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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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Major points: M1): 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 evapotranspiration 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?

RESPONSE: The entropy production term refers to the surface heat fluxes. We will better emphasize this point in the description of the model (section 3.1).

M2): 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).

RESPONSE: The definition of entropy as a flux times the driving potential difference divided by the absolute temperature refers to thermodynamic entropy. In Wang's MEP model use in the present study, entropy refers to Shannon entropy (see line 34, p.2), that is a quantitative measure of information. The MEP model is derived from the principle of maximum entropy (MaxEnt) developed by Jaynes (1957) as a general method to assign probability distribution in statistical mechanics (see line 34 p.2). This point is re-emphasized in the Methodology (see line 12, p.4). To avoid any confusion between these two definitions of entropy, we will clearly state in the Methodology that entropy does not refer to thermodynamic entropy.

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.

RESPONSE: Indeed, the closure of the energy budget is an important constraint to entropy production, but conservation of energy is a fundamental principle controlling surface heat fluxes. Other models of terrestrial evaporation are built around this constraint. For example, in the Penman model, evapotranspiration is constrained by the available energy (i.e. net radiation). The MEP model is based on the same fundamental principle and uses it to develop a predictive tool of the surface heat fluxes. We will add text to the methodology to reinforce the fact that the two models compared in the study, the MEP and Penman-Monteith models, are built around this constraint.

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.

RESPONSE: We agree that this would be a very interesting addition to the manuscript. We will add a section describing the performance of the models at the diurnal scale, as depicted in figure R1. HGS-MEP performed well in describing the diurnal pattern of variation in terrestrial evaporation, although we observed an overestimation of maximum values at mid-day. In addition to the figure, we will also add a table with performance metrics to quantify the ability of the models to capture the diurnal variation in terrestrial evaporation.

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?

RESPONSE: We agree with this point. At line 33 (p.1), we wanted to highlight the fact that hydrologic simulations are generally not much sensitive to the choice of ET model, to better stress out its large influence when performing hydrological projections (line 35). The alternative is to attempt to model hydrologic fluxes without relying on automatic calibration but instead using a priori estimation of parameters from soil and vegetation data (Wagener, 2007). This is in fact the approach implemented by the present study, as no automatic calibration was performed. We stress this point a few times in the discussion (line 2 and 9, p.17, line 44 p.18). We will add one of two sentences in the discussion to stress the fact that a priori estimation of parameter shows promising results in terms of the predictive ability of the model, thus offering an alternative to calibration.

The NSE and the RMSE are not independent, so the authors might consider to skip one of the metrics?

RESPONSE: Yes, we know that these metrics are not independent, but given that they are commonly reported, we would like to keep both. We will add a sentence in the Methodology to recognize the fact that the metrics are not independent.

Page 2 line 45: PM is also constraint by Rn.

RESPONSE: Yes, we will add a sentence to highlight this fact.

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.

RESPONSE: The equation to compute soil thermal inertia is empirically derived and was directly taken from Huang and Wang (2016), as cited p.5 (line 20). We will add a sentence to highlight the empirical nature of the equation. Still, as mentioned in section 3.3 and 3.4, the MEP model is not very sensitive to changes in soil thermal inertia. This parameter was in fact set as a constant equal to the dry soil thermal inertia. Accordingly, any modification to the formula used to compute soil thermal inertia would have little impact on the results of our simulations.

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.

RESPONSE: Field capacity has been defined based on soil matric potential (-0.033 MPa). This definition is common in hydrology (e.g. Dingman, 2002). We defined field capacity accordingly, as detailed on p.8, line 1.

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

RESPONSE: Ec corresponds to the wet canopy evaporation. Since, as stated on p.9 (line 8), interception was not considered in the present study, ðİŘÿðİŚŘ was set to zero.

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

RESPONSE: No calibration was performed for these parameters. We will add a sentence in section 3.4.2 to clarify this.

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

RESPONSE: Indeed, equation 21 assumes a neutral atmosphere. It has been used with success in other studies modelling terrestrial evaporation (e.g., Ershadi et al., 2014). Still, we will add a sentence to highlight the fact that the estimation of aerodynamic resistance is a source of uncertainty in the Penman-Monteith model.

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.

RESPONSE: We will double-check this number.

REFERENCES: Dingman SL. (2002) Physical hydrology. Waveland Press, Long Grove, 646 p. Ershadi et al. (2014) Multi-site evaluation of terrestrial evaporation models using FLUXNET data, Agricultural and Forest Meteorology, 187:46-61. Wagener (2007) Can we model the hydrological impacts of environmental change? Hydrological Processes, 21:3233-3236.

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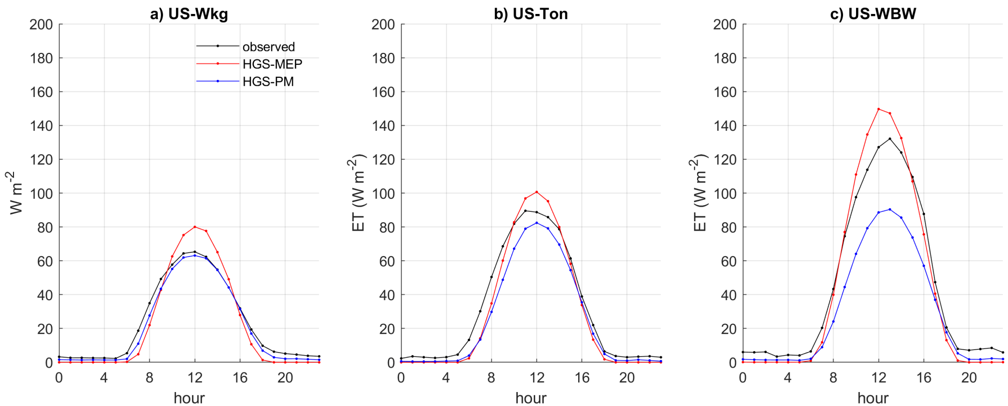

Figure R1. Hourly average of terrestrial evaporation calculated from observations (black) and modelled with HGS-MEP (red) and HGS-PM (blue).

Fig. 1.

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Discussion paper