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Climate of the Past
Discussions

Interactive comment on "A large increase in the carbon inventory of the land biosphere since the Last Glacial Maximum: constraints from multi-proxy data" *by* Aurich Jeltsch-Thömmes et al.

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This ms presents new modeling results of carbon redistribution in an earth system model, focusing on the change in land biosphere carbon storage between LGM and PI. It considers open system processes (i.e., weathering and burial) and uses observations of d13C of atm CO2, d13C of oceanic DIC, atm pCO2, and deep ocean [CO3=] as constraints. Since the seminal work by Shackleton (1977) the change in carbon storage on land over glacial-interglacial time scale has been an important topic in paleoclimatology and global carbon cycle. This ms is thus appropriate for CP readership.

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The extensive simulations and analysis will add to the literature, and I am generally supportive of the study. However, I find the current form of ms requires some effort on the part of the reader to finish reading to the end, because it is quite long, a bit repetitive, and unclear in some places. Consequently this manuscript may not be as impactful as it can be. I make the following comments/questions/suggestions as a way to help improve this ms generally and increase its impact.

1) Overall the ms is long. There is some repetition of information in the Discussion that rehashes the Introduction without necessarily adding value. Effort should be made to shorten not just the Discussion but earlier sections too.

2) Even while the title and Figure 3 try to impress that reader that the authors are investigating the entire deglacial, I do not get a full sense of this. Most of the study is actually focused on differences between two times, LGM and PI (Figs 5-9). There is not a single figure that shows model results of the entire transient deglacial that corresponds to the deglacial forcing shown in Fig 3.

Related to this title-content mismatch is that the authors present Delta (e.g., Dland) as PI minus LGM. I find this to be unnecessarily confusing, because what is changing is the simulation results of LGM. PI remains the same. A main focus is on explaining LGM land carbon storage. In their way of presenting, when LGM land C increases, Dland decreases. As a reader, I have to do that mental calculation and commit what little RAM I have in my brain to storing that information…making reading difficult. For example, as an explanation of Fig 9, I found it a lot easier to understand "a weaker and shallower Atlantic Meridional Overturning Circulation at the LGM compared to the PI" (line 6, p 25) than "the deepening and strengthening of the AMOC during the deglaciation lead to positive Dd13C_DIC changes in the deep Atlantic…" (line 24-26, p 21). They are saying the same but PI-LGM is harder, at least for me.

My preference/suggestion therefore would be that the authors focus on time slice comparisons and present Delta as LGM-PI.

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3) One of the main conclusions is that glacial land C was not larger than PI land C, thus refuting Zimov and Zech... This is a little bit of attacking the straw man, because the notion that glacial land C was larger is highly speculative to begin with, as the authors note (line 21-23, page 2). The more serious and "arguably most reliable" (line 33, page 2) hypothesis of glacial land C is that constrained by C isotopes by Shackleton. It seems to me that the main scientific contribution of this paper is that it credibly revised upwards Shackleton's estimate by considering open system processes, not refuting a straw man argument. I would suggest that the ms be rewritten to make clear what the real scientific contribution is.

4) Why is the control PI run biased high in d13C of DIC? (line 11, page 6)

5) I realize that details of ocean biogeochemistry is not central to this ms, but some more description would have been nice...e.g., how many phytoplankton functional types are there that are limited by P, Fe, and Si? (line 13, page 30)? How much did new production change in the "standard" glacial run compared to PI in terms of Dd13C (Fig 9a) or contribute to the DCO2=27.8 ppm? The authors adjusted the remin depth scale later to modify the impact of new production, but was it that the new production was not sensitive to nutrient supply changes in the standard run?

6) To me, the pulse experiments (section 3.1) are novel and the most interesting part of this study. It shows that neglecting the open system processes, as earlier studies have done, leads to underestimation of Dland. I think though, this section needs more clarification and discussion.

I was confused about the 100 Gt negative pulse emission in a closed system (line 26-29, page 12)...what this is supposed to represent, because in reality 100 Gt does not disappear. It gets redistributed. I think it's supposed to represent the terrestrial uptake, no? If so, it should be stated plainly. Otherwise, you'd have to put it into the ocean (after all, it's a closed system), so that d13C of atm CO2 should increase but d13C of DIC should decrease (not increase as in Fig 4B unless 100 Gt is magically removed or taken up by land).

