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Climate of the Past Discussions

Interactive comment on "A comprehensive, multi-process box-model approach to glacial-interglacial carbon cycling" *by* A. M. de Boer et al.

Anonymous Referee #1

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The article *A comprehensive, multi-process box-model approach to glacial-interglacial carbon cycling* by de Boer et al. shows a very interesting sensitivity study on how parametrisation of ocean circulation and biological productivity in box models have influence on calculated atmospheric CO_2 levels.

In the present form the model misses either some central parts necessary to describe marine carbonate chemistry or the model description is incomplete. So far, the model seems to exist only of fluxes of ocean circulation and biological export production as described in the set of equation on page 873. However, to calculate atmospheric CO_2 concentrations at least temperature information is necessary, as via Henry's Law the

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amount of CO_2 uptake or release is calculated as a fucntion of temperature. A carbon cycle box model normally calculates the concentrations of the three species (CO_2, HCO_3^-, CO_3^{2-}) of the dissolved inorganic carbon (DIC), of alkalinity, and pH as functions of temperature T, salinity S, and pressure p. Apparently, this seems not to happen in the model, or was not described. If it is indeed not happening, then it needs to be explained how atmospheric pCO_2 can be calculated, and how reliable this information then is. If the carbonate system is calculated, it does not need to be explained in detail, some references to the used published system, constants, or equations are enough, but it need to be mentioned what information on T, S, p are used. Furthermore, please be aware that changes in T alone is responsible for one of the three oceanic carbon pumps alone, which partially explains glacial CO_2 uptake: the solubility pump (more CO_2 dissolved in colder waters).

If this major issue above can be solved satisfactory the study seems then to be worth publishing and I have only some small (although a lot) concerns listed below. The most important one is the question of how their model behaves in terms of sensitivity to other box models and the authors might think about some sensitivity experiments as already performed with other models to set their model into a wider context. I am also not sure if the representation of the global ocean as only one row of ocean boxes going from high southern to high northern latitude (thus combing all three ocean basins in similar boxes) is not too simple. The authors make in the final outlook the perspective that Atlantic and Pacific and Indic will be divided in the future, but maybe also some more careful rephrasing about the significance of the resent results might be necessary here. Finally, atmospheric CO_2 of a reference simulation should be stated somewhere, as this would be the point from which all the plotted differences in CO_2 are calculated from. I understand, that this reference state is the average of the 300 best performing (in terms of comparison with World Ocean Atlas PO_4 data) interglacial states. If this is not correct, than it should be explained how a reference state is defined here.

Minor issue:

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- Reference Augustin et al. 2004 is wrong, this is correctly "EPICA Community Members 2004", In the list of authors you have to delete the last "author", Members, E.C., which is another format for the corrected "first author" called EPICA-Community-Members.
- Reference "Luthi et al. 2008 has spelling error, correctly "Lüthi et al. 2008".
- Page 869, line 3: It is not correct, that the ocean takes up atmospheric CO_2 via one of the 3 pumps. Correctly, the three pumps transport CO_2 from the surface to the deep ocean. The ocean takes up atmospheric CO_2 only via gas exchange.
- P 869, l 6: "percentage of nutrients": Which nutrients? The concentration in the surface, or of those preformed via upwelling?
- Reference Fischer et al. 2009 is wrong, this is published in 2010. In this reference, please change the name of one authors from "Kohler" to "Köhler".
- P 869, lines 12-14: "Increased uptake of atmospheric CO_2...". I do not think it works the way as described here. I think the enhanced surface stratification etc lead to a REDUCED upwelling of CO_2 rich waters, thus to a REDUCED outgassing of CO_2 and not the an INCREASED uptake of CO_2.
- Page 869, line 21ff: Given the iron limitation hypothesis, you might cite as one of the basic references the work of Martin (1990).
- p 869, lines 25ff: I am note sure the silicic acid leakage hypothesis is restricted to the equatorial Pacific (should be active in all equatorial oceans), but the way it is written here it is suggested so.
- page 871, line 9, typo: blank after "CO_2" missing
- Model description: Why do you choose to have as fifth flux "northern upwelling flux", and not one describing deep water formation in the north, which is to my

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understanding more one of the principle causes of the global ocean circulation: dense surface waters (cold and saline) which sink down in both the SO and the N Atlantic. You have the related flux in the SO in your setup, but why not in the north?

