

Interactive comment on “Carbon burial in deep-sea sediment and implications for oceanic inventories of carbon and alkalinity over the last glacial cycle” by Olivier Cartapanis et al.

L. Skinner (Editor)

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Dear Olivier,

I must first apologise for the time it has taken to process this manuscript. To some extent, this has stemmed from the stamina levels required to read through the weighty manuscript from ‘cover to cover’; it contains a lot! I concur with the reviewers that this is a very interesting and indeed important contribution, though I also concur that this relevance and importance is somewhat obfuscated by the text and its organisation. Of course, your revisions have helped significantly to address this last issue; however, having gone through the manuscript myself a number of times now, I find that there are

C1

still outstanding ‘editorial’ issues that could be improved. I do not wish to delay much further the publication of this study; however, I feel that it will find a wider readership and will make a greater impact if it can be polished a little more. In order to aid in this (i hope), I provide below a list of issues that I think are worth considering. I would encourage you to address all of these comments in a revised manuscript, to be accompanied by a brief commentary (the use of tracked changes as previously will allow you to keep the commentary short).

Once again, thank you for submitting this very interesting study to Climate of the Past, and for your patience in arriving at a final version that I hope will do full justice to the content of your study.

Sincerely, Luke Skinner

Outstanding matters to address or consider:

P.4, line 3: some of these references refer to peat/permafrost carbon, yet you define these reservoirs as being part of the ‘active’ carbon pool, rather than the ‘geological’ carbon pool. Please reconcile, or clarify.

P.4, line 13: Perhaps: “. . .allows for some first order estimates of changes in. . . These estimates suggest that large changes in both carbon and. . .”

P.4, line 18: I think at this point, given the length and complexity of the manuscript, it might be useful to provide a sort of ‘key’ to what follows, e.g. “ In section 2 we provide an overview of. . . In section 3 we present our reconstruction. . . and methods. . . In section 4 we describe some modelling scenarios. . . etc. . .”

P.4, line 24: “These quantitative constraints. . . past changes in the global carbon and alkalinity budgets.”

P.5, line23: shells and shell fragments? Should we describe coccoliths as shells?

P.5, line 26: “. . .with increasing pressure and therefore water depth.”

C2

P.9, line 3: In general, when referring to carbon, please be specific as to organic carbon, carbonate, or both.

P.10, line 10: Are you saying that the most recent rapid sea level drop was at the start of the last deglaciation? Please correct or clarify.

P.10, line 13: there has been a recent proposal that this value might be more positive, closer to arc volcanoes at -3 permil (Mason et al., 2017). Is this worth noting? Does it make a difference to your calculations?

P.11, line 1: please change to 'the glacial' or 'glacials'.

P.11, line 2: ditto. . .

P.11, line 4: Please break up the sentence. E.g. "...although this too has been debated (von Blanckenburg et al., 2015). It is also possible that... lower temperature, offsetting any increase in physical erosion and allowing global erosion to remain relatively stable..."

P.11, line 6: Note also some new perspectives on the glacial weathering and the temperature feedback (e.g. Torres et al., PNAS 2017).

P.12, line 10: I suggest to formulate this as a 'bulleted' list, allowing the intervening sentences: 1)...; 2)...; and 3)

P.12, line 21: assumes.

P.12, line 22: replace nor with or. I think you can cut "which would be expected to alter etc. . ." Perhaps instead at this stage it is worth making a brief note on the complicating effects of a world always out of equilibrium, contra the approach adopted here?

P.14, line 19: "... (no change), while. . ."

P.21, lines 10-21: this whole paragraph is very unclear. Please have a go at clarifying what is meant.

C3

P.22, line 9: On P.18, line 6 it is stated that "we chose to correct for compaction.. by assuming.. global bulk burial rate was equal during the late Holocene and MIS5e. . ." It seems wrong to then 'report' that mean deep sea carbonate burial was not different from modern during MIS5e, or have I missed something? Please correct or clarify.

P.23, line 3: The phrasing here is not optimal I think; please consider re-phrasing.

P.23: Here is a more substantial issue: the 'model' is not adequately described. Indeed, one of the reviewers asked for equations, and this has not been addressed as far as I can tell. The equation that is provided simply aims at describing the general notion of mass balance, though it needs to be corrected since a rate of change of concentration [C] cannot be equal to the difference between two concentrations; different symbols/nomenclature must be used to distinguish [C] from d[C]/dt, and to include the relevance of reservoir size etc... Furthermore, it is important that the air-sea partitioning scheme and shallow/deep partitioning scheme are fully described, and that equations for the rate of change of e.g. [C] in the atmosphere, shallow and deep ocean etc. . . be provided.

