Review of revised manuscript hessd-12-5123-2015

Stan Schymanski (stan.schymanski@env.ethz.ch)

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1 Summary

This revised manuscript does not have much in common with the original manuscript, offering substantial additional thoughts and theory, but also missing substantial points of the previous manuscript, as described below. Again, the paper is very thought-provoking and I hope that my comments will help to improve its utility and appeal for the scientific community.

The new manuscript provides a detailed theory for the calculation of thermodynamic forces and fluxes in different hydrological settings and points to potential for deducing macroscopic "effective" hydraulic conductances from microscopic simulations. The approach is very promising and may open the way to a range of new insights. Unfortunately, the author has removed any numerical simulations from the manuscript, so the new version remains entirely theoretical and hypothetical. I do not know if this was done willingly or if a section of the manuscript went missing, but I feel that some meaningful numerical analysis would be very beneficial for the manuscript and the readers, as it could be used to test some very fundamental and often implicit assumptions in physically based hydrological models. Without such analysis, I am afraid that further use of the paper would rely too much on a leap of faith that the approach is actually useful. I also found a few other shortcomings that should be considered or at least discussed.

2 General comments

The manuscript derives equations for the calculation of entropy production by water fluxes for a range of different idealised hillslope or catchment geometries and proposes to use these equations in order to deduce macroscopic conductances from microscopic analysis in a way that would result in consistent entropy balance across scales.

However, no test of the utility or even justification of the approach has been presented. For example, the different idealised configurations could be used to answer two questions:

1. Are the deduced macroscopic properties robust under different soil moisture distributions or forcing?

For example, such an analysis might reveal that the macroscopic conductance depends on heterogeneity in moisture, which would invalidate the approach. A numerical analysis along these lines could give valuable insights into general flaws in using effective macro-scale parameters in conjunction with equations derived for the micro-scale, e.g. Richards equation.

2. Does the tracking of entropy production and exchange across scales reveal inconsistencies in common physically based hydrological formulations?

   Anaysis of a model similar to that used in the previous version of the manuscript could reveal that entropy production and exchange at the large (macroscopic) scale does not match the sum of entropy production in all sub-domains, pointing to omissions in the original model or in the formulations used in the present manuscript.

I also noticed that the revised paper does not mention evaporation at all. This would have a large effect on both the mass and entropy balance of the system, and cannot be neglected under any relevant natural conditions I can think of. Therefore, I believe that root water uptake and evapo-transpiration should be included in the analysis or at least the author should provide discussion of how it could be included or why and under what conditions it can be left out.

In the specific comments below, I included thoughts that I had while reading the paper, some of which reflect the above comments and others relate to technical corrections or unclear formulations.

3 Specific comments

1. L13: “explicit calculation”
2. L44: “demonstrating how” (remove comma)
3. L45: This sentence is unclear. Do you mean how the exchange coefficient can be optimised to maximise entropy production?
4. L63: “a simple microscale Klausmeier model” sounds odd. “A simple distributed model of the water and carbon balance”?
5. L62–65: This sentence is convoluted. Please consider splitting it in two.
6. L68: may be
7. L84: The “entropy current per unit volume” should have the same dimensions as change in entropy density, but here they are given as [M T^{-3} K^{-1}], so I assume it is simply missing a L^{-1}. Also, consider using Θ for temperature instead of K, as K is an actual unit, not a dimension.
8. L89: I would refer to Eq. 5 for the definition of chemical potential and maybe mention here that it is equivalent to the sum af matrix and gravitational potentials, to make it more intuitive to hydrologists.

9. L90: The switch from macroscale to microscale here should be emphasised clearer, perhaps by an additional sub-heading? Also, it would be good to clarify that f is not a mechanical force (which would have dimensions of M L T^{-2}), but a thermodynamic force, having units of chemical potential, which is defined using units of energy per mass here.
10. L98: "true steady state" is a bit unclear. Why not: "...at steady state (assuming $s' = 0$)..."
11. L110: Thermodynamic force
12. L133: It would be good to point out that a good consistency test would be whether the sum of the sub-domains equals the entropy balance of the whole domain.
13. L140: Eq. 7 seems to be the wrong equation to get macroscopic fluxes from σ.
14. L161: What is meant by "true" effective conductance?
15. L238: Why $\overline{Q_s} = 0$? This made me realise that there is no evaporation mentioned in the paper. How would that affect the energy and entropy balances?
16. L242: It would be good to provide intermediate steps to allow the reader to follow the derivation of Eq. 35b.
17. L253–257: What is the benefit of calculating the macroscopic S' using microscale variables?
18. L279: Either the "and" is redundant or something is missing.
19. L300–302: Does the macroscopic conductance depend on heterogeneity in moisture, i.e. in the Richards Equation case? This would possibly invalidate the approach.
20. L325–236: This would have been a great thing to do as part of this paper.
21. L344: Should be $\overline{J^2}$ instead of $\overline{J}^2$.
22. L357–359: This is misleading, as 'thus' implies that all macroscopic variables that are functions of entropy production are "thermodynamically consistent". I think the point is that the macroscopic variables derived in this way promise a thermodynamically consistent link between different scales. However, it would be good to discuss what this actually means. Would we obtain different macroscopic properties for different forcing, even if the microscopic parameters were the same? Would we obtain different values for different scales? Is this approach only useful if we are able to obtain reliable microscopic parameterisations?