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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.

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We thank the reviewer for his thoughtful and constructive comments. In the following, we respond to each of the reviewer's points. The individual points are taken from the review and listed in the following in *italic*, with our response following in plain text.

comment 1: *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 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.

The reviewer's point is that it would seem somewhat arbitrary which aspect of a system is being maximized, minimized or alike. This is exactly where thermodynamics provides clarity. All Earth system processes have a thermodynamic foundation and are coupled, and the direction formulated by the second law is as fundamental in physics as one can get. Even though entropy is not used much explicitly in most of the paper, there is a direct connection between conversions of energy that do not involve heat (such as the potential and kinetic energies dealt with in the manuscript), and entropy. When we take the energy of a system composed of heat Q, and non-thermal forms (kinetic energy and potential energy), A, then in an isolated system the dynamics are such as to minimize the non-thermal forms, thereby maximizing Q and, hence, the entropy within the system. This direction towards maximum entropy can be called a "goal function", but it can also be justified on statistical grounds, as it is done in statistical physics. We were not explicit about this connection in the manuscript and so we have added text in section 3 to clarify this connection between the minimization of kinetic and potential energy with the maximization of entropy.

Regarding the reviewer's comments that structures such as river networks can also be explained by other arguments, we would point out that a thermodynamics basis provides likely the most basic and quantitative basis to formulate organizational principles and thus allow us to test and apply such principles to the widest range of phenomena. Hence, we should seek to formulate general organizing principles in thermodynamic terms, even though this does not necessarily contradict other ways to explain organization that are not based in thermodynamic terms. We have added text in the discussion section where we discuss the relationship between our approach and other approaches to point this out.

comment 2: *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 interesting; however, I would have to yield to someone more up-to-speed on mechanics of sediment transport to assess the novelty of this contribution.*

We think this aspect was addressed by the other reviews, particularly of K Paik and GE Tucker.

comment 3: *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?*

The steady-state assumption is very common in geomorphological models, and is well justified. While the changes are certainly not exactly zero, they should nevertheless be quite small. If this were not the case, we would notice substantial and fast changes in river flow and topography, and accumulation of water in rivers and increases in topography through time (or the reverse). Hence, we can usually assume that the changes are relatively small, even though there may be locations and periods where this may not necessarily be the case. Neglecting these changes makes the treatment substantially easier as we do not need to deal with temporal changes in the equations. We have added references to the steady-state assumption in section 4.3 (to also address

the point raised in the review of K Paik.)

comment 4: *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 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.*

There are certainly alternative explanations, based on conservation laws and stability analyses (that were also pointed out by the comment by GE Tucker). We think that it is critical to point out that such analyses do not specifically treat energy conversions in the context of the second law, and while they can explain the structures, they cannot explain these structures as being the consequence of the second law. We added some discussion on this in the introduction (now subsection 1.1) but kept it rather brief since the manuscript is already quite long.

comment 5: *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.*

We added the reference to the revised manuscript.

comment 6: *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.*

The paper indeed shows that fluvial processes act in a particular direction and this could be interpreted as a goal, but this goal of thermodynamic equilibrium corresponds to what would be expected from how thermodynamic systems function. We often do not think of Earth system processes in the context of the second law, the associated energy, and the work involved to maintain these processes, but this does not mean that there is no such thermodynamic foundation for these processes. In this manuscript, we attempt to demonstrate this.

Following the comment by GE Tucker, we formulated this discussion of continental crust in a more analytical way.

comment 7: *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.*

The other reviewers did not think that section 2 was not needed. Since we have now introduced a brief section on thermodynamic equilibrium as it applies to the river network system (new section 3.2), we think that this very brief overview of thermodynamics and its relevance is needed to remain a part of the paper.

comment 8: *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.*

Thanks, that's a very valid point. We included this point by reformulating the beginning of section 3.

comment 9: *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.

To keep the focus in this manuscript, we neglect these processes not because they are not important, but because river flow structures are mostly formed out of the redistribution of sediments. Hence, a minimal description of the system needs to at least account for the potential and kinetic energy of water and sediments. We added some clarification on this at the end of section 3.1.

comment 10: *7328: 1-16: This should express sediment output as the net outflux, or indicate this this applies only to denudational systems.*

We have clarified this in the text.

comment 11: *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.*

We have clarified this in the text.

comment 12: *7332: 13-14: Sentence unclear.*

We removed this sentence as it was not necessary.

comment 13: *7333:8: derivatives?*

We have clarified this in the text.

comment 14: *7335:3-7: Eq. (23), (24) could have been straightforwardly derived from standard flow resistance equations, as is implicitly acknowledged further down.*

