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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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Summary: The authors use the MEP constrained energy balance model derived Wang and Bras (2011) to simulate evaporation with the hydrological model "HydroGeoSphere". More specifically they couple the MEP energy balance model with the model "HydroGeoSphere" (HGS-MEP), and evaluate their approach within a long term uncalibrated simulation against energy flux data and soil moisture data of three distinctly different sites. Moreover, the authors compare their HGS-MEP model to the HydroGeoSphere standard using the Penman Monteith approach (HGS-PM) as a null model.



Evaluation: I very much enjoyed the reading of this study as I generally like the idea of using thermodynamic optimality for constraining the land-surface energy balance. I am also in favor of the proposed evaluation strategy. The scientific significance of the study is in principle high, as evaporation is certainly one of the most important fluxes when it comes to change. Moreover, the study is nicely written, well-structured, based on sound data and nicely illustrated. So I would definitely like to see it published in the ESD/HESS SI. Nevertheless, there are several important issues that need to be clarified in a round of major revisions before the study might become acceptable.

Major points:

M 1): From the presentation of the underlying theory it becomes neither clear how entropy production is defined in the model nor how it has been optimized. While I acknowledge that the study relies on an already published model of Wang and Bras (2011), it is important to share this with the readers. There are several fluxes which produce entropy in the soil-atmosphere vegetation system, while they deplete their driving gradients. The sensible heat flux, depleting the near surface gradient in air temperature, the evapo-transpiration flux depleting the gradient in partial water vapor pressure, and also the soil water flow depleting gradients in soil water potentials (e.g. Zehe et al. 2013). To which entropy production term is the model referring to, or is it referring to all?

M 2): The second major point closely relates to the first one. The proposed transpiration model is driven by dependent variables, particularly the relative humidity and the air temperature from the eddy covariance data are not independent form ET and H. I would expect that an optimization of these fluxes with respect to entropy production needs to account for the feedback of these fluxes on these driving gradients, by defining entropy production as flux times the driving potential difference divided by the absolute temperature. As this is not the case here, I wonder about the definition of entropy production (see M1).

M3) Last but not least the long wave upward flux is a function of the surface temperature and the emissivity in the thermal infrared. By using Rn as driver the authors constrain the amount of energy which is available for ET+H+G. This is a substantial constraint for the entropy production as well.

M4): The proposed results underpin very much that the HGS-MEP perform superior. But does it perform acceptable? The latter requires definition of a model acceptance threshold a priory., e.g. of NSE > x. At US-TON the soil water content and ET are underestimated by -5%, -11%. So where did the water go? The authors evaluate their model using daily mean values. I would be interested in seeing the model performance at the diurnal scale.

Minor points:

Line 60: I very much agree that hydrological model applications are largely insensitive to the choice of the ET model. But is this really a surprise? We calibrate the model to reproduce discharge – so do they have an alternative?

The NSE and the RMSE are not independent, so the authors might consider to skip one of the metrics? Page 2 line 45: PM is also constraint by Rn.

Eq. 3: I wonder why thermal inertia of liquid water is weighted by soil water content, thermal inertia of the solid phase is not weighted by the volume fraction of the solid phase.

From a soil physical standpoint field capacity is a scale dependent, the average potential value at which a probe stops gravity driven seepage depends on the height of the probe.

Eq. 15 and 17. I wonder about the definition of Ec.

Eq. 18: Are the theta_e1 and theta_e2 calibrated, if so this is a substantial constraint to entropy production?



Eq. 21:I wonder whether this relation is only valid for neutral conditions?

Figure 6: The deviations between the model and the observed soil water content value appear a little too large for an NSE of 0.61. Please double check.

Best regards,

Erwin Zehe

References: Wang, J. and Bras, R. L.: A model of evapotranspiration based on the theory of maximum entropy production, Water Resour. Res., 47, W03 521, https://doi.org/10.1029/2010WR009392, http://dx.doi.org/10.1029/2010WR009392, 2011. Zehe, E., Ehret, U., Blume, T., Kleidon, A., Scherer, U., and Westhoff, M.: A thermodynamic approach to link self-organization, preferential flow and rainfall-runoff behaviour, Hydrology And Earth System Sciences, 17, 4297-4322, 10.5194/hess-17-4297-2013, 2013.

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