HESSD

Interactive
comment

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

Audrey Maheu et al.

audrey.maheu@uqo.ca

Received and published: 6 May 2019

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.

RESPONSE: Before addressing the predictive ability of MEP under change, we believe the first step was to demonstrate the interest of this approach under relatively stationary conditions. Indeed, very little work has been done on the evaluation of the MEP model. The present study is indeed among the first to test the model under different climates and for multiple years, as Wang et al. (2011) had only presented short-term proof of concepts, mainly in a semi-arid climate. Issues raised by the reviewer are indeed important and we will address them in the discussion. We will raise the issue of non-independence in section 5.2 of the discussion to recognize this limitation to the MEP model. Moreover, we will modify the text and remove references to the parsimony of the model. Regarding the sensitivity of the model to change, we will raise this point in the conclusion and stress the need to address this important question in further work.

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

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.

RESPONSE: We will perform additional analyses. First, we will add a section describing the performance of the models at the diurnal scale, as per Reviewer 1 suggestion. Second, as suggested here, we will compare the performance of the model under energy vs. water-limiting conditions. To do so, we will compute the monthly aridity index (ratio between precipitation and potential evapotranspiration) and periods with a monthly index > 1 will be considered energy-limited and periods with a monthly index < 1 will be considered water-limited. We will finally compute performance metrics to compare the performance of the HGS-MEP and HGS-PM models for these two periods.

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.

RESPONSE: Regarding the rooting depth at US-Ton, we had performed tests and modelled terrestrial evaporation in stand-alone MEP mode, using soil water content observations as an input variable (Figure R1). Soil water content observations nearest

to the surface were used as input to the MEP-Ev model (z = 0 cm at US-Ton) and observations in the middle soil layer were used as input to the MEP-Tr model (z = 20 cm at US-Ton). While the HGS-MEP simulates soil moisture very well at a depth of 20 cm (Figure 5, p.14), it tends to underestimate soil moisture close to the surface, thus overestimating the water stress and limiting near surface water uptake and at the same time, transpiration. When using soil moisture observation, we avoid this situation and instead have soil conditions with greater water availability. As shown in Figure R1 below, access to a greater water supply did not improve the simulation of evapotranspiration. Instead of underestimation, we now face a large overestimation of terrestrial evaporation in the second half of the year. These results suggest that a greater rooting depth that would allow vegetation to tap deep water resources is not likely to improve the simulation of terrestrial evaporation at US-Ton. Instead, uncertainty relative to the definition of water stress points (wilting point and field capacity), as discussed on p.17 (line 4), may explain the underestimation of the terrestrial evaporation at this site.

Regarding the weighting of water uptake based on a cubic decay function, it is very common in hydrological or land surface models to weight vegetation water uptake based on the vertical root density (see for instance equation 4, Feddes et al., 2001). Using this approach, water uptake in a given soil layer depends on the root fraction in this particular layer. The parameterization of root water uptake is the subject of active research (see Clark et al., 2015 for a review) and while important, it is not the main focus of the present study.

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?

RESPONSE: The main objective of the study was to assess the predictive ability of the

MEP model and various benchmarks could have been used. We chose the Penman-Monteith model as it is a theoretically-sound model of terrestrial evaporation. In our experience, the MEP model has been met with a certain reluctance given its roots in information theory, thus our choice of the process-based Penman-Monteith model as a benchmark. We will add a few sentences in the discussion to point out the systematic bias observed with the Penman-Monteith model, as shown by Milly and Dunne (2016).

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

RESPONSE: We did not quantify the uncertainty associated with the lack of energy balance closure for the eddy flux data. We will add text to the discussion to raise this additional source of uncertainty. However, since we are mostly interested in a comparison between models, we can expect their performance to be similarly impacted by the lack of closure of the energy balance.

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?

RESPONSE: As suggested by Reviewer 1, we will add an analysis of the performance of the models at the diurnal scale. âĂČ Specific:

p4, lines 29-30. How are C_1 and C_2 "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.

RESPONSE: We will remove the term "universal" as it can be confusing. As for the von Kármán constant, it is involved in the calculation of the apparent thermal inertia of air given the latter is derived from an extremum solution of the Monin-Obukhov similarity equations.

C5

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

RESPONSE: The Bowen ratio, as predicted by the MEP model, agrees with the ratio derived with the Priestley-Taylor model, as demonstrated by Wang et al. (2011; Figure 1).

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).

RESPONSE: As stated above, it is very common in hydrological or land surface models to determine the depth of vegetation water uptake based on the vertical root density. Improving the parameterization of root water uptake was not the focus of the present study.

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.

RESPONSE: The longwave upwelling flux is measured above the canopy at US-Ton (z = 23 m) and US-WBW (z= 36.9 m), which we do not believe would offer a good proxy of the skin temperature when considering the soil surface.

REFERENCES: Clark et al. (2015) Improving the representation of hydrologic processes in EarthSystem Models, Water Resources Research, 51:5929-5956. Feddes et al. (2001) Modeling Root Water Uptake in Hydrological and Climate Models. Bulletin of the American Meteorological Society, 82(12):2797-2809.

—————————————————————————

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., https://doi.org/10.5194/hess-2018-636, 2019.
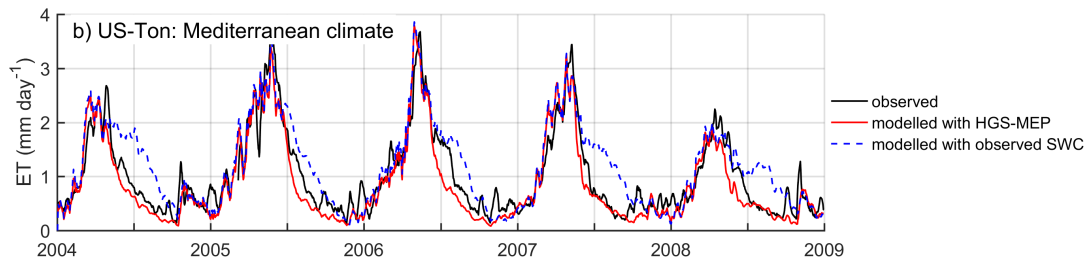
Figure R1. Comparison of observed evapotranspiration and evapotranspiration simulated by the HGS-MEP model and by the MEP-ET model using soil water content (SWC) observations at US-Ton

Fig. 1.