

Responses to the referees' comments and description of the changes

Dear Markus,

thanks for the good news! We have prepared a revised version and would like to thank both referees for their help in improving the clarity of the indeed quite complicated theory presented in the manuscript. The points addressed by the reviewers are discussed in the following.

Referee 1 (M. Westhoff)

In this manuscript, the authors use the concept of minimum energy expenditure as an organizing principle to predict the optimal configuration of subsurface channel networks. The authors start with explaining this principle for river networks and subsequently extend this analysis for subsurface flow. The final equations that should be optimized are derived analytically and a short comparison with observations has been made. I consider this article as highly novel and interesting for HESS and advice publication after some (probably minor) revisions. These revisions should mainly consider a better explanation of all assumption made in the theory development and some extra steps in the mathematical derivation. Maybe the most important assumption made – and to my opinion not well enough justified – is the use of the Hagen-Poiseuille law (P5838, L22) which is restricted to laminar flow. I can imagine that flow through conduits in karstic systems is turbulent. This may be described by the Darcy-weisbach equation, but K is than not proportional to r^4 anymore and the whole derivation will be different. So the authors should justify that flow in the systems they look at are indeed laminar. Otherwise the complete theory collapses!

We have added a sketch of the theory for fully developed turbulent flow according to the Darcy Weisbach law and on the transition to turbulent flow (Sect. 5). In order to illustrate the difference between laminar and fully turbulent flow we have replaced the results for $n = \frac{3}{2}$ in the figures by those for $n = \frac{4}{3}$ as this value was found to be equivalent to turbulent flow according to the Darcy Weisbach law.

Below I will discuss other assumptions and missing steps in the derivation:

P5834, Eq.1: Explain that this equation describes the loss of potential energy, under the assumption that the change in kinetic energy is negligible. Maybe refer to Evans et al. (1998) who use this formula to describe the heat gained by frictional losses (which they derived in W/m^2): In fact minimum energy expenditure means minimum frictional losses. You can also make the link to the principle of maximum free energy dissipation (Zehe et al. 2013). This principle is a more overarching one stating that a system tries to move to thermodynamic equilibrium as fast as possible. For the system described in this manuscript, this means that the depletion of potential energy is as fast as possible: thus with minimal frictional losses.

We have added short explanations on this (lines 122–126 and 203–205). According to the suggestions of the second referee, we have rewritten the part of the introduction on the relationship of minimum energy dissipation to other principles of optimization, namely minimum and maximum entropy production (lines 50–97), so that the relevance of the principle of minimum energy dissipation should now be clear from the introduction.

P5835, L17: Add that this minimization is performed under the constraint that q equals the mean input by rainfall (minus evaporation).

Yes, but q_i itself is not the input by rainfall minus evaporation, but the discharge of a river segment. We have added a statement that all the q_i are known if precipitation minus evaporation is given (lines 171–173).

P5837, L6-8: I am not familiar with the Euler-Lagrange equation, but what I understand from Eq. 10 is that q should be maximized (since $q = K\nabla h$). But I don't understand the statement that follows on line 7 and 8.

It was indeed not very clear what this statement was good for. Apart from the relationship to finite element modeling, it should just show that it makes no difference whether we optimize K and h simultaneously or optimize K and consider Darcy's law in combination with the mass balance explicitly. An explanation on this has been added (lines 234–241).

P5839, L5: I could not follow how this formula was derived.

This was indeed a bit short. We hope that the new explanation and derivation (lines 286–295) is easier to follow.

P5843, L18: I came to a different formula: If $q(r)$ equals recharge in the circle between R and r , $q(r) = S\pi(R^2 - r^2)$, with S being the recharge.

Yes, but your result is the total flow across the surface of the cylinder which is the dominator of our formula. However, this flow must be divided by the surface of the cylinder in order to obtain the volumetric flow rate which gives the factor r in the denominator. We have added a show explanation on the dominator and the denominator (lines 408–410).

P5844, L4: I could not follow how this formula was derived.

This was indeed a bit short, and we have introduced some steps in between (line 417).

P5845, L21: I don't agree with the statement that "It is easily recognized that focusing flow reduces the total energy expenditure as long as $\gamma < 1$ ". It could be that I just don't understand it, and more explanation is needed, but a simple example shows that this statement does not seem to be correct: Consider the following 3 networks: . . . assuming that the additive discharge for each grid cell is 1 and the length between two grid cells is either 1 or the square root of 2. Using Eq. 4 along a whole range of γ results in the following graphs: Network 3 – which is the most dendritic network – uses less energy than network 1 for $\gamma > 0.16$ and it uses less energy than network 2 for $0.5 < \gamma < 3.36$. So either there is something wrong with this example or the statement is not correct.

Your argument is, of course, true. However, at that point we thought of focused flow on a lower level, namely tree-like structures compared to arbitrary graphs. The idea was just that splitting up the discharge of one site to more than one flow target is energetically unfavorable, justifying the conjecture that each site has a unique flow target. In this sense, all your three examples are focused flow, and we did not intend to find the best among them at this level. We have now added a more detailed reasoning why it makes sense to assume that each node has a unique flow direction (lines 430–461), so that the confusion should be resolved now.

Minor comments

P5834, Eq. 1: Use a capital L for length. At first sight I confused it with the divide sign (/).

This was just a matter of the typeface in HESSD. As the final article style of HESS uses a serif typeface, this should not be any problem in the final version even with the small *l*.

P5835, L4: change to: surface erosion is in equilibrium with a given, . . .

This phrase has been changed according to the rewriting suggested by the second referee (lines 143–166).

P5837, L18: leave the reference to Eq. 10 out: This equation does not describe the mass balance.

This paragraph has been rewritten (see comment on P5837, L6-8), so that it should be clearer now that Eq. 10 is the mass balance for an incompressible fluid under saturated conditions.

P5843, L9: The word “therefore” seems inappropriate here: the effective transmissivity should only increase if the gradient does not increase enough.

Yes, of course! We have added a statement that large hydraulic gradients are not found in the vicinity of karst springs (lines 394–396), so that the word “therefore” is correct now.

P5848, L6: refer to Eq 47 instead of 46?

It's basically the same as Eq. 47 just points out the proportionality. We have changed the reference (line 576).

Referee 2 (A. Kleidon)

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.

We were indeed not aware of the large number of recent publications in this field, and in particular not of these very interesting publications in ecophysiology. We have now rewritten this part of the introduction (lines 50–106) and adjusted the expressions accordingly at some other positions, including the abstract (lines 1–4).

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.

We agree that it is useful to explain this more precisely. We have added explanations for river networks (lines 115–121) and for subsurface flow (lines 203–207) and replaced “expenditure” with “dissipation” throughout the text as suggested.

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.

The rewritten part of the introduction (lines 50–106) should now express this result correctly, and we have adjusted the arguments at some other positions in the manuscript accordingly (lines 186–190 and 799–805).

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.

This aspect should also be explained correctly in the rewritten part of the introduction (lines 50–106) now.

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

We have now added a section “limitations” (Sect. 6) addressing the two presumably most important limitations of the approach.

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?

We have rewritten this part (lines 143–166), so that it should be clearer now that the constraint is an equilibrium between erosion and a given uplift rate.

page 5835, eqn. 3: It is unclear to me what S is: slope, as before?

We have added the meaning of S and q explicitly now (lines 147–148).

replace “side condition” by “constraint” throughout manuscript

Done (lines 10, 143, 146, 246, 657, and 773).

Best regards,

Stefan