

Interactive comment on “A simple metabolic model of glacial-interglacial energy supply to the upper ocean” by J. L. Pelegrí et al.

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Dear Sirs:

I find the Manuscript entitled “A simple metabolic model of glacial-interglacial energy supply to the upper ocean”, interesting. However, I find some points that should be addressed before it would be published on Earth System Dynamics.

Overall, I find that the use of a very simple metabolic model having into account few metabolic premises is, by itself, an interesting approach to the glacial-interglacial change of the atmospheric CO₂ concentration. The model presented is related to the metabolism of mammals, and it is introduced in a two box oceanic model.

Since my point of view, the most interesting result, is the good relation between the

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modelled variations in atmospheric CO₂ and those observed in the Vostok's time series. However, I was missed in the explanation of some parts of the model, and in some of the arguments. For example, the authors claim (page 276) that Brown et al. (2004) consider the rate of photosynthesis as a gross rate. While the metabolic theory considers that any kind of metabolism, per unit of mass, is related to temperature, either gross or net. This should be changed in the manuscript. In addition, on page 277 ro should be ro(s), and it must be clarified if the “net energy supply” is equal to “oxygen”. This is a mayor point, since “energy” could be directly linked to carbon through metabolism. Photosynthesis transform energy (light) to organic carbon, which is directly linked to oxygen by respiration.

The second major problem that should be addressed is that one thing is anabolism in mammals, and the other is if the earth is autotrophic, heterotrophic or could be in steady state. During the text this it is not clear. I have the general feeling that the earth system could be changing between autotrophic and heterotrophic states over geological timescales. But, some times, you say that the earth is “autotrophic in the long term”, and you also cite a work that affirms that most of the ocean is net heterotrophic (e.g. Del Giorgio and Del Giorgio 2004); while some biogeochemical approaches give us just the opposite view (see for example the works of Riser and Johnson, :10.1038/nature06441) I think that it could be important to not mix between your model and the metabolism of the system under study. Since my point of view, and since your own results and the observations of the Vostok core, the earth do not seems a long term autotrophic system. It looks to me, a system oscillating between autotrophic and heterotrophic periods. In addition, this oscillation could be the product of the metabolism of different processes acting in different times cales and places: as for example the surface of the sea versus the deep sea, or the sea versus the land. This is an interesting result of your manuscript as, even considering that the forest is not in your model, the correlation between the model and the CO₂ of the Vostock cores is still surprising high (0.8). Since my point of view, this is the most important point of the manuscript.

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Minor changes:

A shorter discussion and a few points conclusion could be acknowledged by people that is not used to work with models, and that could be interested in your work. In the same sense, it could be easy to the readers to have a maximum 5 to 6 figures. Maybe some of the actual figures and panels could be simplified to focus on the main point(s) of the manuscript.

Yours sincerely,

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