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Discussions

Interactive comment on "Using the Maximum Entropy Production approach to integrate energy budget modeling in a hydrological model" *by* Audrey Maheu et al.

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This paper describes the application of a information-theory based Maximum Entropy Production (MEP) approach to partition turbulent fluxes into sensible and latent heat. The authors use three sites in the US to test this approach and compare it to one that is based on the Penman-Monteith (PM) equation. They showed that the MEP approach appears to work better than PM. I think this is a useful paper that is well written and carefully explained, but at present a little thin on insight. I think that some points need to be clarified, some additional evaluations would help to better interpret the results, and some disadvantages of the methodology need to be discussed before it may be

suitable for publication.

Major comments:

1. Information-based MEP approach: Despite the success in applying the MEP approach that was developed by Jingfeng Wang and that is shown in this manuscript, I have some reservations about the approach. First, by using six measurements, it seems to me that this is already quite a bit of information for the partitioning of sensible and latent heat and is probably already overconstrained. You use net radiation (minus ground heat flux), this already sets the magnitude of the turbulent fluxes, and then it is only a question about partitioning these into sensible and latent heat. Also, the variables are not independent from each other. Net radiation, for instance, combines net solar radiation with downwelling longwave radiation and thermal emission, with the latter being strongly correlated with temperature. So these input fields do not contain independent information. This aspect, however, is nowhere mentioned, discussed, or evaluated.

In addition I feel uneasy about this approach because it is not process-based. So would this approach also be able to predict the right sensitivity to, say, global warming, land cover change, or vegetation-caused phenology changes? It seems to me that with natural vegetation, it may have adapted so well to its environment that one does not see a sign of vegetation, but this may change with human-caused land cover change. So I am doubtful whether this approach can represent such sensitivities, because it is not really based on mechanisms. Because of this absence of mechanisms, I would also not refer to the approach as parsimonious.

I do not expect the authors to solve these issues, but at the minimum, I would expect the authors to discuss these thoroughly and evaluate potential impacts. It would need some critical evaluation of this approach and point out some further needs to evaluate, especially when advocating a non process-based approach.

2. Additional analyses: At the moment, I feel that there is relatively little done in terms

of analysing the conditions when one approach works better or worse than the other. What would help in this direction is to analyse the time periods when soil water or atmospheric demand are the primary limitations to ET. I think this would be easy to do and useful.

Also, I noticed in Fig. 4 that at the US-Ton site, evaporation seems to be consistently underestimated. I could imagine that this has to do with the relatively shallow rooting depths that have been assumed in both modelling approaches. The Tonzi site is in a mediterranean climate, and vegetation there is well known to have deep roots. The model uses rather shallow rooting depths of 1m or less, and such a depth could be too shallow. Also, in the model formulation of water limitation, it weighs root uptake with some sort of cubic decay function. This is not really how roots work. When water is available in a soil layer, it is being taken up if roots are there, and it seems this is fairly independent of biomass. So this formulation may also result in the low evaporation bias during the dry season.

So I think it would be instructive to include a sensitivity analysis to evaluate if both approaches can be improved by better rooting depth parameterisations.

Minor comments:

General:

Why do you use the Penman-Monteith equation as a reference? Milly and Dunne (2016) have, for instance, shown that it can lead to some systematic biases in sensitivity. Have you checked the Priestley-Taylor approach as well that presumably works better?

What is the uncertainty related to the lack of energy balance closure of the eddy flux data?

How do the fluxes look like when evaluated at the time scale of the diurnal cycle? At the moment, only daily means are being evaluated, but the observations should be

available at a higher temporal resolution. So why not look at and evaluate the simulation of the diurnal cycle as well?

Specific:

p4, lines 29-30. How are C1 and C2 “universal” constants? Also, why does the von Karman constant appear in the expressions? I thought the information-based approach does not rely on semi-empirical parameterizations of turbulent fluxes. Please clarify.

p5 Eq. 8. How does this equation for sigma relate to more common expressions in micrometeorology, such as the equilibrium Bowen ratio?

p5, line 32. Why is water uptake weighted by the vertical root distribution? There is quite some evidence for roots being able to take up substantial amounts of soil moisture even at low root biomass concentrations (see e.g., Nepstad et al. (1994) Nature).

p8 lines 10-15. Why did you not use the radiative surface temperature as the skin temperature that can be inferred from the longwave upwelling flux? It seems to me that the radiative temperature would be a more adequate representation of skin temperature.

Milly, P.C.D. and Dunne, K.A. (2016) Potential evaporation and continental drying. Nature Climate Change, 6, 946–949.

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