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Earth System Dynamics Discussions

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Interactive Comment

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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The main objective of the paper by Pelegrí et al. (2011) is to explore the idea that the ocean circulation and the circulatory system in mammals have similar roles in the distribution of nutrients to the whole system, so that the temporal patterns of energy supply in both systems share important similarities. We know it is not an easy task to convince readers about the potential of such an approximation, but we do want to try. We think that Earth System Dynamics is a good forum for proposing this idea. The journal asks for "novel concepts and theories and their application to better understand the nature of the Earth as a complex, coupled system". The journal has Prof. James Lovelock in his advisory board, to me a guarantee that any "Gaian" approach will be carefully considered.

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Discussion Paper

Before going into the comments raised by the Reviewers allow me, first, to spend some time explaining what has motivated this paper and, second, to emphasize what is, from my perspective, the main paper's contribution.

Motivation

The similitude between ocean circulation and the circulatory system of a living being was proposed, five centuries ago, by Leonardo da Vinci: "While man has within him a pool of blood wherein the lungs as he breathes expand and contract, so the body of the earth has its ocean, which also rises and falls every six hours with the breathing of the world; as from the said pool of blood proceed the veins which spread their branches through the human body so the ocean fills the body of the earth with an infinite number of veins of water..." (from da Vinci's Notebooks). Leonardo da Vinci was a genius because of his intuition. He was capable of foreseeing relations, of imagining new perspectives, where nobody else could. Some of his peers surely thought his ideas were absurd, and perhaps indeed some of them were, but they were all worthy exploring. This is what we invite readers to do with this study.

I had the fortune of having Prof. Gabriel T. Csanady as my doctoral advisor. Prof. Csanady was a leading physical oceanographer, among his many recognitions he got the Huntsman award in 1991 (for a complete relation see Pelegrí et al., 2006). In 1989, while working on my doctoral dissertation under his supervision, we found that the Gulf Stream transports inorganic nutrients in subsurface layers, nearly as a close conduit, nutrients which eventually reach the sea surface at high latitudes to sustain enhanced primary production. In 1990 we presented these results at the Brookhaven National Laboratory with the title "The Nutrient Stream, artery of Gaia, the living planet", because of their similarity with arteries in mammals and because of their analogous role in transporting oxygen and nutrients (as explained in page 96 of Pelegrí et al., 2006). Shortly after the presentation we published a paper on the nutrient stream which has been extensively cited (Pelegrí and Csanady, 1991).

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This was the origin of my interest in the physiological analogy. I realized that mammals, as oceans, have long and short circuits with different dynamics and sustaining distinct biogeochemical processes. In 2004 I had the chance of spending some quality time to read about Gaia theory and also about physiology, and was intrigued when I realized that the temporal pattern of some physiological variables (like the heart beat or the artery-venous oxygen difference) during rest-exercise-rest transitions is very much alike the temporal pattern of climatological proxies during glacial-interglacial-glacial transitions.

Main contribution

From the reviewers' comments I have realized that possibly we were not clear enough pointing out what is the main novel contribution of the paper, so I will clarify it next. We propose the amount of solar energy transformed by the Earth System switches between two different states (basal and enhanced) through the utilization of proximal and remote nutrients, in a similar way as any mammal would transform organic matter to generate energy. With this idea in mind we propose a very simple model for the transformation of solar energy, fully inspired in classical ideas on the metabolism of mammals. The supply of dissolved inorganic carbon (DIC) and nutrients to the autotrophic productive ocean occurs through remineralization of upper-ocean organic reserves (proximal sources) and the advection of deep-ocean waters (remote sources). The metabolic rationale leads to equation (6) which turns out to be the same equation we would derive from simple conservation arguments for DIC in the upper compartment of a two-box ocean (equation 7). This equation, with only two controlling parameters, is tuned to provide a good fit to atmospheric CO_2, which changes in approximate linear proportion to variations of DIC in the upper ocean.

