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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.

G.E. Tucker

gtucker@colorado.edu

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

Overview:

This paper takes an intriguing look at the evolution of topography and river networks on continents, using a thermodynamic perspective. The authors cast the problem in a very general and generic light, building on the simple but important notion that there is a fundamental limit to the available energy supply.

The paper is certainly not the first to look at terrain or drainage networks from an optimality perspective. But one of its strengths is that it encompasses continental isostasy and potential energy gradients created by plate tectonics, as well as an analysis of the erosional side of the story. I like the attempt to boil down the system to its (energetic) essence, and the notion of a maximum available power supply is both intuitive and interesting.

Sometimes attempts to interpret geomorphic features in terms of optimization principles fall into the trap of not providing a pathway to optimality. In my view, in order to demonstrate that a system optimizes something like energy dissipation, it is important to look at the MECHANISM behind it. That is, one ideally should not merely show that a minimum solutions exists, but that the dynamics of the system have a natural tendency to progress toward that minimum, and to subsequently stay in or near that minimum (or maximum, as the case may be). In this paper, the authors commendably avoid this particular pitfall by providing at least a semi-quantitative analysis of how a drainage network – a 'structure' in their terminology – could arise and evolve toward an optimal state in response to a perturbation.

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 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).

Some elements of the paper are difficult to follow, and I have flagged these in the comments below.

General:

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?

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 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.

Specific:

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.

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 problem, 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.

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.

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 (MLˆ2/Tˆ3) while the right side has dimensions of energy (MLˆ2/Tˆ2). 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.

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 be -dW for another.

7325 please state units of J_h (Watts?), and define Q (heat energy with units of J?)

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

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).

-, 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.

7332, 3: here Fw,fric appears again instead of Fw,d

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

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).

Equation 22: does Nd 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.

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 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.

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.

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

Eq 31: you could more intuitively derive this result from ts v » L, meaning that the effective transport distance before settling is much larger than the basin length.

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.

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

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 Js,out that explicitly show dependence on gradient.

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?

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.

7340-1, discussion of drainage density (number of channels): - explain how Nopt -> infinity means there are no channels ... would this not imply maximum dissection by channels, with no hillslopes? How does this compare 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. - What happens if you include an explicit relationship between water flow and spatial scale? For example, if Jw,in is the product of a runoff rate and Lˆ2, then your length-scale dependence seems to

disappear (Lˆ2/3 in both numerator and denominator), but you still have the oddity of an inverse relation between channel number and runoff rate. - 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).

7341: you might point out that 1 kg/m2s of rain is 60 mm/hour (if I did the calculation right) – a heavy rain but not unheard of.

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.

Section 4.3 generally: maybe I am getting fatigued, or maybe the airplane 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.

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, rather than unsteadiness in time. Apparently one could have a time-steady system that is in 'disequilibrium' by this definition.

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?

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.

7352: 'susceptible' rather than 'perceptible'

7352, 27: the condition Astructure $\sim$ 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.

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

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.

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