

## ***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 Greg Tucker 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:** *There is a rich literature on the related topic of channel and drainage network initiation, much of which takes a more detailed, process-oriented viewpoint (and, often, says little or nothing about energy conservation, emphasizing instead conservation of mass and momentum). It would strengthen the paper if the authors could*

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*connect their ideas with some of this literature. I am thinking here of the classic paper of Smith and Bretherton (1972), and subsequent papers that have pointed toward instabilities in the equations of flow, sediment transport, and/or erosion (e.g., Izumi and Parker, 2000, and later work by T R Smith and colleagues, such as Birnir et al. circa 2001, and a more recent solo paper by Smith in JGR Earth Surface).*

Thanks for pointing this out. We added a paragraph in the discussion where we describe that the fast, positive feedback that we describe in the paper is likely to be closely related to the instabilities identified in these studies. We also mention these studies briefly in the introduction.

**comment 2:** *7327, 20-25: The assumption that the continents are already made of 'sediment' carries with it the assumption that the limiting factor is transport rather than mechanical/chemical wear of the rocks. The authors are very clear about this assumption. Nonetheless, it would be interesting to discuss whether this is merely a detail, or something fundamental. Suppose we live in a world where far more energy is required to fragment a rock than to transport the pieces. What would the implications be for the theory?*

Excellent point. When we frame this setting of a rocky surface into the broader context of the dynamics to deplete gradients in continental topography, then the weathering of rocks would likely be the limiting process in this depletion, and not fluvial transport. The weathering of rock also involves energy, so that such an energy-based approach would also seem to be very suitable, but would need to consider different processes. We added some text in the first paragraph of the discussion to discuss this further (also to address comments by the other reviewer).

**comment 3:** *Momentum conservation: it is interesting that the authors chose to include a threshold for imparting momentum to sediment by water, while ignoring other potential nonlinear effects (one could, for example, view hydrologic processes such as interception and infiltration as imparting a threshold on water mass flux). It would*

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*be helpful if they could comment on the motivation for the inclusion of such a 'detail' (around page 7328). What motivates this? It raises the more general question of what aspects of reality are mere details and what are critical components that, were they missing, would lead to a very different outcome from what we observe.*

That's a very good point. We included it in the model because it is very commonly included in parameterizations of sediment transport (including the work by Andrea Rinaldo and Ignacio Rodriguez-Iturbe on fractal river networks). But the reviewer is very right to question the need to include this detail. In fact, to understand the evolution of structure as a result of strong interactions, this threshold only plays a role in setting the magnitude of sediment transport, but not the conditions under which structures should form. We added this important point in the discussion section.

**comment 4:** 7319, 24-26: *this statement – that maximization and minimization principles are consistent rather than contradictory – is not really explained or demonstrated in this section. It is not clear therefore whether it is simply being asserted (without obvious support or explanation) or is a concept that the authors intend to explain and demonstrate later in the paper. If the former, then more explanation is needed here. If the latter, then the text should signal to the reader that all will be made clear later.*

Agreed. We added text in this section, saying that "This maximization is also related to minimization. When frictional dissipation of a moving fluid is minimized, its ability to transport matter along a gradient is maximized. This aspect will be shown explicitly in this manuscript for the case of river networks as well as their surrounding hillslopes."

**comment 5:** 7321, 10 and following: *this discussion of crustal dynamics is not totally clear. In fact, it probably could be made simpler and more straightforward. One suggestion is to remove Fig 1a and the discussion of what happens to potential energy when an imaginary low-density block rises into isostatic equilibrium. Geodynamicists could take exception to this simplistic view of continental crust production. Why not simply start with the local isostatic equilibrium in Fig 1b? This would also solve a related prob-*

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*lem, which is that in referring to oceanic crust subsidence, it is not immediately clear whether the authors mean thermal subsidence, or simply subsidence due to the flow of material that fills beneath the rising continental block. (I think it is the latter, but one is so used to thinking of thermal subsidence that this is ripe for confusion) Further, because the reference to gain in potential energy precedes Fig 1b, readers may wonder where this gain comes from (after reading several times, it seems to mean gain from isostatic uplift of the continental block). So: overall this transition from 1a to 1b holds potential for lots of confusion, and does not seem to add much to the concept being presented here.*

Agreed. We changed Figure 1 according to the suggestion by the reviewer.

