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Discussions

Interactive comment on "Transferring the concept of minimum energy expenditure from river networks to subsurface flow patterns" *by* S. Hergarten et al.

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This manuscript describes the application of "minimum energy expenditure" to subsurface flow processes. This is a very nice manuscript, it is well written and describes a novel and original approach of an optimization principle to subsurface flow and that should be published. My comments in the following concern mostly a need for some further explanations and some corrections in the argumentation.

Optimality principles: The statement in the abstract (and also in the remaining text) that "[optimality principles] still seem to be on a visionary level except for the theory of

minimum energy expenditure for river networks." is incorrect. There are plenty of applications of optimality theory that are predictive, including many in ecophysiology. In terms of energetic optimality principles, there has been, for instance, the work by Geoffrey West, Brian Enquist and James Brown on 3-D dendritic networks that is based on minimum dissipation (see series of Nature and Science papers in the end-1990s and 2000s, e.g., as a starting point, West, G. B., Brown, J. H., and Enquist, B. J.: A general model for the origin of allometric scaling laws in biology, Science, 276, 122–126, 1997), or maximum power (see e.g., A Kleidon, M Renner, and P Porada, Estimates of the climatological land surface energy and water balance derived from maximum convective power, Hydrol. Earth Syst. Sci., 18, 2201-2218, 2014) that have been rather successful and predictive. These applications are certainly beyond being visionary.

minimum energy expenditure: This approach is typically quite poorly described and justified. Which energy is expended on what? The authors do not provide a clear description either (in section 2). It is mentioned that "potential energy of the water is dissipated when it flows downslope in a channel", so do you mean minimum dissipation of potential energy? If it is minimum energy dissipation, then why not name it that way? See also the work by Enquist, West, Brown and coworkers who have shown how scaling laws can be accurately predicted by minimum dissipation and fractal geometry. In fact, I recommend that the authors look at this work, as it is mathematically and physically much clearer than the approach by Rinaldo and Rodriguez-Iturbe. I think the approaches are more or less the same, so that this manuscript is probably not affected. But the work by West et al. is just formulated more clearly.

Maximum Entropy Production (MEP): On page 5833 it is described that "it should be clearly stated that this is a conjecture that cannot be proven by the second law of thermodynamics in general". The further development of this approach to maximum power (as in Kleidon et al., 2014) shows that maximum power is a thermodynamic limit of dissipative systems, and not a conjecture. The conjecture is to assume that natural processes operate at their thermodynamic limit.

Maximization vs. minimization: also on page 5833 it is mentioned that MEP would be contradictory to the idea that flow patterns organize in such a way that flow is facilitated most efficiently. It is actually not a contradiction. The maximization of power of one process does not necessarily contradict the minimization of dissipation of another process, it can very well simply depend on how one looks at the process. For river networks, for instance, where the input of potential energy is more or less fixed by runoff generation at some elevation, the minimization of frictional dissipation can be associated with the maximization of the work done on sediment transport (see e.g. pertinent discussion on this on page 247, bottom right column in A Kleidon, E Zehe, U Ehret, U Scherer, Thermodynamics, maximum power, and the dynamics of preferential river flow structures on continents. Hydrol. Earth Syst. Sci., 17, 225-251, 2013). These can simply be two different sides of the same coin. It would be good to be precise on this topic to avoid unnecessary confusion, so this passage should be corrected.

Discussion: The manuscript misses a discussion section on the potential shortcomings and how these could be addressed. This should be included in the revision.

minor comments:

- page 5835: The way that the trivial solution of no slopes seems somewhat ad-hoc, and the way how this was avoided is not clear to me. What exactly was the constraint that was used here? That the sum of all slopes is fixed?
- page 5835, eqn. 3: It is unclear to me what S is: slope, as before?
- replace “side condition” by “constraint” throughout manuscript

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