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Earth System Dynamics Discussions

Interactive comment on "Assessing life's effects on the interior dynamics of planet Earth using non-equilibrium thermodynamics" *by* J. G. Dyke et al.

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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 ? 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. 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.

2) 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 ?

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.

4) 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.

Interactive comment on Earth Syst. Dynam. Discuss., 1, 191, 2010.