**comment 6:** 7321, 20-23 *the notion that the lowest-PE state is Fig 1d makes intuitive sense, but would be even more convincing if you could show it mathematically (briefly). Also, this would be a good place to define carefully what you mean by isostatic equilibrium. Many readers (or at least this one) would say that Figure 1b is in 'isostatic equilibrium', and by this they would mean 'local isostatic equilibrium.' I think that the point the authors are trying to make here is that local isostatic equilibrium among continents and oceans is nonetheless a state of \*disequilibrium\* because it maintains a potential energy gradient. Definitions are really important here, because of the different ways in which the equilibrium concept is used and applied. Or, alternatively, maybe they are noting that, when a continent undergoes erosion, there will be some disequilibrium that drives isostatic uplift – in other words, that isostatic uplift (or subsidence) requires disequilibrium in order to drive it back toward equilibrium. In either case, they should explain more precisely what they mean.*

We have rewritten the text in the introduction and placed it into a separate subsection 1.2. We also show mathematically that the global equilibrium corresponds to a lower potential energy than the state of local, isostatic equilibrium. This was posted as a comment in the discussion forum, and we added it to the appendix of the manuscript for completeness (and refer to it in the main text).

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**comment 7:** Table 1: This is quite helpful. But it will be even more so if all the variables are defined, their units or dimensions are listed, and the units are consistent in all the equations. For example, please explain the dimensions involved in the 'expression for power' column. In each case, the left side seems to have dimensions of power ( $ML^2/T^3$ ) while the right side has dimensions of energy ( $ML^2/T^2$ ). How, therefore, can they be equal if they are dimensionally inconsistent? Also, shouldn't the heat equation include factor for specific heat capacity and mass density? Otherwise,  $\nabla J_h$  must have dimensions of degree-meters per second, rather than the more common watts per square meter. Similarly, the net force equation seems to say that the time rate of change of momentum equals the divergence of force rather than the sum of forces; assuming that the  $\nabla$  operator implies an inverse length scale, and that  $F$  indeed has units of force, then the equation is dimensionally inconsistent. The same applies to the third conservation law, in which  $J_m$  ought to have units of mass-length / time. Clearly the authors are simply trying to give a shorthand sketch of what the conservation laws look like, but being imprecise with units/dimensions risks losing reader's attention. Better to be precise and consistent.

Thanks for pointing out these inconsistencies. It is, in fact, not  $dP$ , but rather an incremental amount of work  $dW$  that is shown in the table. Then, the units work out, since  $P = dW/dt$ . We added the heat capacity and density to the heat equation, and corrected the other equations as well by replacing  $\nabla$  with sums.

**comment 8:** 7323 the reference to conserving the total energy of the system is potentially confusing, because if you are conserving total energy, and energy merely changes form, then there would be no net  $dW$ , would there? A clearer way to express this might be that when one conserves each of several forms of energy – thermal, kinetic, and potential – then the  $dW$  for one may be  $-dW$  for another.

Yes, with internal conversions of energy, there is, in the end, no net  $dW$ . We clarified this section following the reviewer's suggestion: "For instance, when a small amount of work  $dW$  is being performed from differential heat to generate motion, kinetic energy

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is increased by  $dW$  at the expense of heat, which is reduced by  $-dW$ . During this conversion process, the total energy of the system remains constant,  $dU = 0$ , and it is merely the form of energy that is being altered."

**comment 9:** 7325 please state units of  $J_h$  (Watts?), and define  $Q$  (heat energy with units of J?)

We added the units to the text.