By the way, I found the inverted y-axis of d13Catm (Fig 4A) to be confusing. Why invert? The reader has to commit that to memory as well.

Why does d13C_atm recover faster than pCO2 (Fig 4A, line 4, page 13)? In standard chemical oceanography, we learn that C isotope equilibration is a lot longer than CO2 chemical equilibration in the surface ocean (roughly 10 yrs. vs. 1 yr).

What is the authors' definition of achieving equilibrium (line 11, page 13)? 60 kyr seems quite long as a time scale of carbonate compensation.

The most interesting part is the open system response of d13C of DIC (line 3-16, page 14). I feel that the main message is a little bit lost in the details. Is another way of putting it that open system damps out the penetration signal of light d13C from the atmosphere into the ocean? So when benthic foraminifera records seawater d13C, its perturbation signal would appear to be much smaller than closed system/actual, and therefore the reconstructed Dland would be underestimated?

I think you mean, compared to the "closed system," not the "open system" in line 14, page 13.

I don't understand "the removal of light terrestrial carbon" in line 5-6, page 14.

I think this whole section should be more carefully worded, perhaps even expanded, so that its important message is up front and clearer.

7) Pages 16-17 make clear that d13C of DIC provides the single most important constraint on Dland, which I believe is what most people in the community thought anyway: "arguably most reliable" (line 33, page 2). So it seems to me that, in the bigger scheme of things, the place of this work in the literature is that it builds on the C isotope-estimated Dland by making the important point that open system processes lead to underestimation. I think that is how the ms should be framed, not refuting straw man argument of Zimov or trying to explain glacial pCO2...

As such the rest of the ms is interesting (especially Figs 7 and 8), but it enters the familiar territory of trying to explain DpCO2$\sim$90 while being consistent with observational constraint. . .concluding that more than one mechanism would be required to explain the full amplitude. I think David Archer was in fact the first to say that it takes multiple mechanisms. . .not the later papers cited in lines 27-28, page 27. There is very good discussion about the individual mechanisms but in the end, this ms does not point to a feasible combination of mechanisms that would solve the glacial CO2 problem satisfactorily. In fact, looking at Figs 7 and 8, I get the sense that these mechanisms in any combination cannot satisfy CO2 and C isotopes targets. For example, observations indicate a larger change in d13C of DIC than d13C of atm CO2, but none of the mechanisms have the right slope (in Fig 8D). Also, some of the findings seem quite obvious: e.g., that CO2 is inversely related to CO3= (lines 7-12, page 19; Fig 7C). . .that seems pretty standard chemical oceanography stuff.

So while Figs 7 and 8 are interesting and illuminating, I feel there is a lot of text devoted to a topic that is rather familiar without reaching a satisfactory solution. I think if the text after section 3.1 is shortened, the novelty of the ms becomes more apparent and ultimately increases its impact.

8) The ms has awkward phrasings/words here and there, which need to be rephrased. The following is not an exhaustive list:

- is “paleo soils” (line 22, p.2) correct? is it “paleosols”

-replace “by” with “to” (line 24, p. 3)

-insert “on land” after “organic material” (line 10, p.10)

-insert “the pulse to” after “apply” (line 27, p. 12)

-insert “marine” before “sediments” (line 10, p. 13)

-last line, p.13 is confusing; rephrase

-replace “recognize” with “represent” and rephrase rest of sentence (line 33, p. 14)

-line starting “Also, it is visible. . .” (line 7, p. 17) is confusing; rephrase

-line starting “In summary. . .” (line 28, p. 17) makes little sense; rephrase

-insert “of” at the end of line 16, p. 19 after “mechanism”

-line starting “Though, the overall. . .” (line 18, p. 19) is awkward; rephrase

-replace “The following” with “Subsequent” on line 23, p. 10

-replace “build” with “built”, line 19, p. 24

-line 30, p. 25: Is “Information in” necessary? Or are the authors inferring from Baldini’s study? Awkward in any case. . .rephrase.

-misspelling of phosphorus, line 8, p. 27

-a brief description of the 4 box veg model might be helpful

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