Earth Syst. Dynam. Discuss., 2, C96–C99, 2011
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Earth System Dynamics Discussions

ESDD

2, C96–C99, 2011

Interactive Comment

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

Anonymous Referee #1

Received and published: 2 May 2011

In this manuscript, the authors are using an analogy between the metabolism of some complex organisms consumming oxygen, like mammals, and the changes in the carbon cycle oberved between glacial and interglacial states. Analogies can be useful when they provide some enlightening explanation of some complex problems. This is unfortunately not the case here, where the analogy appears not relevant at all, or even preposterous. Besides, the model developped on these premises does not satisfy some standard of good modeling practise, like conservation laws. I therefore consider that this manuscript cannot be published it is current state, and I do not believe that any improvement is possible based on the manuscript premises.

The analogy. The basic assumption is to consider a metabolic system with 2 different states, an "active" or "high energy" one, and an "inactive" or "low energy" one. When a

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mammal becomes "active", its muscle cells require more oxygen, supplied by a blood circulation which will accelerate, because of this increasing demand. Potentially, in the meantime, the muscle will work on its stockpile using anaerobic processes. The reverse is also true when the organism becomes "inactive", blood circulation slows down, and stockpile can be replenished. The authors translate this into simple equations and apply them to the glacial carbon cycle, with "carbon" or "nutrients" replacing oxygen, ocean circulation replacing blood circulation, glacial replacing "inactive" and interglacial being "active". But they do not "explain" in plain english what this analogy actually means. So I will try to do it here: At the end of a glacial period, the ocean biology "decides" to develop, or is triggered in some fashion into a very high productive mode (why it should be so is not explained). In order to develop faster, they "request" that the oceanic circulation accelerates in order to bring more nutrients to the surface (I personnaly do not quite understand how a "higher energy demand" - in the authors terms - can lead to a stronger ocean circulation. Conventional wisdom works the other way around: increased nutrient supply triggers blooms, not the opposite). Since the organisms can't wait for the deep ocean nutrient supply, they eat in the meantime the dissolved organic matter available (the "anearobic" stockpile, though here the process is in fact aerobic). This succession of events is against all known biogeochemical processes. I cannot imagine how this works, and I believe that the analogy is simply not possible between these two systems. 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 ?

The model. Carbon is not "conserved" in the model. Indeed, the authors assume that the DIC in the deep ocean is constant through time. In other words, the deep ocean is an infinite reservoir, while the upper ocean has a mean depth of 1400 m (page 288). Since the mean Ocean depth is about 3500 m, the upper part accounts for about one third or almost one half. Carbon conservation requires that, when carbon decreases in the upper part, it must increase in the lower part (and reciprocally). This turns out to be actualy the main difficulty with the glacial CO2 problem: when atmospheric (and ocean surface) carbon is low, then the deep ocean DIC content has to be high, and even a weak ocean circulation tends to increase the surface (and atmospheric) carbon content. The authors state: "Another important limitation in our model is the lack of particulate organic matter export. Such export is partly responsible for maintaining the large DIC and inorganic nutrient concentrations in the deep ocean, something we waive by simply setting constant concentrations in the deep ocean". It is of course not equivalent at all...

Dissolved CO2 accounts for about 1% of DIC (which translates into a similar ratio between oceanic and atmospheric carbon content), the larger part of DIC being bicarbonate or carbonate. It is very misleading to consider that the evolution of DIC at the ocean surface should always be equivalent to the evolution of CO2. Since the model is "dimensionless", it is only possible to compare the shape of the model results (the timing is in fact imposed externally at terminations) with the data, not the amplitude which is meaningless here (see above, DIC does not easily scale with CO2). So, except for a very classical exponential-type of decrease, I do not see what the model actualy reproduce or predict...

The real world. There is a considerable amount of information on the state of the ocean, the productivity, the carbon content of the surface or deep ocean during glacial interglacial cycles. The authors appear to be unaware of all these data. For instance, it is quite unrealistic to change the global biological production (the "metabolism") by more than 10 or 20% according to numerous paleoproductivity data. Though the manuscript is not very explicit on this point, the changes between the two-states (the ocean circulation as well as the biology) are scaling with epsilon, which can be as high as 50. It is completely unrealistic (ie. against all available evidences) to think that biology or ocean circulation can change by such a large factor. The only mention to paleoceano-

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graphic data is page 280: "As a system changes from basal to enhanced states the advective supply increases notably, in the earth this has been reflected by large glacial-interglacial changes in the intensity of the thermohaline circulation (Shackleton et al., 1983; Imbrie et al., 1992; Labeyrie et al., 1992)". Actually, the authors are refering to changes in the deep Atlantic Ocean (below 2000 m). The data suggests that intermediate circulation (were nutrients are more abundant) is probably more active, that many upwelling are also probably intensified in glacial times. The abyssal circulation may not be very relevant for biology. 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.

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.

More generally, the paper is quite difficult to read because the "analogy" brings a lot of confusion. The word "energy" is used instead of carbon or nutrients (but not always). The word "anaerobic" is used for (aerobic) consumption of DOC, and so on.

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

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