**comment 10:** 7328-9 There appears to be a sign problem here. When setting the left side of (9) to zero and solving for  $F_{w,acc}$ , shouldn't the right-hand terms be positive? Similarly, there is a change of terminology between (10) and its steady state form:  $F_{s,d}$  versus  $F_{s,fric}$ . Also, there is apparently an implicit  $F_{w,s} = F_{w,d} - F_{w,crit}$ , which should be made explicit.

When eqn. 9 (or eqn. 11 in the revised MS) is set to zero, then  $F_{w,acc} = F_{w,d} - J_{w,out}^p$ , so the right-hand terms are positive. This is also written this way in the manuscript, and we do not see a sign problem.

We changed  $F_{s,fric}$  into  $F_{s,d}$  in the text.

We made the definition of  $F_{w,s}$  explicit in the text.

**comment 11:** 7329, 8-10 In fact, ALL precipitation adds mass, and evapotranspiration removes some of it. Suggest stating explicitly that you are ignoring ET influxes and outfluxes (and why).

We added text to state explicitly that we exclude the fraction of precipitation that is evaporated and only consider runoff.

**comment 12:** -, 22 Can you explain/demonstrate why  $\phi_w = \phi_s$  is a good assumption? Presumably the geopotential is that associated with the mean land elevation, so this amounts to demonstrating that energy added by uplift is equivalent to mass flux times surface altitude times  $g$ .

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Yes, it simply means that we assume that the input to our system takes place at the surface, which is at the same elevation. We added text to clarify this.

**comment 13:** 7332, 3: here  $F_{w,fric}$  appears again instead of  $F_{w,d}$

Corrected.

**comment 14:** -, 21-3: the meaning of this sentence is not clear. Are you referring to reduced contact through channelization? Please expand/clarify.

Yes. We reformulated this sentence, which now reads "Once sediment is transported, it can be arranged into channel networks that have a lower wetted perimeter for a given water flow in relation to a uniform surface, thereby reducing frictional dissipation".

**comment 15:** Equation 15: justify the expression used for mass flux out of the system. Often, mass flux in a river system is expressed as something along the lines of mass density times cross-sectional area times mean flow velocity. How does that translate into  $m_w v/L$ ? In particular, explain to readers how  $m_w/L$  equals mass density times cross-sectional area (we can work it out for ourselves but it is easier if you explain it).

We added a brief derivation to show that the export can also be expressed as the mass density times cross-sectional area and flow velocity.

**comment 16:** Equation 22: does  $N_d$  have a physical interpretation; that is, can we think of it as the ratio of drag and gravitational forces or something like that? If so, it would be helpful to readers to explain this.

Not exactly. It sort-of reflects the ratio of drag to gravitational acceleration, but not quite. We added a sentence of physical interpretation to this number.

**comment 17:** 7335, 10-11: It is nice to see how the scaling limits correspond to two modes of flow. I think however that it might be clearer to interpret this as corresponding to turbulent and laminar flow, respectively. This is because the 'open channel flow scaling' could in principle also apply to turbulent sheet flow over a rough surface, while

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the scaling in eq 24 could apply to laminar flow in a channel. Later, when you refer to 'open channel flow', things start to get confusing because you are also contrasting cases with and without channels. Referring to these as 'turbulent-like' and 'laminar-like' scaling would remove this potential source of confusion regarding the presence or absence of channels.

We have clarified this explanation, also in response to review #2.

**comment 18:** 7335-6: the concept that sediment transport rate is proportional to power is at the heart of Bagnold's work on sediment transport, so it might be appropriate to cite that work.

Agreed. Reference was added to the text.

**comment 19:** 7336 Explain why you derive settling velocity from the horizontal (basin) length scale  $L$  rather than the water depth, or some representative scale thereof.

The "velocity"  $v_s = L/\tau_s$  is used to keep the expressions simple and not to represent a settling velocity as commonly used. We changed the terminology to avoid confusion, also in response to review #3.

**comment 20:** Eq 31: you could more intuitively derive this result from  $\tau_s v \gg L$ , meaning that the effective transport distance before settling is much larger than the basin length.

Yes, that is more intuitive. We added this interpretation to the text.

