

Reply to reviewer ESDD-1-C149-2011 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.

In this very interesting contribution, the authors describe a model of the Earth system and explore the impact of the presence of life at the surface on the dynamics of the Earth interior. This is mostly a mathematical view of how energy is transferred through the Earth (from the core up to the surface), allowing to explore the coupling between the Earth surface and the planet interior.

Overall, the subject and the modelling deserve publication in ESD. Although the description of the processes at play is probably oversimplified, this is inherent to the scale of investigation. The organisation of the manuscript is a bit awkward and the role of biotic activity on erosion and weathering must be further discussed. However, accounting for the comments below, I think the manuscript can be improved. For the following reasons, I recommend a major revision before publication.

The paper starts with a general description of the processes carrying energy from the core to the surface. Then the models used are described. I have no particular problem with this. My main concern is about the results and discussion section. The results are mainly summarized by plots of the model output as a function of the choosen parameters. Showing the sensitivity is fine, but the physical and geological meaning of the parameters should be discussed. What are the plausible physical range for these parameters?

1. We have included more quantitative discussions of the results and have situated them within other empirical/modelling studies. For example we use the solutions for the uplift & erosion model to produce an estimate of global erosion of continental crust of 8.2×10^{14} kg/yr. This is, for example, approximately a magnitude greater than the estimates of Syvitski et al (2005).

I’m particularly concerned by the kcs constant, which describes the efficiency of the erosion. The authors illustrate the impact of kcs on the crustal thickness (low erosion efficiency results in high thickness). But by how much should kcs change in the absence of biotic activity? I found that the discussion is pertinent, but too disconnected from the model itself. Indeed the discussion is largely qualitative (less biotic activity promoting more intense degassing at ridges), but I would like to see numerical ranges and values.

2. We have updated the manuscript with a more quantitative discussion about the impacts of life on weathering and total erosion rates. Following Walling & Webb (1983) we assume that the ratio of eroded/suspended and weathered/solution of flux from continents to oceans via rivers is 4:1. Erosion is more important for the transport of material from continents to oceans. Indeed, a ratio of 40:1 may be more appropriate. Following

Schwartzman & Volk (1989; 1991) we assume that life (in particular vascular plants) has had a ten fold increase in chemical weathering rates (they argue for a stronger effect, perhaps up to 1000 fold). Therefore, if all life on Earth (or at least all life involved in chemical weathering) were to suddenly become extinct, then we would approximate that our estimate of total flux from continents to oceans of 8.2×10^{14} kg/yr would be reduced to 6.7×10^{14} kg/yr.

For instance, by how much should the hydrothermal activity increase if the biotic activity is removed ? The authors should produce more quantitative results. I feel that there is a gap between the model description and the discussion, which should be filled with quantitative results.

3. We have extended the “Implications for the long-term carbon cycle” section. This includes a more quantitative discussion of the impacts of outgassing of CO_2 with altered rates of oceanic crust recycling. We enumerate how much CO_2 outgassing would change with a decrease in the flux of material from continents to oceans. The text now reads:

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“The BLAG model of Berner et al. (1983), models interactions between tectonic processes such as sea floor spreading and partial pressures of CO_2 in the Earth’s atmosphere. One conclusion is that faster rates of sea floor spreading would lead to faster outgassing of primordial CO_2 as this is largely released from mid oceanic ridges where new oceanic crust rises to the surface.

Gerlach (1989) estimates outgassing of CO_2 are mid-oceanic ridges to be $10 - 37.8 \times 10^{12}$ g/y while a more recent study on analysis of empirical data (Chavrit et al., 2009) gives lower estimates of 1.4×10^{10} g/y. It is worth noting that the significant amounts of volcanism is ultimately driven by the friction heating of subducting slabs of oceanic crust at convergent tectonic boundaries (Williams et al., 1992)

For a small change in oceanic crust recycling, it would be reasonable to assume a linear change in outgassing. However the dynamics are complex and consequently a linear assumption becomes less tenable with large increases in oceanic crust recycling. The degree of partial melt and solubility of CO_2 is unlikely to remain constant as the solubility depends on pressure, temperature, oxygen fugacity and the dynamics of CO_2 bubble formation (Bottinga & Javoy, 1989; 1990)

Following Chavrit et al (2009) we assume that the current rate of outgassing of CO_2 is 1.4×10^{10} g/yr. We scale this rate to our estimated rate of oceanic crust recycling of 2.84 cm/yr.

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There then follows two sets of results. The first set of results is based on the assumption that a change in the upper mantle temperature produces a change in the rate of oceanic crust recycling with the gamma ‘diffusivity’ term fixed. Previously, the gamma term was allowed to vary in the oceanic crust recycling model in order to find the rate of oceanic crust recycling the produced maximum power. When the gamma term is fixed, a decrease in the flux of continental crust from land to oceans produces an increase in continental crust thickness and an increase in upper mantle temperature This increase in mantle temperature produces an increase in the rate of oceanic crust recycling of 0.05 cm/yr and an increase in outgassing of CO_2 of 2×10^8 g/yr.

