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Earth System Dynamics Discussions

Interactive comment on "Quantifying the thermodynamic entropy budget of the land surface: is this useful?" *by* N. A. Brunsell et al.

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Response to Comments

We wish to thank the three reviewers for their thoughtful comments. We feel that by addressing these comments the manuscript is now better.

Reviewer 1

Response to overall comment

This reviewer has raised one primary issue with the manuscript which is the connection between MEP and entropy production during ecological succession. While this manuscript can not fully address this, we have attempted to follow the reviewer's com-

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ment that this should be followed up in future work. We have added some text to the conclusions section specifically addressing the still open issues related to what this study can say about MEP, as well as ecological succession. Along the same lines, we have added text about whether eddy covariance data can be used to assess the validity of MEP at all, since (as the reviewer notes), MEP is usually applied to the atmosphere-surface coupled system in steady state. We still believe that using these observations are useful approach, particularly for assessing land cover variation and possibly land use history. Although we fully acknowledge that this study is a first step and more research is necessary.

Response to specific comments

All of the minor, textual modifications have been made as suggested. The following are responses to some of the more detailed comments that were raised by the reviewer.

3. We thank all three reviewers for noting the typos related to the signs in equation 1. This has been fixed.

4. We have changed the nomenclature for referring to the transport and production terms as suggested.

7. We have fixed the references for temperature between the figures and the text. In addition, we have added text were appropriate to illustrate the differences between the various temperatures, how they are determined from the model and the observations.

8. Yes, this is true and we have now stated this. However, in addition, there is the so-called 'energy balance problem' with eddy covariance observations where the measurements are not capable of closing the energy balance. In this case, it is a measurement/technique issue. In addition, there are some energy terms (e.g. photosynthesis) that are not accounted for. We have added text to this effect to attempt to make this more clear.

15. We suspect that this results from the local microclimatic impacts in the observa-

ESDD

2, C89–C93, 2011

tions and the fact that the atmospheric properties are imposed in the model. Thus the modeled fluxes are always responding to the atmospheric temperature etc. via altering the partitioning of the surface fluxes. In the observations, this is also the case, but there is also modification of the air temperature, humidity etc. which will then feed back on altering the surface fluxes. This has been expanded upon within the manuscript.

16. We have added text that clarifies the large standard deviation in the observations and the lack of statistical significance. This is used to suggest the trends as well as justification for further research over more sites with different land cover types and different land use/disturbance histories.

17. As stated above, we have attempted to downplay the connections with MEP and while we still point out some connections, we are now more explicit in the needs for further research into the connection between the application of the second law to ecological succession and the connection with the MEP hypothesis.

Reviewer 2

Response to general comments

We appreciate the reviewer's comments. As suggested by reviewer 1, we have more explicitly stated the (lack of) relationship between our study and the MEP hypothesis. While we point out that some of our findings are consistent with the hypothesis, we ultimately conclude that more research is necessary. However, even if the quantification of entropy terms from eddy covariance can not be used to illustrate MEP, they can be used as a useful metric for quantifying the thermodynamic state of the land surface.

Entropy and entropy production have been suggested in the ecological literature as potential measures of quantifying variation in land cover type, land cover disturbance history and sustainability (Schneider and Kay (1994), Svirezhev (2000), Steinborn and Svirezhev (2000), Patzek (2008), Tesar et al. (2007), Holdaway et al. (2010) as referenced in the manuscript) and while there is no agreed upon definition, the use of a

ESDD

2, C89–C93, 2011

metric that can quantify the 'distance from steady-state' may have potential. We feel that this is more than 'hand-waving' and an area of active research that is in need of further analysis. The reviewer's comment of (even if we know that the global temperature is not fixed) "... and all we can do is to manipulate the location of the entropy production for our own benefit" is perhaps a good justification for this manuscript. We wish to assess how different land covers and disturbance histories alter this entropy production so we can alter 'it for our own benefit.'

Response to specific comments

1. Thank you for pointing out the typo in the equation. We have fixed this.

2. Yes, we are not considering many transfer terms that in reality exist: rainfall, subsurface transport of water, friction, photosynthesis etc. We have added to text to the entropy budget methodology to specifically note that we are not considering those terms.

3. This statement has been expanded upon in the conclusions, along the lines of the suggestions of reviewer 1 for clarifying the role of MEP vs. entropy production in a non-steady state related to ecological succession.

Reviewer 3

Response to specific comments

1. We have fixed the typo in equation 1.

2. We have fixed the various references to the temperature terms in order to clarify the meaning. Tatm is the effective radiant temperature of the integrated atmosphere, calculated by inverting the Stefan-Boltzmann law from the downwelled longwave atmosphere. In the mid-latitudes this temperature is significantly cooler than the surface (even at night). This is fundamentally different than the near surface temperature (Ta) measured at some height (3 m, in our case) with the eddy covariance method. We have fixed the units and changed the nomenclature as well.

ESDD

2, C89–C93, 2011

3. We feel that this comment is generally the same as to why the model did not result in negative values at night and why the observations are a different magnitude. Ultimately, neither of these systems is in steady-state, and the box that defines the system should probably include the entire atmospheric column not the surface. We have expanded upon this discussion in the conclusions of the revised manuscript.

Interactive comment on Earth Syst. Dynam. Discuss., 2, 71, 2011.