Reply to reviewer ESDD-1-C135-2010 comments on “Assessing life’s effects on the interior dynamics of planet Earth using non-equilibrium thermodynamics” by J. G. Dyke, F. Gans and A. Kleidon.

We would like to take this opportunity to thank the reviewer for their insightful and very helpful comments which has allowed us to make significant improvements to the manuscript. Below we have reproduced the reviewer’s comments and responded on a point by point basis. The reviewer’s comments will be in ***bold italics***. Our replies will be numbered.

This manuscript provides some very interesting and thought provoking models of the interaction between Earth’s interior and surface, between biotic and geologic activities. The simple models are very interesting and I believe the work presented in this manuscript deserves being published. Still I have so major comments concerning the presentation. Indeed, the equations are simple but the assumptions used and the notations are very often simply not explained. It is very cumbersome to try to guess what the authors have assumed, what is the meaning of the symbols and then try to get the same equation. Quite often, some assumptions are given at the end of the problem (example line 4 page 213) not at the beginning.

1.We agree with the reviewer that insufficient information about the model formulation was given in certain parts of the manuscript. This did not allow easy comprehension of the models and their solutions. We have addressed this by explaining/defining each term immediately after its introduction in the text.

The schematic figures (Fig.4, 7and 9) are not helpful at all to understand the equations and should be redrawn with model notations/equations in mind.

2.We have redrawn Fig. 4, 7 and 9 and included in them additional terms from the respective models. We have also expanded the figure captions in order to further explain these terms and the model formulations

Finally, I have some big concerns with the third model (which may be linked to a lack of explanations?) and the assumption of isostatic dis-equilibrium.

3.We have rewritten and extended parts of this section in order to better explain the assumptions of the model, but also and importantly, to correct an error in the original manuscript that relates to the solution of the uplift model.

Introduction (also line 9 page197): it is often said that the Earth heat production is linked to 2 sources: fossil heat and radiogenic heat. This is oversimplistic and wrong (see eg. line 10 page 206) since phase transition (freezing) or other processes may have an important contribution.

4.We agree that we failed to mention the important process of outer core freezing. The text has been changed in a number of places to include this process.

The presentation of MEP is a bit lengthy and probably not necessary. In particular (lines 10-15 page 205) the example is not appropriate: if J = Jc+Jv is assumed fixed, then the maximum of equation (8) is easily computed and (for deltaT small compared to T) we find Jv = Jc (equipartition of fluxes). I am not sure this very

specific example is a good illustration of MEP. Besides, it is a bit strange to write a simple equation without solving it (equation 8)... But may be the text "we assume a fixed heat flux J" (line 11) was not what the authors meant?

5.For the model of mantle convection, we assume that heat production in the mantle (via radiogenic decay and release of heat from the core) is fixed along with the rate of conduction of this heat through the mantle. What is allowed to vary is the rate of mantle convection (which is a more important process for the transport of heat from hot interior to colder surface). We assume steady state, so the heat flux through the surface of the Earth, J, is fixed. What varies is the temperature within the Earth's interior. As the rate of convection in the mantle changes, so will the temperature profile in the Earth's interior change. We have changed the text in the presentation of the principle of MEP in order to make it clear that the principle may be useful in cases where important information about a system is missing.

line 6 page 206: Figure 5 instead of Figure 4.

6.The text has been changed accordingly.

line 20 page 207: "fossil and latent heat input will decrease". I don't understand why this should not be the case also for radiogenic heat....

7.This was an error in the original manuscript. The text has been changed accordingly.

equation (12). The choice of boundary condition is very strange: r=0 is the center of the Earth, not the core boundary. Tcore is not known, why not use Tsurface ?

8.The temperature of the surface of the Earth (approx 280K) is used as a boundary condition for the mantle convection system. The other information that we have is the rate of conduction of heat through mantle rock (from laboratory experiments). What is unknown is the rate of convective transport of heat through the mantle and consequently the mantle temperature at different depths. This is not strictly true because for the model of oceanic crust recycling we know the temperature of the material that rise up through mid oceanic ridges which will be approximately the temperature of the asthenosphere. However this additional information would not allow us to determine the temperature of the mantle at lower points within the Earth. Our approach can be understood as an extension and refinement of Lorenz (2002).

