Entropy Production of Soil Hydrologic Processes and it Maximization, P. Porada, A. Kleidon, and S.J. Schymanski

This manuscript presents a framework for analysis of energy expended in the various phases of the hydrologic cycle in terms of entropy production, and the estimation of certain parameters via maximization of entropy for that particular process. Overall, the ideas put forward in this work make sense and it was very interesting to read. However, I do have a few questions and comments that would clear up some ambiguities that I note in the manuscript.

1. First and foremost are the related issues of the scale of the analysis and resolution of the model. The authors state on p 2 that "the aim of the present paper is to determine parameter values of a global land surface land surface model". However, nowhere in the paper do they give the resolution of the model. I believe that the resolution of most BATS models is on the order of 20 km. I bring this up because in validating the model, the authors show results for the "30 largest rivers in the world". I would think that the model resolution would be sufficient to resolve smaller watersheds than that. The reason I think this is important is because hydrologic processes can look a lot different depending on the domain scale and resolution of observation. The discussion of entropy production by the different processes on pp 4-5 would only be applicable at large scales and resolutions. For example, the result that surface runoff produces little entropy may be true for larger basins such as examined here, but would not necessarily be true at smaller scales. Also, interflow or lateral subsurface movement of water, a process ignored by the authors, can be dominant at some scales. So, I think a discussion of the importance of model scale and resolution is important to add context to the results. The authors should admit that their formulation of the hydrologic process is very simplified and that their conclusions for this formulation are only valid at the scale and resolution of this particular study.
2. Following up on point 1 above, it might be nice to have a table listing the 35 basins that form the basis of the results. That way the scale of these basins could be judged by the readers.
3. The second big issue that I have concerns the "conductivities", i.e., c_{root} and ., c_{base}. I am having a hard time understanding the physical meaning of these parameters. Based on equations 5 and 6, they seem to be playing the role of hydraulic conductivities and later on in the discussion on p. 4 the term "hydraulic conductivity" is used in a couple of places. However it is not clear that this refers to the parameters in equations 5 and 6. Now, I see that the quantities, u, are in units of kinetic energy (velocity squared or m^2/s^2) so that the units of c necessarily must be s/m in order for the units of q to be m/s. I assume that q represents mass flux per unit of soil area (m^2) so the units work out OK. But with these units I am not sure that the "conductivities" in equations 5 and 6 equate to what I understand hydraulic conductivity to be or not.

However, either way, these "conductivities" must be related to ordinary hydraulic conductivity. So, given the context of this study, I don't see why they are assumed to be constant. They are estimated via maximization of entropy (and indeed this is one of the salient points of the study) but are then assumed to be constant for the assumed soil matrix (sandy loam- a point that I will get to later). But of course, in the vadose zone, hydraulic conductivities are functions of soil moisture content just as is the matric potential. Now, are the conductivities assumed to be for saturated conditions so they would be constant? If so, then that should be stated in the manuscript and certainly constitutes another gross simplification in the method. And if they are not constant, then one can see that their estimation by maximum entropy would be greatly complicated.

4. The parameter of interest in equation 3 is relative soil moisture $\theta_{soil(z)}$, as it must be, however, the authors do not define it anywhere. Normally, relative soil moisture content is taken as $\Theta = \dfrac{\theta - \theta_r}{n - \theta_r}$ where θ_r is the residual soil moisture and n is porosity which would equate to maximum absolute soil moisture. Is this the definition the authors are using?
5. Why is the model run only for sandy loam soil conditions (p. 3)? Again, estimating the parameters for only one soil over the whole (global?) spatial domain and assuming the parameters are constant represents a gross simplification of reality. And again, if one did not make these assumptions, would the estimation of parameters by maximum entropy become so complex that the model would no longer be viable?
6. As a minor point, it is stated on p. 2 that the soil hydrologic model and the biophysical model are designed to run independently. I don't understand how that can be. Any soil moisture model must have an algorithm to compute plant water uptake and ET and any biophysical model must have a soil moisture accounting component. It seems to me that one model could not run without the other.
7. As another relatively minor point, I would suggest adding an arrow to figure 1 to represent surface infiltration. This is a key process to separate soil water processes from surface runoff.
8. It is not clear that from a practical standpoint, the proposed model represent an improvement over other current global hydrologic or BATS models. Can the authors supply some results for other models (maybe based on the same 35 largest basins) for comparison?

Let me reiterate that basically, I like this manuscript and find it very interesting and novel. It could very well represent an advancement in large scale hydrologic modeling as well as providing some insight into hydrologic processes and their relative importance in model formulation. I would just better understand what the authors are doing if the questions I have posed above could be answered.