This is a simple but yet very important conceptual approximation. It surely is a simplified approach to the Earth System but not a simplistic one. We believe the model may be improved by incorporating additional elements but yet retaining the basic idea: the ocean switches between two states and does so through a combination of short-fast

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and long-slow supply, as it happens in complex living beings such as mammals. The paper does not aim at explaining when and why the system changes from one state to another. Instead, it shows that these changes have similar temporal patterns as those occurring in mammals, and uses the physiological analogy to gain insight into what may be the energy transformations that take place during the glacial-interglacial-glacial system transitions.

We next respond to both referees. I would like to sincerely thank them for their input, we appreciate it. I recognize the paper presents a non-traditional approach which, despite our increased awareness of the homeostatic behaviour of the Earth System, is difficult to explain and accept. I will try to respond to both referees under this perspective.

Response to Referee #1

Referee #1 writes a comment which he entitles "Wrong analogy, wrong model, wrong results". He recognizes "analogies can be useful" but thinks in this case "the analogy appears not relevant at all, or even preposterous" (lines 3-6 of comment by Referee #1). I think "preposterous" is an unfortunate adjective, as in no way the referee has shown the physiological analogy to be "contrary to reason or common sense; utterly absurd or ridiculous" (definition of "preposterous" according to the online Oxford Dictionary). A referee has the right and duty of being critical but he/she should also be equanimous, the referee is not the depositary of all reason and should refrain of disregarding other people's work.

Anyway, I am grateful to this reviewer for his time reading and commenting the paper, I'm happy our paper did not leave him indifferent. I respect, but do not share, most of the referee's argument against the paper. I will next clarify some points hoping I can convince him that the analogy may be useful to help understand how the Earth System works. I will follow the same organization used by the referee in his review: The analogy, the model, and the real world.

The analogy. The referee summarizes well the basic analogy of the paper but goes

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on to say that we did not “explain in plain english what this analogy means”. I do not agree, the analogy is clearly posed from the very same Abstract (lines 8 to 14 in page 272) and carefully formulated along Section 2. Actually, we did spend more than six pages slowly explaining the idea, as we knew the importance that it became very clear. At the end of Section 2 we summarized it with a full paragraph (lines 4 to 15 in page 282) and through Figure 1. The concept is again explained in the Concluding remarks (Section 6, page 296, lines 5 to 25).

The referee argues that we do not adequately justify the succession of events: at the beginning of an interglacial period the ocean becomes more productive and a faster circulation develops. The referee is correct, we do not properly substantiate it. We simply give some hints (page 298) on what is happening based on the similar sequence of events in mammals, this is why the physiological analogy is so important. Why the heart of a mammal starts pumping more blood as it wakes up? The answer is, as noted by the referee, that higher energy demand leads to a stronger ocean circulation. We don’t have a clear justification for this as this was not the purpose of the paper (see, however, lines 17 to 27 of page 297). We simply say that the chain of events may be similar to the sequence of processes that provides energy to mammals. The referee says that “this succession of events is against all known biogeochemical processes” but does not justify his statement.

The referee ends up saying the following: “This leads to some quite remarkable statements (page 295), like for instance: "these are the reserves that will be necessary to sustain the next glacial-interglacial transition" How does the "Earth" accomplish planification for things to happen next? "the deep ocean circulation attempts to match the required aerobic supply" (page 296) How does this work ? Where is the nervous system in the analogy?” There is, of course, no planning for this sequence of events. The Earth does not plan to begin exercising, the same way as a mammal does not plan waking up or falling sleep. The early Gaian perspective was criticized of attributing the Earth such “self-conscience” but this was long ago clarified by Prof. Lovelock and other scientists.

The model. The referee has two main concerns. The first one is that the model is not conservative. This is not so, the referee is ignoring the equation for organic carbon. Equations (7) and (8) are conservative for the upper ocean. Equation (8) shows that the thermohaline circulation is a source of organic nutrients to the deep ocean, where these materials will have plenty of time to become remineralized. We agree the model is largely idealized by the absence of factors such as particulate export to the deep ocean and changes in coastal erosion between glacial and interglacial cycles, both of them affecting the content of inorganic nutrients in the deep ocean. However, it is not clear whether inorganic nutrient content has significantly changed within the deep ocean in the past (e.g. Sigman and Haug, 2003) so the simplest hypothesis is to choose it constant.