We do not claim that this is a new result. Our derivation is nevertheless needed and important because it shows that these expressions follow directly from the mass and momentum balance constraints when evaluated at different limits that are further considered in the following.

comment 15: *7335:23: This is standard stream power theory, and was worked out at* *least as far back as 1966 by Bagnold.*

We added the connection to stream power and the reference in the text.

comment 16: *7338:11-20: This is an important point, but could have been arrived at much more efficiently.*

We think it is necessary to show the complete derivation to arrive at these contrasting limits as the different limiting cases can explain why structures can change system behavior so that it is energetically more efficient. Since the other reviewers did not mention that this derivation is lengthly, we maintained the text as it is.

comment 17: *7341:1: Of course, drainage density is also strongly influenced by substrate resistance and the stage or state of development of the drainage network.*

We agree that other factors play a role for the drainage density that is being observed as well. Since this paragraph deals with the interpretation of the simple model, we added a brief statement about the other factors.

comment 18: *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.*

We added a comment to this assumption and state that this is a very common assumption in landscape evolution models (see also review by K Paik).

comment 19: *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.*

This expression directly follows from the physical expression for the buoyancy flux. Furthermore, this expression does not imply $J_0 = 0$. It implies that $J_{s,in} = J_0$ when $\Delta\phi = 0$. We added some text to clarify this. Since this is a pretty straightforward

relationship based on buoyancy, we do not think that it is necessary to cite literature on height limits of mountain ranges.

comment 20: *7343:12-13: As phrased, this sounds teleological. Alternative: Maximum power occurs when the sediment transport rate is proportional to Jo/2.*

We changed the text to the reviewer's suggestion.

comment 20: *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.*

We made our use of the term structure more explicit in the text.

comment 21: *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.*

We have added some text to help clarify the terminology.

comment 22: *7349:15-16: Unclear, is it implies that slopes less than the mean slope would also export more sediment.*

Yes, that is correct. The combination of steeper slopes and less steep slopes produce greater sediment export than a uniform slope. This is shown mathematically in section 5.1. We added text to clarify that the structure consists of the "combination of steeper hillslopes and less steep channels").

comment 23: *Section5.3: Several comments:*

comment 23a: *first, the term "disequilibrium" conflicts with the way the term is often used by geomorphologists and 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.*

We think that we have very clearly defined the term "disequilibrium" in section 5.1 specifically by eqn. 53, and refer to the defined symbol $D\phi$, so that it should be clear what we mean by "disequilibrium" within the same section.

comment 23b: *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?*

The different steps shown in Fig. 7 are hypothetical and qualitative, but can be related to the insights gained from the three models of the previous section. It is likely that landscape evolution models capture these dynamics to some extent. In a way, it states a certain equifinality in the sense that such a structure develops as a faster way to deplete the driving gradient, but the particular shape of the structure can depend on the initial perturbation and other random events. We have added text to clarify the purpose of Fig. 7 in section 5.3.

comment 23c: *Third, there is again an implicit assumption of topographic equilibrium as a goal function, which has never (here or elsewhere) been satisfactorily demonstrated.*

Yes, there is an assumption of a goal. As explained above, thermodynamics has a very clear goal of maximum entropy. Section 2 relates this goal to the dynamics of minimizing potential and kinetic energy (that is also related to in the Appendix regarding the dynamics of continental crust cycling). This thermodynamic goal is extremely well established in physics.

comment 23d: *Finally, there are a great many denudation/elevation/slope/uplift feedbacks that are not considered here.*

Yes, we agree, and we mention this in the discussion. But the point is that the dynam-

ics towards structure evolution can be understood simply on the basis of the models of section 4 without a great number of additional feedbacks. This minimum set of ingredients hence suffice to explain why such structures should occur and evolve and represent the means of the system towards greater gradient depletion.

comment 24: *Section 6.2: This and figure 9 are a good summary of the arguments.*

Thanks.

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

We clarified the term "completeness" in the text by writing: "This two-way interaction of a driver causing a flux, but the flux depleting the driver, is what we mean by a “complete" view of the dynamics of river networks (even though we omitted many aspects in this study)."

comment 25: *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.*

We added text to clarify this point by saying "although Phillips (2010) did not associate these feedbacks with optimality, we place such kind of feedbacks into the broader context of thermodynamic directions".

comment 25: *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).*

We want to keep the focus here specifically on potential effect of vegetation on river channel networks, but not on the general role of vegetation on land. The work by Rasmussen focuses on pedons, not river channel networks. We added the reference to Phillips (2009) and text to clarify the aspect we wanted to mention.

comment 25: *7360: 27: I applaud the authors' use of the term "tendency," rather than claiming law status for this principle.*

Good, thanks!

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