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Hydrology and Earth System Sciences
Discussions

Interactive comment on "Energy states of soil water – a thermodynamic perspective on storage dynamics and the underlying controls" *by* Erwin Zehe et al.

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On behalf of all co-authors I sincerely thank reviewer 1 for her/his thorough, eloquent and helpful assessment of our work. Please find below our reply structured along the main headlines selected by the reviewer.

General considerations A):

- We tried indeed to keep the language simple; and the analogy to an outreach paper is maybe not totally wrong. Beside our scientific objectives, we seek indeed to introduce a wider part of the catchment hydrological community to indeed straightforward

thermodynamic / and energy based reasoning. While we admit that a sub group of hydrologists has a background in thermodynamics – maybe not always that strong as the one the reviewer obviously has – it is our experience that a thermodynamic perspective is not so straightforward to many of our fellow colleagues from catchment hydrology.

- The proposed analysis can indeed be carried out with the knowledge of an undergraduate course in thermodynamics. The header "theory" of section 2 of the manuscript might thus indeed be misleading. But it was by no means intended to claim that the approach is fundamentally new. We will rename section 2 as "theoretical background" in the revised manuscript. However, the fact that an analysis is based on straightforward thermodynamic grounds, and might thus be understandable for a wider range of readers, does not imply that the underlying science is not helpful. In fact we provide evidence that the proposed science is helpful to inter compare storage dynamics among systems in different geological and pedological settings in an informative and illuminating way.

- It is particularly new to visualize of capillarity and gravity of soil water dynamics in a single thermodynamic potential. This defines the possible range of system states, determines whether the system is in a state of a storage excess or deficit and is helpful to visualize which part of this state space is visited real world observations. In this context our statement on the gravity control is maybe misleading. We do not doubt that soil physicists and hydrologists are aware of gravity and potential energy, but the fact that this "is a linear control" is not so frequently discussed. The major focus is often on capillarity – while this is appropriate at small systems sizes like a soil core, gravity starts to dominate in case of a pronounced topography (as shown in our study).

- In this context I wonder which a) in consistencies the reviewer of the "theory section" the reader is actually referring to and b) which the missing references are. If we missed relevant literature in this respect, we would very grateful if the reviewer provided the missing references she/he has in mind. We will happily read those and include them, in case they are relevant (I come back to this point later on).

Perturbations B)

- We absolutely do not believe that the earth system is forced in a trivial manner, with perturbations from and relaxations back to a static equilibrium. And I do not think that we claimed that the earth system is that simple in our manuscript.

- We refer to perturbations exclusively associated with soil water dynamics in the unsaturated zone. And please note that in this context we do not talk about “thermodynamic optimality”. We refer to the paper of Savenije and Hrachwitz (2017) as they speculate about a storage optimum which balances recharge and release. But this is to show that this balance is a straight forward equilibrium. We will better stress this in the revised paper.

- Please note that the question whether the soil system operates linearly nor non-linearly depends on the fact whether gravity controlled potential energy dominates strongly against capillarity influences or not, because it is later which adds non-linearity. Potential energy may indeed dominate against capillary surface energy, as shown for the Colpach, and particularly if one moves upslope. I think the thermodynamic potentials we show make this very clear.

- The reviewer may forgive me, when I stick to the point that free of soil water shows indeed fluctuations around the local equilibrium – in fact we provide experimental evidence for this. Let’s think about an isolated soil column in contact with a ground water reservoir. Such a column will for sure relax to local equilibrium either by means of capillary rise (if it is too dry) or by seepage loss to the GW body. After that it will remain there, because it is a maximum entropy state! The related saturation profile depends on gravity and the retention curves (this is indeed textbook knowledge). We do of course not think that this storage equilibrium is static in a real world hydrological system. It will change with tectonic changes in topography, with changes in climate regimes (affecting ground levels, as mentioned in the paper) and also which ongoing weathering of the soil. This is for sure worth to be mentioned in the revised manuscript.

However, the timescales of these processes are much larger than the timescale of the typical inter-storm period as well as of the infiltration and seepage processes in the area of interest. Also ground water levels vary on much longer times-scales. Hence, free energy of soil water does really fluctuate around the local equilibria, forced either by evaporation loss and infiltration during recharge. Note that Figure 7 and Figure 8 provide clear experimental evidence for this, at least for the Wollefsbach. (We can provide also simulation evidence if wished). In case of the Colpach the system does indeed never reach its equilibrium, but this is clearly explained in the manuscript.

