiration a constant fraction of gross primary production? *Global Change Biology*, 13(6), pp.1157-1167.

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Sensing of Environment, 112(4), pp.1633-1646.

Volk, T. and Pauluis, O., 2010. It is not the entropy you produce, rather, how you produce it. *Philosophical Transactions of the Royal Society of London B: Biological Sciences*, 365(1545), pp.1317-1322.

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