This model makes a lot of implicit assumptions that are not even mentionned (km, cm, Phi... are uniform in the Earth, do not depend on temperature, ...) but may contradict some later statement (line 4 page 209: "rates of convection will vary with temperature")

9.We have taken care to clearly define/explain all terms in the model formulation. Within the mantle we assume that rates of conduction are fixed, whilst convection will vary.

equation (15): now Ts is given (in contrast to equ. 8). Why not giving the analytical formula for entropy production (this integral is a priori easy to compute from equ. 8... a priori with arctanh()). ? This is a pity when we have a nice, simple model, not to give the answer !

10. It is possible to formulate entropy production within the mantle as a function of mantle convection. We did not include this equation in the manuscript as it offers little insights. It is reproduced below:

$$\sigma_m = 16k(N-1)\left(\left(\frac{3}{2}+\frac{J_h r_e^2}{12kN}\right)r_e - \frac{3}{2}\sqrt{\frac{6T_s kN}{J_h}+r_e^2}\tanh^{-1}\left(\frac{r_e}{\sqrt{\frac{6T_s kN}{J_h}+r_e^2}}\right)\right)$$

Its solution is found numerically and shown in Fig. 5.

line 25 page 210: Figure 7 instead of Figure 6.

11.The text has been changed accordingly.

page 211: what are Toa, Tmc, Tca... Notations should be introduced and explained. And assumptions written (eg. Tmc = Tmo...)

12.We have defined each term immediately after it is first introduced by adding a brief explanation in parentheses (please see reply 1). in this instance, *T_oa* is surface temperature of oceanic crust, *T_mc* is temperature of the upper mantle (which we assume is the temperature of newly formed oceanic crust), *T_ca* is the surface temperature of continental crust.

line 14 page 211: "heat diffusivity ... is the ratio of its density and heat capacity". This is wrong (dimensionaly impossible).

13.The text has been changed accordingly.

this model 2 is very difficult to understand since assumptions are not clear. Why using a time varying equation (equ. 19) for a steady state problem? Time should be replaced from the beginning by space (t = x/v0), otherwise it is very difficult to understand the model. This part should be reformulated completely (with a figure explaining the different terms).

14.We have replaced time with space from eq (19) to (23). We have updated Fig 7 and its caption (please see reply 2.).

This is a 2D advective diffusive steady state problem and it should be explained as such. As a result, I cannot understand the use of gamma (equation 29) as the control parameter of this model. Again, please explain the hypothesis, what is fixed, what is variable, ...

15.The assumption is that ocean crust production at mid oceanic ridges and subduction at plate margins will change as upper mantle temperatures change. We assume that the rate of oceanic crust recycling can be estimated by solving the model so that the oceanic crust recycling produces maximum rates of entropy production. This gives us a ‘tuneable’ parameter - the gamma term - that in part determines the rate of oceanic crust recycling. The gamma term is analogous to the mantle diffusivity term in the model of mantle convection that determined rates of mantle convection.

model 3 line 16-18: "not at isostatic equilibrium... isostatic imbalance". I have a big

concern with this third model... Mountains have roots which makes them at isostatic equilibrium.

16.We agree. This was an error in the original manuscript. The text has been changed accordingly. This does not alter the assumption that if material is removed from continental crust there will be a proportional change in uplift.

Besides, the isostatic time scales (thousands of years) are much faster that the erosion time scaes (millions of years). So I do not understand how geopotential energy can enter into this model.

17.The reviewer is correct in that erosion rates are typically magnitudes smaller than isostatic rebound rates. Following on from the reviewer's comments, we discovered an error in the solution for eq (38) and so the rates of continental crust erosion that leads to maximum power generated by the continental crust uplift system. An appendix has been added to the manuscript that gives details for the solution. We find that maximum rates of power generation in the uplift & erosion model are found when the erosion parameter is set to a value that produces an erosion timescale of some two million years.

Again, a simple schematic figure would help to explain Zca, Zmc deltaZc, deltaZs, what is the zero line deltaZc1 and deltaZc2 ?

18.We have updated Fig 9 (please see reply 2.) and have changed the description of the model. The "zero line"' is a hypothetical distance from the centre of the Earth to the surface which has no continental crust or oceans. The zero line is analogous to the water level on the hull of floating ship. Continental crust floats on the underlying asthenosphere. The total thickness of continental crust (from surface to crustal root) is denoted with Δz. The proportion of continental crust above the zero line is denoted with Δz_1. The proportion of continental crust below the zero line is denoted with Δz_2 and Δz_{ca} is height above the mantle. z_{mc} is the depth of the mantle/crust boundary.

<u>References</u>

Lorenz, R. D. (2002). Planets, life and the production of entropy. International Journal of Astrobiology, 1(1):3–13.