- Can you insert the shortcuts of you boxes as used as subscript in the Equations on page 873 into both the Tables 1, 2 and the figure 1? This would make orientation a lot easier.
- page 875, line 23: μmol does not need to be explained here, μ for 10^{-6} is part of the SI unit convention, so to say.
- Model performance: What about the general model performance / sensitivity with respect to other box models or GCMs. It would help the reader to set you model into context to others, see for example "model evaluation and sensitivities in Köhler et al. (2005).
- page 875, line 26: Please reformat "Holden et al. (2009; Edwards et al., 2010)" to "Holden et al. (2009) or Edwards et al. (2010)"
- page 877, line 2: "biopump" is a bit sloppy, please rewrite.
- page 877, line 2-3: I can not reveal the numbers you are giving here out of Fig 3 left. If I am in a glacial state, Fig 3 says to rely on the left side, there I find change of pCO_2 of -160 to -200 ppmv (not 100 ppmv (which is without a sign and therefore presumably positive (+)), but preformed nutrients is always below 0.5 μmol/kg (and not 1.6 μmol/kg as started in the text). Something seems to be wrong here.
- page 877, lines 4-6: The way the contribution of other processes on CO_2 (especially on those rising glacial CO_2) are discussed here is incomplete. Not only the terrestrial carbon release during glacial, but also sea level rise contributes to

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pCO_2 in the opposite direction of what is observed in the ice core CO_2 record. Carbonate compensation modulating CO_2 with a delayed response might also be of interest which need to be considered, if other processes are discussed.

See for example, Köhler et al. 2005, GBC (not cited in MS), Brovkin et al. 2007, P (already cited in MS), Sigman and Boyle 2000, N (already cited in MS).

- page 880, line 3: typo in "dependant"? Isn't "dependent" better here?
- Section 4.1 and 4.3 and in the Conclusion: In discussing the effect of shifted winds in the SO and on the stratification please consider also the results of two two papers Tschumi et al. (2008) and Menviel et al. (2008), which showed that the idea of Toggweiler et al. 2006, that northward shifted winds might explain a drop in glacial pCO_2 seems not to be supported.
- page 887, line 3: Change "Vostok and Epica ice core records" to "Vostok and EPICA Dome C ice core records" and give references (Petit et al., 1999, Siegenthaler et al. 2005, Lüthi et al., 2008).
- References: Reference "Siegenthaler et al. 2005" is wrong. The way it is cited you mean the paper in Science in the same year covering CO_2 between 400 and 650 kyr BP, but what is in the reference list is just CO_2 in EPICA DML over the last 1000 years.
- page 893, line 18, typo: change "Mcgee" to "McGee".
- page 892, line 12, typo: change "Muller" to "Müller".
- Table 1 is confusing. As I understand it columns 5 and 6 have nothing to do with columns 1-4. If so, please place those columns (starting with AABW) as additional lines on bottom of the table, and then subdivide the table in 2 parts, one concerning nutrient concentration, the other transport.

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- Fig 2:

  - x axis label: it should read μmol kg-1, (kg NOT with capital K).

  - y axis: The is something wrong here. It can not be named "atmospheric pCO_2" and then go below zero. It is probably "changes in atmospheric pCO_2". Similar thing applies to x2 axis (top x-axis on Fig 3).

  However, throughout the MS only changes in pCO_2 are shown, but never the background pCO_2, from which the changes start from.

  So what is the modelled derived atmospheric pCO_2 for the average preformed nutrient concentration of the best 300 interglacial solutions (similar to your ΔCO_2 = 0 - point in Fig 2)?

- Fig 6: It says "Same as Fig 5", but this should probably mean "Same as Fig 3"? What is the difference between Fig 3 and 6?

References

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Interactive comment on Clim. Past Discuss., 6, 867, 2010.