**comment 21:** Eq 33: I think this is essentially stream power: water discharge times mass density (so, mass flux) times  $g \sin \alpha$ . Suggest pointing this out in the text.

Correct. We added the description and link to Bagnold.

**comment 22:** 7337, 20: explain what 'deposited back on the slope' means: is this the power lost to heat when moving sediment grains return to the bed? Or is it an additional source of fluid energy loss to heat? If the former, then do equations 34-36 imply that all

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*power above the threshold is spent on sediment transport, and is that really realistic?*

Yes, this term represents the return of sediments to the bed. We clarified this sentence in the manuscript. Since we account for turbulent dissipation in the term  $D_w$ , the power  $P_{w,s}$  should represent the power available for sediment transport.

**comment 23:** 7337-8: *the argument that sediment flux dependence on gradient can be square root, linear, or square is difficult to follow. The two different scaling laws for friction are clear, but you can help readers by pointing out which equations turn this into three different scaling regimes for sediment transport. Better yet, write out explicitly the equations for  $J_{s,out}$  that explicitly show dependence on gradient.*

We followed the suggestion, rewritten that section and added a Table to more easily demonstrate the three scaling regimes.

**comment 24:** 7339, 14-22: *As the text notes, this is a simplification that ignores the multitude of pathways that water can take to reach a stream. It seems reasonable to simplify this, but can you justify why you choose overland (sheet) flow rather than, for example, porous-media flow for the hillslopes? Would the overall scaling come out the same?*

That's a very good point. At our level of simplification, the contact area of subsurface flow would be substantially greater, but otherwise will result in the same trade-off. We mention this point in the text.

**comment 25:** 7340, top: *insert "OF THE CHANNEL" after the word radius, to be clear that it is the channel, not the hydraulic radius, that is semi-circular.*

thanks, done.

**comment 26:** 7340-1, *discussion of drainage density (number of channels):*

**comment 26a:** - *explain how  $N_{opt} \rightarrow \infty$  means there are no channels ... would this not imply maximum dissection by channels, with no hillslopes? How does this compare*

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*with the famous Smith-Bretherton (1972) finding that the smallest wavelengths grow the fastest? There is some kind of apparent paradox here that ought to be resolved.*

The text has changed (following review #2) so this point does no longer applies. The question regarding the comparison to Smith and Bretherton (1972) is interesting, but since we consider steady state configurations, we are not sure how these two aspects are related.

**comment 26b:** - *What happens if you include an explicit relationship between water flow and spatial scale? For example, if  $J_{w,in}$  is the product of a runoff rate and  $L^2$ , then your length-scale dependence seems to disappear ( $L^{2/3}$  in both numerator and denominator), but you still have the oddity of an inverse relation between channel number and runoff rate.*

There was an error in the reasoning here, as pointed out by review #2, so we modified this section. It now shows that the optimum channel density and rainfall intensity (rather than runoff rate).

**comment 27:** - *lines 21 to top of next page: to the best of my knowledge, the observed relationship between drainage density and humidity/precipitation is not a simple one, or at least not monotonic. There is a fair amount of literature on this (though I don't have references to hand; one that comes to mind is by Moglen, Bras and possibly others, circa late 1990s). It would be good to note these studies, and point out where the model is and is not consistent with them (vegetation is thought to play an important role, which in the context of the authors' model might be seen as a correlation between humidity and roughness).*

Again, the point was removed from the manuscript, following reviewer #2, so that this point no longer applies.

**comment 28:** 7341: *you might point out that 1 kg/m<sup>2</sup>s of rain is 60 mm/hour (if I did the calculation right) – a heavy rain but not unheard of.*

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Thanks for pointing this out (oops). We adjusted the effective precipitation to a lower number and altered Fig. 5.

