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

Anonymous Referee #2

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This paper tries to use an analogy between the mammal metabolic system and the earth system to gather insights in the glacial/interglacial transitions, seen as the analogue of the basal/enhanced state transition in a mammal. Reviewing this paper has been quite painful, as I was trying to understand whether I was not understanding the model because it was bad explained or because it did not make much sense. In the end, my opinion is that the analogy presented here is not particularly relevant, as mammals decide to "excercise" while it's not clear to me how the Earth system could "decide to start running". I like the idea of exploring analogies between different research fields, and this one could be potentially interesting, but in its present form it does not work. The premises of the model and the use of the model itself seem wrong to me (see details), and the manuscript is not clear. My opinion is that it is not suitable for publication.

Details:

The authors state that (page 296 line 16) "As the global thermodynamic state of the system changes, the deep circulation attempts to match the required aerobic supply." How can the deep circulation "attempt" to overturn faster? Do they mean that when there is more/less sunlight, more/less nutrients are needed at the surface and thus the circulation needs to speed up to bring more? What would be the motor of such circulation change? They say that an initial temperature increase would lead to more respiration and thus more DIC available for photosynthesis, but how could this affect the circulation? The reasoning is certainly lacking some important parts.

The model is eq. 10. This equation states that the rate of change of DIC in the upper ocean is defined by a relaxation to a time-dependent quantity (gamma) on a time-dependent temporal scale (1/Q). Specific assumptions on the shape of those time-dependent functions are made, so that their behavior is defined by a few parameters (three). Ice core data representing atmospheric carbon dioxyde concentration (assumed to be proportional to upper ocean DIC concentration) are used to obtain the best value for those parameters so that the modeled time series fits the data. It is thus wrong to use the same observative time series to validate the model. It is not a surprise that an equation such as eq.10 (that gives exponential relaxation to the fixed point, if it has fixed gamma and Q) with time-varying gamma and Q can produce some type of relaxation oscillation. The fact that the model and the observational time series have a high correlation does not prove anything about the model. So the model is not useful and cannot be right or wrong. The model does not model anything.

The description and the wording are often unprecise or confusing. Just some examples: -many pages are written to get to the model, which is eq.10. Many of the things that are written are not necessary, such as for instance the model for DOC (eq. 8). -page 277 line 1: the metabolic balance is defined as a comparison between gross primary production and community respiration, or net community production. The metabolic balance is the EQUILIBRIUM between intake and loss of nutrients, it's not

a COMPARISON; -many times ENERGY INPUT RATE and ENERGY are used interchangably) -page 297, line 2: "We have shown the earth's metabolic rate at any time is net autotrophic community production, which at steady state becomes net community production". Cfr with eq.3 where the metabolic rate is defined as net autotrophic community production. There is an inconsistency.

Interactive comment on Earth Syst. Dynam. Discuss., 2, 271, 2011.