The second concern of the reviewer is related to the relation between atmospheric CO_2 and DIC in the upper ocean. He says that the larger part of DIC is in the form of bicarbonate or carbonate and argues that "it is very misleading to consider that the evolution of DIC at the ocean surface should always be equivalent to the evolution of CO2." This is not so, the change in total DIC in the upper ocean (carbon dioxide, carbonate or bicarbonate) will be reflected approximately in linear fashion by changes in atmospheric CO_2. Actually, if we let constant temperature, salinity and alkalinity then it is easy to show that the relation is almost exactly linear in the 200 to 300 ppm pCO_2 range. The referee is right that in general the relation is not truly linear because of the dependence with temperature, and to a lesser degree through coupling with salinity. However, under the assumption of constant alkalinity, changes in CO_2 depend only slightly on temperature and salinity. To assume constant alkalinity is a reasonable approximation as this variable does not change through the introduction of CO_2 into the water and neither changes much because of synthesis or remineralization of organic matter. A similar approximation has been used by other authors, such as Brewer and Peltzer (2009), where changes in anthropogenic pCO_2 are translated almost directly to changes in ocean carbon content.

The real world. The referee says it is “quite unrealistic to change the global biological production (the "metabolism") by more than 10 or 20% according to numerous paleoproductivity data.” There is a lot of controversy in these numbers but, even if true, our model is not against it. There are two different concepts: energy supply and metabolism. During a glacial-interglacial transition the energy supply to the upper ocean (in the form of DIC, either from remineralization or from deep sources) temporarily increases by an order of magnitude but all this increase does not traduce into an increase in metabolism. Most of it goes into increasing the DIC in the upper ocean and only a fraction goes into increased productivity. Actually, during a glacial-interglacial transition we propose that respiration will initially exceed production and only at the peak of the interglacial period, when a steady state is approximately reached, production may substantially exceed respiration. This excess of production over respiration will remain but progressively decline until the deep glacial. We agree with the reviewer that this issue was not properly explained in the original paper and thank him for pointing it out.

The referee also says that “abyssal circulation may not be very relevant for biology”. I believe this is not true at the time scales we are interested, as the deep ocean is the only new significant source of inorganic nutrients. The referee continues saying “Similarly a time constant tau0 = 40 kyr (or equivalently a deep ocean circulation of 0.4 Sv) is way out of any physically relevant range. This looks more like a diffusive time for the Ocean, which would imply no circulation at all (ie. no winds, no tides, anoxia in the bottom, ...). This is also against all the available evidences on physics and paleoclimatology.” The referee is probably correct that a deep-ocean glacial circulation of less than 1 Sv is likely too small (but do we have actual quantitative estimates for the thermohaline circulation at the peak of glacial periods?). However, it must be noted that the maximum recirculation time occurs only when epsilon becomes zero. In most of our simulations we let epsilon go to zero right at the beginning of the glacial period

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but this is not necessary for the model to give good results. Figure 8 shows a case where epsilon decreases progressively during the glacial period and reaches zero only at the glacial maximum. An increase in epsilon reduces the recirculation time and, therefore, will cause a faster glacial exponential decrease. Therefore, in this particular simulation we could let epsilon never reach zero and yet the data fit would improve slightly. Please note the legend of Fig. 8 has a typo, the Gaussian function refers to the epsilon recirculation parameter as explained in the text (page 291, lines 6 to 10). In summary, for the model to work reasonably well it is not necessary to totally block the deep ocean circulation. However, we agree with the referee that these low recirculation rates are likely too small, they are probably indicative that a more realistic model would require a high-latitude compartment, as suggested in the last paragraph of the paper (page 299).