P.23, line 16: please consider rephrasing, e.g.: "...we have not considered variable erosion fluxes, due to a paucity of consistent observational constraints on these. . ."

P.24: I think that at this stage in the manuscript it would be useful to provide a short overview of the various scenarios that are considered and why (i.e. of what is to come in the next subsections). E.g. "Next we consider X different model scenarios where we calculate XYZ, on the basis of assumed inputs XYZ etc etc. . . We label/name each of these scenarios KJH, as listed in table 3. . . Our goal with these scenarios is to. . ."

P.25: Is it possible to clearly link each of these sub-sections to the relevant model scenarios, e.g. in the title, or in the first few lines? I think it would be helpful.

P.25, line 12: Please add another word or two to this line to specify why the minimum is a robust outcome of the CaCO₃ burial fluxes. The minimum is close to the minimum

C4

in carbonate/ALK output, which would be counter-intuitive given constant input fluxes, though I suspect that the minimum in ALK in fact occurs just before the minimum in carbonate burial (MIS4) and results from the sustained high/peak outputs leading up to the MIS5/4 transition. Please clarify.

Fig. 7: In this figure (and the others) please identify the vertical shaded bars. Also, it might be useful to clarify that 'net C source' is the net flux of carbon from the geological to the active carbon reservoirs, as defined here. . . if indeed this is correct.

Fig. 7, caption: I think it would be clearer to write: "A. Variable deep sea CaCO₃. B. Variable deep sea dea.." etc.. The same goes for the other figures.

P.26, line 3: "...experiments, the mean.."

P.26, line 4: is it worth noting by how much 500PgC exceeds the modern flux, and that this would imply a downward drift in atmospheric CO₂ across a glacial cycle if it was not actually realised?

P.32, line 8: This is a sentence fragment; please amend. E.g. "Although the preserved. . . (appendix A), our broad spatial. . ."

P.32, line 12: remove 'then'. . . Though I think this could be stated much more clearly with a different construction: e.g. "Absolute changes in CaCO₃ burial were obtained by scaling/multiplying reconstructed relative changes in CaCO₃ MAR to/by the modern MAR. Uncertainties were estimated. . ."

P.32, line 17: is 'very large' necessary?

P.32, line 18: again, was it not assumed a priori that MIS5 CaCO₃ burial was the same as modern/late Holocene? Hence, for example: "on the assumption that it was the same during MIS5 and the late Holocene, CaCO₃ burial dropped to the lowest rates. . .".

P.32, line 23: this is an odd set of citations. Lynch-Stieglitz et al. (2007) is spot on, but

C5

Hain et al. did not present observations, and Guihou et al., is mainly about the glacial inception. There is a wealth of arguments for shoaled NADW during the LGM based on proxy data going back to Duplessy, Curry, Boyle etc. . .

P.33, line 4: "...which would be expected to increase. . ."

P.33, line 7: ". . .in the Atlantic Ocean during the last glacial. . ."

P.33, line 16: please evaluate the plausibility of post depositional dissolution removing the evidence everywhere, otherwise it seems like a throw-away comment.

P.34, line 9: "even more counterintuitive" . . .than what?

P.34, line 14: CO₃²⁻ is only approximately equal to ALK-DIC. Also, throughout the manuscript carbonate ion concentrations should be referred to as [CO₃²⁻], as was suggested by the reviewers I think.

P.35, line 6: would it be more precise to refer to changes in shallow/deep carbon partitioning, or changes in deep sea respired carbon storage?

P.35, line 10: ". . .are of the same order as the [observed. . . simulated??] CO₃²⁻ excursions.."

P.35, line 12: an alternative suggestion: "The discrepancy between inferred and observed [CO₃²⁻] changes across the last glacial cycle emphasises the likely importance of changes in the distribution of carbon within the ocean. . ."

P.36, line 7: As proposed by a reviewer, please avoid starting a new sentence and subsection with "Yet. . .". Indeed, it might be better to write: "Even if the climatic impact. . ."

P.37, line 27: ". . .during the Holocene. . . compared to the glacial.."

P.38, line 26: ". . .unquestionably would have increased the 'active' carbon..." Incidentally, I wonder, is there a 'non-active' ALK inventory? A question of definition no doubt.

P.38, line 27: ". . .followed by the reduction of these inventories. . . and rapid shelf burial

C6

resumed.”

P.41, line 2: “Each record may be biased relative to its local”? What does this mean?

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