**comment 29:** *Eq 46: please explain why the geopotential gradient appears in what looks to be a buoyant isostatic uplift term. I would expect this term to look something like a force balance on a floating block, with a pressure difference above and below and some kind of viscous resistance, such that you would have an asymptotic relaxation toward isostatic equilibrium.*

It is not explicitly stated, but with this formulation for  $J_{s,in}$ , one gets an asymptotic relaxation to isostatic equilibrium in the mass balance since  $\Delta\phi \propto m_s$ :

$$\frac{dm_s}{dt} = (J_0 - k_{up}\Delta\phi/L) - J_{s,out} \quad (1)$$

The case of isostatic equilibrium is reached (with  $J_{s,out} = 0$ ), if  $\Delta\phi/L = J_0/k_{up}$ . We mention this in the text to make the choice of the expression for  $J_{s,in}$  more plausible.

**comment 30:** *Section 4.3 generally: maybe I am getting fatigued, or maybe the air-plane I'm sitting on is too crowded, but for whatever reason, I find the logic here difficult to follow. It is not intuitive why there should be a maximum in the rate of input in potential energy. Clearly, if you have a low erosion rate, you'll have a correspondingly low rate of isostatic uplift, but the opposing effect isn't clear. Perhaps it is that a high erosion rate implies a rate of loss of geopotential gradient that is too rapid to sustain with corresponding isostatic uplift? I'm sure if you set up a simple isostatic block with a height-dependent erosion rate, the system would automatically reach a kind of declining equilibrium state, and maybe that is what the optimum state is pointing toward. At any rate, I sense that there IS actually an intuitive explanation for the results in this section, but it eludes me. Please help readers like myself navigate this reasoning.*

We explained the results of this section in more detail.

**comment 31:** *Section 5.1: Use of the term 'disequilibrium' is potentially confusing. If I understand correctly, what is meant here is spatial variation in the driving gradient,*  
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*rather than unsteadiness in time. Apparently one could have a time-steady system that is in 'disequilibrium' by this definition.*

Yes, this is understood correctly. We altered the description to clarify the definition.

**comment 32:** *Section 5.2, lines 10-14: would not the 'non-structure' (hillslopes) also deviate from mean Del phi? In other words, if a network formed, would you not expect the hillslopes to become steeper than the original surface, just as the channels become (in general) less steep?*

Our definition of a structure includes both, the steepened hillslopes as well as the channels. Both parts of a catchment deviate from the mean slope. We added text to make this aspect more clear.

**comment 33:** *7350: I like the elegance of this thought experiment, but if it were posed in terms of an actual sediment transport law  $qs(x,y)$ , you might find (depending on the law) that the initial surface is concave-upward in one of its two dimensions – see Smith and Bretherton (1972). In other words, a simple planar surface might not be compatible with the assumption of steady, uniform erosion, because the water flux would increase downhill as it accumulates precipitation.*

Yes, agreed. Since this is only a qualitative thought experiment, we decided to simply point out this fact in the text, but kept the figure 7 as it is for simplicity.

**comment 34:** *7352: 'susceptible' rather than 'perceptible'*  
corrected.

**comment 35:** *7352, 27: the condition  $A_{structure} \sim A$  does not seem to fit with observations of real drainage basins, in which the channel network occupies a relatively small fraction of total surface area.*

In our definition of a structure (or better, its related disequilibrium) includes both aspects, the steepened hillslopes as well as the channel network. With this, this condition

fits generally with the notion that channel networks and organized hillslopes dominate real drainage basins. We added text to emphasize this point.

**comment 36:** *7356, 6-10: can you elaborate on how it is known that these two feedbacks are required to drive a system toward maximum power?*

Correct. Just the existence of the two feedbacks does not necessarily imply that the system reaches a state of maximum power, but rather the reverse. We altered the text and provided some more explanations.

**comment 37:** *6.2 general comment: this qualitative argument about feedbacks seems to be all that is offered in support of the claim that continental topography evolves 'at the fastest possible rate'. I suppose if there were a mathematical argument, the authors would have told us, but this comes across as a relatively weak demonstration of maximization.*

Except for stating that the maximum power state is stable to perturbations (which we added in response to comment 36 to the text), we agree that we do not have a better, quantitative mathematical argument.

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