- The above mentioned issue of timescales needs to be properly mentioned in the revised manuscript. And yes we will stress that the perturbations are small (particularly in the Colpach), much smaller than the straightforward physical limit defined by saturation and the residual water content.

Energy currency C)

- It is absolutely right that the proposed characteristic is a thermodynamic potential, characterizing capillary and gravity control as function of time. We will stress this in the revised manuscript. To my knowledge it is however not true that this particular potential is frequently used in hydrology – I am not aware of a single study that uses such a combination. In contrary, usually soils are characteristics by their retention curves (which are by the way also a thermodynamic potential) and topographic maps but not in the way we proposed in this study.

- With respect to the selected name – it is nice to have a name for a baby, easier to reference as the "thermodynamic potential of unsaturated zone introduced in section 2 Eq. xxx". Also the soil water retention curve is by no means more than a thermodynamic potential, not for total free energy but for capillary surface energy density. Yet the baby has a name. Alternatively one could speak about the total potential function of the soil system. We do not care too much about which name might be more appropriate, but a name is certainly helpful.

- And yes any system can be characterized around free energy and thermodynamic potentials, this is why we selected the approach, we will stress this even more. Yet I am not aware about study that uses free energy of soil water for comparing storage dynamics in catchment systems. I am keen to read any study I missed in this respect, and to include it into the reference list, if it is relevant. And yes, energy and entropy and a thermodynamic perspective is used in many fields of geo-science including hydrology. This was never denied.

Caveated principle D)

- I agree that thermodynamic optimality is by no means an accepted principle which is universally valid as for instance the second law. On the contrary thermodynamic optimality is controversially discussed and it makes rather strong assumptions for instance like steady state conditions, and close to equilibrium conditions. I agree that both assumptions are problematic in hydrological systems, particularly when talking about the intermittent perturbations of the soil system. In this passage of the introduction we sure overdid it with our will to write this paper in a smooth and simple way. We will revise this passage to put much more emphasis on these points and thank very much for this hint.

- A periodic forcing does however not imply that no MEP configuration exists. On the contrary Westhoff et al. (2014) et al showed for a simple bucket model that periodic boundary conditions lead to an MEP configuration that largely differs from the one obtained using a steady state forcing.

- Last but not least, optimality principles are quite successful in physics for instance in classical mechanics the principle of minimum action – as the reviewer does for sure know. So the quest/search for a similar principle which is valid for dissipative systems is maybe not that astonishing. And the idea that the steady state configuration of open systems coincides with a configuration that maximizes entropy production is not too awkward - also from the Gaia perspective which was mentioned by the reviewer.

Caveated principles and quotes D)

- I agree that the holistic view/approach to terrestrial ecosystems and even the earth system as a whole is much older than the opinion paper of Savenije and Hrachowitz (2017).The hint to Gaia hypothesis is very well placed in this context. We will refer to this in the revised manuscript. The reason why we selected the quote of Aristotle in the context of the study of Savenije and Hrachowitz (2017) was not to "over-mystify" but in fact to argue that the proposed balance of release and recharge is a straightforward manifestation of very basic thermodynamic laws. It was by no means our intention to embarrass anybody with this opening statement. I agree that we do not address the question whether the whole is more than the sum of it's parts in our study. So the quote is maybe indeed misplaced and we might remove it. Self-organized systems might however be more than the sum of its part, as shown by Haken with his work on laser light, his later ideas on synergy and enslavement.

- Section 1.2 "Thermodynamic reasoning in hydrology" might indeed miss a few important references on the use of thermodynamics in environmental sciences. We referred to quite a few in this context, studies in this context, after stating that "thermodynamics in catchment hydrology gained substantial attention after the work of Kleidon and Schymanski (2008)". I think this claim is correct – but I also agree that we left out quite a bit of work of other groups such as of a) the group of Amilcare Porporato (e.g. characterizing the water cycle as idealized thermodynamic cycle), b) the group V.J. Singh and the work on entropy based concepts for soil water movements and c) or most of the work of Majid Hazanizadeh, Casey Miller and Bill Gray (we referred a little bit to their work when discussing the REW concept). This has been left out not to over emphasis our own importance but simply because it was not considered as too relevant here. Be assured that I have high respect particularly for the work of the latter scientists.

To conclude, we will carefully revise the study and particularly parts of the presentation as outlined above. Let me stress again that we would very grateful if the reviewer provided the missing references she/he has in mind. We will happily read those and include them, in case they are relevant.

Thank you very much,

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