Hydrol. Earth Syst. Sci. Discuss., 9, C2654–C2659, 2012
www.hydrol-earth-syst-sci-discuss.net/9/C2654/2012/

Hydrology and Earth System Sciences Discussions

Interactive comment on "Thermodynamics, maximum power, and the dynamics of preferential river flow structures on continents" *by* A. Kleidon et al.

HHG Savenije (Referee)

h.h.g.savenije@tudelft.nl

Received and published: 5 July 2012

Review of "Thermodynamics, maximum power of river systems" by Kleidon et al.

I very much enjoyed reading this fascinating paper, which may become a benchmark paper in trying to understand the geomorphology of drainage systems. The beauty of the paper is that by focusing on optimal sediment export it brings together different approaches on "minimum stream power" in the river system and "maximum entropy production" to reduce the topographic gradient. In the authors approach these seemingly opposing concepts are part of a larger whole that aims at optimizing the sediment

output. The paper is lengthy, but this is necessary. I am happy that the authors build up the argument from first principles and take the reader by the hand.

This is the first paper that looks at the continental formation processes, sediment export by erosion and transport, and the efficiency of the process of continental formation and depletion as an integrated system that maximizes the dynamics and maximises dissipation. In doing so they solve one of the most fundamental riddles in landscape formation, and they explain why 'minimum stream power' in the river channel network is part of maximizing the power of the system as a whole.

Although I think the paper provides an enormous leap forward in our understanding of the thermodynamics of landscape formation and the structure of river basins, there is still something missing. It does not tell us why the majority of the water travels through the sub-surface, where it helps to erode the subsurface by chemical processes, where it allows ecosystems to live on the substrate, and where it dissipates the potential energy very regularly and equally distributed along the pathway. In fact, the subsurface processes of chemical erosion are required to break-up the rock into finer particles that can be eroded by surface runoff described in this paper. Hence the subsurface processes should be part of the overall analysis (but this can be done in a follow-up paper).

Also, I am still curious to understand why groundwater flow in river basins can be so well described by a simple linear reservoir. This paper concentrates on surface runoff only (indeed responsible for landscape formation), but it neglects the subsurface processes, where also a substantial amount of potential energy is dissipated. Groundwater flow is probably similar to the sapflow in leafs (although opposed in the direction of flow). It is not by accident that a catchment is often compared to a leaf. The development of the drainage structure in a groundwater system and a leaf are not driven by erosion, but the result is an efficient structure to transport water and to dissipate a gradient.

An interesting thought may be that in arid climates the runoff is primarily through surface runoff, whereas in wet climates the runoff is predominantly through sub-surface processes. As a result, the examples presented in this paper are particularly relevant for arid climates, where we can indeed see these structures being developed after torrential rains.

I hope the authors see these observations as a recommendation for further research, in line with their observation on P7360, L20, and the closing remarks on the last page.

I did, however, encounter some mistakes and some issues that require further explanation, but overall I am very happy with the paper and would like to see this published in HESS.

I think that the derivation of Eq (4) is unclear. There does not seem to be a match between Eq (1), dW/dt=P= $J_{h,in}$-$J_{h,out}$, Eq. (2), and dS=dQ/T. There are some steps missing in this derivation and it is not clear if the last equation should be interpreted as: dS/dt=1/T dQ/dt or dS/dt= d(Q/T)/dt. I suggest this is explained either in the discussion, or in the paper itself. Since it is not essential to the argument of the paper, maybe it is best to make it part of the discussion in HESSD.

One mistake is probably due to my own incorrect suggestion during some earlier discussion. On page 7335, it is suggested that the case of low drag (N_d«1) corresponds with Chezy flow and that the case of large Drag (N_d»1) would correspond with Darcy flow. This is not correct. In fact one can show that the case of large drag corresponds with Chezy flow, while the case of low drag corresponds with supercritical flow (since v^2=$g\Delta z$ while critical flow occurs when v^2>gh and Δz is much larger than the depth of flow). That the case of high drag corresponds with Chezy can be simply seen by substituting in (24):

$$J_{w,in} = \rho Q = \rho v B h$$

$$F_{w,d} = \rho g B L \frac{v^2}{C^2}$$

where Q is the river discharge, B is the stream width, h is the stream depth, and C is Chezy's coefficient. Substitution yields:

$$v^2 = C^2 h \frac{\Delta z}{L}$$

which is Chezy's equation for channel flow. This realisation bears on several statements later in the paper that need to be revised, in particular: P7344, L8; P7358, L15.

By the way, if $F_{w,d}$ is considered a linear relation of the flow velocity (linear friction) then we find that the flow velocity is indeed directly proportional to the slope, which is Darcy's equation. So the case of v being proportional to $\Delta\phi$ can imply both the Darcy and the Chezy equation depending on the assumption of linear or quadratic friction (laminar or turbulent flow).

Regarding the conclusion that channels tend to have uniform slope (P7358, L24), this is not true. What they tend to have is a uniform sediment transport capacity, which is proportional to the velocity at a power of 2.5. For uniform sediment this would lead to a constant velocity, but coarser sediments require higher velocities and hence larger slopes. As a result there is a gradual decrease in slope and an associated decrease in sediment size as we move downstream.

Finally, there are a list of minor mistakes and small corrections that I indicate below:

P7319, L24: replace by yields

P7324: It is better not to use multi-symbol variables. Why not use instead of NEE the expression

$$\sum J_S$$

for the sum of all the entropy exports. This would be consistent with the rest of the definitions.

P7331: Similarly I don't like the use of PE and KE in eqs (11)-(14). Why not use (consistent with the rest of the paper) $m_w\phi_w$, $m_s\phi_s$, p_wv_w, p_sv_s. This also applies to the equations and text of Section 5.2 and 6.1

P7331, L23: replace 'power' by 'kinetic energy'

P7334: put brackets in the second term of (20)

P7335, L5: this is wrong. There should be brackets around ($2N_d$)

P7335, L10-11: is consistent with supercritical flow in a low friction channel, while Eq.(24) is consistent with the flow in a frictional channel (i.e. Chezy flow).

P7339, L8: I don't understand the word quadratic here

P7339, L9: remove "at the top"

P7339, L11: remove 'of'

P7339, L20: There are again brackets missing. It should read L/(4N). This leads to the erroneous observation in the next line that N=0, whereas N should become infinitely large.

P7340, L12: replace 'as' by 'with'

P7340: I think in Eq.(43) there is a mistake. It should be $(2\pi)^{(1/3)}$. But maybe I made a mistake. Subsequently the reasoning below these equations is wrong. Because J also depends on the density, the density has no effect. In fact the water inflow equals:

$$J_{w,in} = \rho i L^2$$

where i is the effective precipitation intensity. It then follows that:

$$N_{opt}^3 = \frac{1}{8\pi}\frac{v}{i}$$

Hence the drainage density depends on the ratio of flow velocity to rainfall intensity. If there is no effective rainfall, then there is no flow velocity and the equation is not

determined. I don't know what the asymptotic ratio is. That would be nice to find out. What we can conclude is that the stream velocity is something that does not vary too much from river system to river system (about 1 m/s). Hence a high rainfall intensity leads to a low channel density and a low rainfall to a high channel density. I don't know if there is empirical proof for this.

P7345, L7: I think this is not true. The above calculation shows that N_{opt} is size independent. L^2 drops out. One can say that large rainfall amounts (with a limited velocity of flow), hence wetter climates lead to larger and fewer channels. Please check if this is true.

P7347, L22: I suggest to write 'Taylor approximations'

P7354, L9: 'dominant'

P7359, L9: 'pursue'

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 9, 7317, 2012.