Finally, the referee says: “A typical statement (page 297): "Box models have been repeatedly used with substantial success in ocean sciences but, to our knowledge, they have never been used to simulate glacial-interglacial transitions". There are numerous examples of box models that have been designed explicitly for this question. Did the author actually read the papers they are citing? (eg. Sarmiento et al. 1984; Siegenthaler et al, 1984; Toggweiler, 1999; Paillard et al. 2004). There are probably hundreds of other references on this point. Box models have been heavily applied to glacial-interglacial carbon cycle changes.” In the paper we talk about simulating glacial-interglacial transitions, i.e. the temporal evolution of the system during these state transitions. Sarmiento and Toggweiler (1984), Siegenthaler and Wenk (1984) and Toggweiler (1999) do not simulate the temporal evolution. Rather they describe the conditions during glacial or interglacial state, examining different scenarios. Didier Paillard and collaborators (e.g. Paillard and Parrenin, 2004) have done an excellent work to improve our understanding of what are the mechanisms responsible for switches in the state of the system. Their model does very well at simulating the transitions by solving simple equations for global ice volume, Antarctic ice sheet area and atmospheric CO_2 with the help of an oceanic system-switch parameter which itself is a function of

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global ice volume, Antarctic ice sheet and daily insolation at 60°S. We did not think of this model as a box model, as the connection between the upper and lower box is not explicit, but we do agree with the reviewer that, in a sense, it could be considered as a box-type model. We will correct the paper accordingly.

Response to Referee #2

We are grateful to Referee #2 for his time reading and commenting the paper. This referee thinks “that the analogy presented here is not particularly relevant, as mammals decide to "exercise" while it’s not clear to me how the Earth system could "decide to start running".” This is not the case. Most of the metabolic changes in mammals, as for any living being, are not conscious. The simplest example is waking up or falling sleep, in normal conditions the system does so following the day-night cycle, but there are many other instances. Certainly, a human being deciding to exercise is an “abnormal” example. We are sorry to hear the referee found the paper “not clear”. We understand it is not an easy paper as it combines concepts from two very different disciplines: oceanography and physiology. Further, we think the usage of “aerobic” and “anaerobic” terms applied to external and local energy sources may have been confusing. This was explained at the beginning of Section 2.2 (lines 8-10 and 15-16 of page 280) but we agree it is counter-intuitive and may be confusing. We are committed to improve the readability of the revised version of this paper.

Referee #2 raises two major concerns and points at a number of minor issues. The major aspects raised by this referee are (1) the lack of justification for the sequence of events implicit within the model, and (2) the little value of the model results because its parameters are tuned with the same time series it aims at predicting. I will address them next.

As mentioned above (“Main contribution” epigraph) in this paper we did not aim at justifying the sequence of events during state transitions. We solely wanted to use the physiological analogy, in our case with mammals, to propose what the sequence of

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events could be. As explained before ("Motivation" section) we had several reasons to pursue this approach and I still believe the approach is potentially very useful. Under the physiological approach the question "Why the heart begins to pump more blood to an organ?" is analogous to the question "Why the thermohaline circulation increases?" A good answer to the first question may be useful to improve our understanding of the second one. With our paper we try to open new perspectives, we think a journal like Earth System Dynamics is the right place for it. Despite the many fundamental differences between mammals and the Earth System, we believe this new approach may be enlightening.

The second issue raised by Referee #2 relates to whether it is a malpractice to calibrate a model, i.e. to choose the best-fit parameters, using the same data set we aim at reproducing. I think it is not, this is what modellers do continuously when hindcasting events. The parameters of the model are carefully selected so that the numerical prediction fits well some of the observed data, typically at just a few points, and then the model is assumed to work well elsewhere. We are essentially doing the same: we use a fraction of the data to find the best-fit model parameters which are then used to predict the full time series. As noted by the referee, we have only a few parameters to select for each glacial-interglacial cycle which then serve to predict the whole cycle. In my opinion the important point is that the simple model equation and the two-state switch are inspired on the physiological analogy. They are simple, the reviewer is right, but they do grasp the essence behind the observations. Otherwise, no matter how much tuning we do, they would no be able to properly reproduce the data.

The referee also points at several minor issues, as follows:

- We use many pages to get to the model. The referee is right, but we thought it was important to clarify the main concept behind our paper. In the revised version we will try to simplify this explanation. I think the model for dissolved organic carbon is not superfluous as it helps understand another way carbon is transferred to the deep ocean. Its importance, however, will be clarified in the revised manuscript.

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- We thank the reviewer for his clarification on the concept of metabolic balance.

- We will be careful with the usage of "energy" throughout the paper.

- The referee is right, the sentence in lines 1-3 of page 297 should read "We have shown the earth's metabolic rate at any time is net autotrophic-community production."

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