However, if we allow the gamma term to vary so that the oceanic crust recycling system relaxes back into a maximum power generation state, then whilst there is the same increase in continental crust thickness, there is a higher increase in upper mantle temperature. This leads to a *decrease* in the rate of oceanic crust recycling of 0.23 cm/yr and a *decrease* in outgassing of CO₂ of 0.12×10^{10} g/yr.

We explain this counterintuitive result by first noting its limitations. For example we do not vary the overall proportion of continental to oceanic crust, nor the ratio of the length and width of oceanic plates. Allowing any of these parameters to alter may produce significantly different results. The decrease in the rate of oceanic crust recycling and so outgassing of CO₂ is a consequence of the system relaxing back to a state of maximum power generation. By decreasing oceanic crust recycling, the temperature of the upper mantle temperature increases and so the temperature gradient over which heat flows increases. This allows the system to generate more power. For the new set of boundary conditions (increased thickness of continental crust), decreasing the flux of heat via the recycling of oceanic crust can produce more power even when this decrease in oceanic crust recycling leads to an increase in the flux of heat through continental crust; heat that is otherwise 'wasted' in the sense it is not used to drive the recycling of oceanic crust.

According to the title of the paper, the aim is to quantify the impact of the biotic activity on erosion on the Earth interior dynamics. Regarding the biotic impact on weathering and erosion, there are some key studies suggesting that chemical weathering is enhanced by a factor of 4 to 10 in the presence of land plants. Maybe the authors should check these contributions and cite them: - Drever, J.I., 1995, *Geochimica Cosmochimica Acta*, 58, 2325-2332 - Moulton K.L., West J., Berner R.A., 2000, *American Journal of Science*, 300, 539-570 - Berner R.A., 2004, *The Phanerozoic Carbon Cycle*, Oxford U. Press. Those contributions claims for an increase in weathering when vegetation cover rises. When the authors deal with an abiotic world, are they decreasing the kcs parameter by a factor of 4 to 10 ? Is it comparable?

4. Please see reply 2. We assume a 10 fold decrease in chemical weathering with the instantaneous extinction of life. We are very grateful for the suggestion of additional contributions and have included them in the manuscript. Following Schwartman & Volk (1989), it is important to note that if the geological carbon cycle were to relax to a steady state, then weathering of CO₂ would ultimately equal outgassing. It is possible that purely abiotic processes would lead to new steady state in which higher concentrations of CO₂ and global temperatures lead to the same rates of weathering as currently observed on the living Earth. However, we argue that given the possible feedback between continental crust and oceanic crust recycling rates, it may be necessary to relax the assumption that outgassing remains fixed whilst the system progresses to a new steady state.

However, the enhancement of weathering by biotic activity is not so obvious. As demonstrated by Millot et al. (2002, *EPSL*, 196, 83-98), the key factor controlling weathering rates is probably the physical erosion. In flat tropical areas, despite the presence of a dense vegetal cover, weathering rates are extremelly low (see for instance Viers et al., 2000, *Chemical Geology*, 169, 211-241). This is because land plants promote the formation of very thick weathering profiles. Furthermore, physical erosion is not promoted by the vegetal cover, but rather depends on the runoff, slopes and even uplift rate (see Von Blanckenburg F., 2005, *EPSL*, 462-479). Uplift and erosion cannot be seen simply as competing mechanism, but one can promote the other. I also suggest that the authors check the paper by Vanacker et al.

(2007), Geology, 35, 303-306. Vanacker et al. show that a dense vegetal cover slow down mountain erosion. This contradicts the hypotheses of the present paper. This should appear in the limitation section.

5. We do not consider possible changes in the potentially more important mechanical erosion processes and how they would be affected by life. We agree that there may be important local variations of biotic effects on weathering rates. We also agree that slope and runoff are very important for determining physical erosion rates. This was an important omission from the manuscript. Again we are very grateful for the suggestions of additional citations. We have included them in the Limitations section.

Regarding the organisation of the paper, I would rename the model 1,2,3 sections because they describe various part of the same integrated model. This is confusing.

6. We have renamed the model sections and removed their respective numbering. The intention behind numbering the models separately was to avoid possible misunderstandings that we had formulated a fully coupled, dynamical model. Although we see this study as the first step towards such a model, we have not done this. The models are either analytically or numerically solved in isolation. The series of sensitivity studies demonstrates how the model of uplift/erosion can affect the oceanic crust recycling and mantle convection models.

References

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