

Comments on “Quantifying the thermodynamic entropy budget of the land surface: is this useful?” by Brunsell, Schymanski & Kleidon

General comments

Is the entropy budget of the land surface a useful diagnostic for detecting and analysing processes such as land cover disturbance and ecological succession? This study explores how vegetation cover fraction and land cover type affect (a) the entropy exchange ($d_e S$) between a land surface layer and its environment (atmosphere & soil) due to various energy exchanges (short- & long-wave radiation, latent & sensible heat, heat flux to soil), and (b) the entropy production ($d_i S$) due to conversion of absorbed radiation to heat within the surface layer itself. These dependences are explored using the NOAH land surface model and eddy flux measurements at 3 contrasting sites. The authors conclude that $d_i S$ has a consistent sensitivity to land cover, implying “yes” to the question posed.

As the authors note, the methodology used is fairly simple, and the conclusions are therefore tentative and need to be backed up by more extensive studies. I would agree, since in particular it is not clear if the estimated differences in $d_i S$ between the 3 sites are statistically significant.

At various points the authors make suggestive links between their study and the conjecture of Maximum Entropy Production (MEP). However, it is not yet clear whether $d_i S$ is the appropriate quantity for applying MEP to land cover changes. Climate modelling studies suggest that MEP may apply to the entropy production associated with surface-to-atmosphere latent and sensible heat fluxes [e.g. Ozawa & Ohmura, *J. Climate* 10: 441–445 (1997); Herbert *et al.*, *Earth Syst. Dynam. Discuss.* 1: 325-355 (2010)], so perhaps the atmosphere needs to be included as part of the system, especially since atmospheric transport adjusts on faster timescales than land cover. The authors’ analysis using the NOAH model excludes surface-atmosphere feedbacks, however.

Another issue here is that the authors seem to be referring to MEP in a dynamical sense (entropy production increasing with ecological succession) rather than in a steady-state sense (selection between different steady states); it is not yet clear how these different hypotheses fit together. Ultimately, the link between this study and MEP will require a better understanding of the theoretical basis of MEP itself.

In summary, this study represents a useful benchmark for future work.

Specific/technical comments

1. Typo, p74, line 1. Delete extra 'in'.
2. p74, line 7. By 'altering' do you mean 'increasing'?
3. The sign convention in eqn (1) looks wrong. I think the r.h.s. should read $H+LE+G+\varepsilon$ to be consistent with the sign convention in eqn (9) and Fig. 1 in which H , LE and G are defined as positive in the direction *out of* the surface layer (*i.e.* they are export fluxes). Then $\varepsilon = R_n - H - LE - G$ can be interpreted as the net instantaneous rate of heating of the surface layer ($\varepsilon = 0$ in steady state), rather than as an estimation error. This is relevant to the later interpretation of dS – see comment 8 below.
4. p76, line 10/11. It might be clearer to say 'entropy transfer into the surface layer' rather than 'entropy transport' (you use 'transfer' later on p77, lines 3, 8 and 13; better to be consistent). I'd also recommend using the notation J instead of $d_e S$ because you are talking about a flux of entropy rather than a small increment of entropy; also use σ instead of $d_i S$ for the rate of entropy production within the surface layer. This recommendation also applies to eqns (2)-(11), *e.g.* J_{Q_s} instead of $d_e S_{Q_s}$, σ_{Q_s} instead of $d_i S_{Q_s}$ *etc.* The increment 'd' notation should be avoided.
5. On p76, line 19, subscript 'L' should read ' Q_L '; and on line 21 after 'longwave radiation' insert 'and conversion to heat'.
6. Are eqns (2), (3) *etc.* taken from Wu & Liu (2010)? If so, please cite their equation numbers.
7. It is not clear what is the relation between T_{atm} (eqn 4), T_a (Fig. 1), T_0 (eqn 13) and T_{air} (eqn 16, Fig. 2). Similarly between T_{sfc} (eqn 3, Figs. 2, 5 & 9) and T_s (Fig. 1). On p77 and p81 you refer in this regard to Campbell & Norman (1998) and Stewart *et al.* (1994), but these verbal statements are not explicit enough because they do not specify notationally which temperatures (T_{atm} *etc.*) you are talking about. It would help to be more explicit mathematically about how you calculated the temperatures T_{atm} and T_{sfc} appearing in the entropy budget eqns (2)-(8), both in the NOAH model and at the flux sites.
8. According to my calculations, if you insert eqns (1)-(10) into the 'entropy budget' eqn (11), using what I think is the correct sign convention in eqn (1) (see comment 3 above), then one gets $dS = \varepsilon/T_{sfc}$ (which would be better written with dS/dt on the l.h.s.) This is indeed what one expects if ε represents the net instantaneous rate of heating of the surface layer. Therefore $dS/dt = 0$ if the surface layer were in an instantaneous steady state. This is relevant to the interpretation of Figs. 4d, 7d, 8b & 11d; they reflect the non-stationarity of the diurnal surface layer energy balance.
9. Eqn (5) implies that $d_i S_{Q_L}$ is negative if T_{atm} is less than T_{sfc} , but this seems inconsistent with Figs. 2 and 3c. Presumably this is because T_{atm} is not the same as T_{air} , which comes back to comment 7 above.
10. p77, line 6. 'heat flux (H) is proportional to the temperature gradient'. Where is this assumption actually used? In the NOAH model? If so, it would be better to keep it out of the general presentation of the entropy budget, and present it in section 2.2.

11. Section 2.2 refers the reader to other papers for details of the NOAH model. Nevertheless it would be useful to know what are the key assumptions, *e.g.* what is prescribed in the model and what is calculated *etc.* specifically in relation to the fluxes and temperatures appearing in section 2.1. For example, at the top of p79 you state there is no surface-to-atmosphere feedback: does this mean that T_{atm} is prescribed?
12. p78. Avoid Fr as a symbol; it looks like ' F times r '. As a general rule, avoid the use of acronyms or multiple-letter abbreviations as mathematical symbols.
13. Section 2.3, p79. You contrast the three sites in terms of their different disturbance histories and vegetation types. Do they actually differ in vegetation fraction too? (*cf.* the NOAH model sensitivity analysis). If so, how?
14. p82, line 10. '*seasonal* mean diurnal pattern'?
15. p86, lines 10-14 & Fig. 11d. Can you explain why the observed values are an order of magnitude larger than in the NOAH model? Also why the NOAH values are always positive whereas the observed values are negative at night? This mismatch seems quite important to understand.
16. p86, line 19. How significant are the differences between the estimates of daily total d_iS at the 3 sites? For example, how does the difference between the values for K4B (23.81 JK^{-1}) and KZU (23.60 JK^{-1}) compare with the error bars on these values? These differences form the basis of your ranking of the 3 sites with respect to d_iS , and hence one of the main conclusions of the paper.
17. p88, lines 3 and 17. Do you mean 'maximise' or simply 'increase'? Also the subsequent reference to the MEP hypothesis (line 18) is too vague. The MEP hypothesis is usually applied as a selection principle between different steady states, rather than as a dynamic principle describing the approach to steady state. You need to distinguish these to avoid confusion. It might be safer to say that the link between your results and the MEP hypothesis is a subject for future work (see the general comments above).