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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.

Anonymous Referee #1

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General Comments

The tendency of river channel networksâĂŤand other flow networksâĂŤto form similar patterns and forms in a staggering variety of situations has long driven a search for (ideally) a single underlying principle that explains this tendency. This paper continues in that tradition, this time basing the argument on thermodynamic principles and maximization of power. The fundamental issue is that (both in this paper and in general) no one has offered a satisfactory explanation of why fluvial (or other systems) should operate so as to maximize, minimize, optimize, or equalize any particular matter or energy

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fluxes. The same observations regarding structures can be explained on the basis of emergent phenomena associated with a simple principle that more efficient flow structures persist, while less efficient ones tend to be abandoned. Thus concentrated flows and channels are preferred to diffuse flows, and dendritic networks (where geological structures allow) are preferred to other configurations. While the thermodynamic arguments are consistent with this idea, they are not necessary to it. Since such gradient selection (see, e.g., Phillips, J.D. 2011. Emergence and pseudo-equilibrium in geomorphology. Geomorphology 132: 319-326) is all that is needed to explain the observed features of fluvial systems of interest here (which, incidentally, are not as ubiquitous as is often assumed), one wonders about the need for more complex explanations relying on assumed goal functions.

This paper does an excellent job of relating basic thermodynamic principles to fluvial systems, and this represents a major advance from the thermodynamic analog approaches common in the 1960s and 1970s. The explicit consideration of the relationship between force applied to sediment transport, frictional dissipation, and velocity is quite interestingâĂŤhowever, I would have to yield to someone more up-to-speed on mechanics of sediment transport to assess the novelty of this contribution. The establishment of some theoretical limitations on power expenditure is also quite useful. The major contribution, in my view, is a detailed explication and justification for the feedbacks described in figure 9, which is useful and valid independently of the maximum power hypothesis.

While the discussion acknowledges the fact that steady-state is not always found in real landscapes (I would say it is quite rare), the arguments and models here seem to rely quite heavily on this assumption. To what extent are the conclusions robust to relaxing this assumption?

Finally, there are a number of situations where the results and implications would benefit from comparisons with previous work, generally based on physical principles, which seems to lead to similar conclusions. These are often based on the same or similar

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steady-state assumptions, but without hypotheses of goal functions (see, e.g., Smith, T.R., et al., 2000. Transient attractors: towards a theory of the graded stream for alluvial and bedrock channels. Computers and Geosciences 26: 541-580.). The debate over extremal hypotheses in hydraulic geometry could also inform this work.

Specific Comments

7319:6-18: reference should be made here to the work of Mike Woldenberg, who worked out as early as 1967 the principles rediscovered by the fractal and optimal channel network research decades later (this synthesizes some of that work: Woldenberg, M.J., 1969. Spatial order in fluvial systems: Horton's laws derived from mixed hexagonal hierarchies of drainage basin areas. Geol. Soc. Am. Bull. 80: 97-112). Some of Mike Kirkby's late 1960s/early 1970s work reached similar conclusions.

7321:25- : In the context of the remainder of the paper, these statements seem to suggest that the rate at which material is redistributed matters somehow to the planet, and that some advantage is to be gained from acceleration. Just because fluvial (as well as glacial and other processes) do speed up this transfer does not imply that this is somehow a goal function.

7322 et seq. (section 2): These principles are very well known to most HESS readers, and are in any case reviewed in many textbooks, and in some of the authors' previous publications. I recommend greatly condensing this section to perhaps a highlighting of the key points relevant to the work at hand.

7327: 1-9: It may be worth distinguishing this direct thermodynamic consideration from the thermodynamic analog approaches applied to drainage basins from the early 1960s forward.

7327: 22-25; sections 3.2, 3.3: This is a very incomplete view of the energetics involved, since far more energy is required to turn rock into transportable "loose particles" than to move this material downhill. The solar (including biological) energy inputs far exceed the potential-to-kinetic conversions. The statement as written also implies no significant role for bedload or dissolved transport, or debris flows.

7328: 1-16: This should express sediment output as the net outflux, or indicate this this applies only to denudational systems.

7328:25-7329:4: As these steady-states are often not approximated in real landscapes, the authors should acknowledge them as a reference condition and/or computational expedient rather than as normative conditions.

7332: 13-14: Sentence unclear.

7333:8: derivatives?

7335:3-7: Eq. (23), (24) could have been straightforwardly derived from standard flow resistance equations, as is implicitly acknowledged further down.

7335:23: This is standard stream power theory, and was worked out at least as far back as 1966 by Bagnold.

7338:11-20: This is an important point, but could have been arrived at much more efficiently.

7341:1: Of course, drainage density is also strongly influenced by substrate resistance and the stage or state of development of the drainage network.

7341:21-26: This cannot be true unless total rock uplift exceeds isostatic compensation, as the relative densities of the crust and mantle dictate that there will only be about 0.8 m of uplift for every 1 m of crustal denudation.

Section 4.3: Eq. (46) doesn't make sense to me, and thus subsequent developments are hard to follow, since 7342:14 implies Jo = 0. This needs clarification. There is also a significant body of work on height limits of mountain ranges and maximum uplift rates that is not referenced.

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7343:12-13: As phrased, this sounds teleological. Alternative: Maximum power occurs when the sediment transport rate is proportional to Jo/2.

7346: 12-19: You may also want to make clear you are referring to landscape or topographic structure, not structure as typically envisaged/defined by geologists.

7349: 5- : Nomenclature is becoming problematic here, as it is implied that the term "structure" applies only to some parts of the landscape and not others. This is intuitively confusing, as to most of us a flat plain (e.g.) has a structure, just as much as (e.g.) an incised channel.

7349:15-16: Unclear, is it implies that slopes less than the mean slope would also export more sediment.

Section 5.3: Several commentsâĂŤfirst, the term "disequilibrium" conflicts with the way the term is often used by geomorphologists & hydrologists. This use is no worse (or better) than the various and sundry other ways the term is used, but there is a potential for confusion. Second, the sequence of stages of fig. 7 corresponds qualitatively with a great many landscape and channel evolution models. Is this a case of model equifinality? Are those models effectively capturing the dynamics described here? Or is this paper effectively a formalization & codification of phenomena already known & incorporated in those models? Third, there is again an implicit assumption of topographic equilibrium as a goal function, which has never (here or elsewhere) been satisfactorily demonstrated. Finally, there are a great many denudation/elevation/slope/uplift feedbacks that are not considered here.

Section 6.2: This and figure 9 are a good summary of the arguments.

7356:25 – 7357:19: The numerous simplifications acknowledged do not seem consistent with the claims of completeness.

7358: 28-29: Yes, but the cited paper shows that the development of channels and networks does not require any assumptions regarding maximization or optimization of energy, power, or anything else.

7360: 13-24: I suggest looking at subsequent work on biological energetics, such as Rasmussen & colleagues work (some in this journal) on energetics of the critical zones, and Phillips on biological energy in landscape evolution. I do not think the model in this paper at all constrains the limits of biological energy contributions (or, for that matter, other solar-driven contributions).

7360: 27: I applaud the authors' use of the term "tendency," rather than claiming law status for this principle.

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