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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.

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Response to interactive comments on "Carbon burial in deep-sea sediment and implications for oceanic inventories of carbon and alkalinity over the last glacial cycle" (cp-2018-49) by A. Schmittner (Referee ; https://doi.org/10.5194/cp-2018-49-RC1).

The authors reconstruct changes in global CaCO3 burial fluxes through the last glacial cycle using a large number of ocean sediment core data and evaluate its effects on global DIC and alkalinity changes and on d13C. They also evaluate effects of shelf burial of CaCO3 and organic carbon by reducing the fluxes proportional to the shelves surface area. They conclude both changes in deep ocean and shelf burial did poten-

tially affect global DIC, ALK and d13C. I think this is a very nice paper with important implications for the understanding of glacial-interglacial changes in the carbon cycle. It is very well written (except for a few typos) and nicely illustrated and it is a great contribution to CP. In my opinion the most important contribution of this paper is a quantitative reconstruction of deep ocean CaCO3 burial fluxes. However, the effects of these on deep ocean DIC, ALK and d13C are relatively small (Fig. 8A). On the other hand, shelf burial would have larger effects on DIC, ALK and d13C (Fig. 8B) but it is not reconstructed based on data but rather based on the assumption of fluxes proportional to shelf surface area. I wonder how good this assumption is. E.g. I could imagine that most burial happens on the inner shelf and is not distributed equally across the shelf area (defined as depth < 100 m). If this is the case, then the burial fluxes may not have decreased that dramatically. I don't know if what I think is correct, but it seems to me that the shelf burial changes are more uncertain and less constrained by observations than the deep burial changes. Perhaps the authors want to entertain this thought in their discussion. Also, I think recent papers by Wallmann and collaborators already addressed the effects of sea level changes in shelf burial and d13C. This should be acknowledged.

-We thank the reviewer for his supportive comments on the quality and importance of our manuscript. It is absolutely true that the burial on the shelves remains relatively poorly constrained in the present and in the past, despite its potential importance in balancing the alkalinity budget of the ocean. For that reason, we used a very simple but illustrative scenario based on the commonly-assumed reduction of shelves burial during glacial times, but take into account the uncertainties in modern burial based on published literature.

-We recognize that the uncertainties affecting this component of the global flux did not come across strongly enough in the original manuscript, and will emphasize them in the revised manuscript, pointing out the necessity to address this knowledge gap. The Wallmann papers will be cited more prominently in the appropriate section of the manuscript.

-We are also thankful for the corrections of few typos, which have been corrected in the final manuscript.

P1, L15: "... removal rates, which mainly occurs in marine sediments" I think the correct syntax should be "occur". However, I'm not sure this statement is correct. If the "active" carbon inventory include terrestrial soils and vegetation and the "inactive" or "geological" inventory includes permafrost and peats, then the fluxes between the active and inactive land reservoirs may also be important at least during certain periods.

-We agree with this statement and have redefined the 'active pool' to include permafrost and peat (but not marine sediments).

P1, L17-18: I don't think this sentence is supported by the evidence presented. I think what you wanted to say was something like " : : : the reconstruction provides a first order constraint on the effect of changes in deep-sea burial fluxes on carbon and alkalinity inventories over the last glacial cycle." I think this ("the effect of changes in deep-sea burial fluxes on") qualifier is needed because you don't provide constraints on the absolute (total) DIC and ALK changes, just those resulting from changes in burial fluxes.

-We agree with this statement and have reformulated this sentence accordingly.

P1, L21: "active carbon inventory" It is not clear what this is. Does it include ocean sediments? I wonder what you wanted to say with this sentence. If the active inventory includes atmosphere, ocean and land then I think it is not news that it was a dynamic, interactive component of glacial cycles.

-We reformulated this sentence

Fig. 1: Please explain why three arrows are red.

-Red arrows represent alkalinity fluxes. The figure legend has been amended accord-

ingly.

P4, L20: Parenthesis should be after "Milliman"

-Corrected.

Fig. 2: Consider using a color scheme readable to color-blind people (without red or green)

-We will consider it for future publication. We took care to use a perceptually uniform color map, but unfortunately did not sufficiently consider color-blind readers.

P6, L8: The figure says 100-150.

-Corrected.

P6, L13-15: I thought the d13C of buried CaCO3 was close to that of surface DIC assuming that most of the CaCO3 was produced at the surface.

-Yes, we agree. But this is not inconsistent with the quoted sentence.

P6, L30: Parenthesis should be after "Burdige" Corrected.

P7, L7: Parenthesis

-Corrected.

P9, L12: It is claimed here that the province approach is preferable. Has this actually been shown somewhere? Why could it not also be prone to interpolation and extrapolation biases?

-We agree that the phrasing regarding 'interpolation and extrapolation biases' was misleading, as it is true that our approach is also prone to these biases to some degree. However, to us it would appear obvious that the province approach would be preferable, given that it is more likely to take advantage of local covariation, and that we include many different province configurations in order to capture a range of different possible interpolation and extrapolation biases. For example, our 'Oceans' provinces are at such large scale that they are relatively close to a standard interpolation.

-We would therefore propose to change the text in order to say that we believe the province approach is an improvement over straightforward interpolation, and that because it assumes local covariance among oceanographically/geographically similar regions, it is less likely to produce spurious results. Future work could explore the validity of the approach more quantitatively.

P9, L17: Remove parenthesis with Cartapanis. Typo: it should be “assumes” instead of “assume”

-Corrected.

P10, L13: the noaa ftp link was not working

-Thanks for pointing this out – It was impossible to include the link into a PDF document for some reason.

P10, L20, 21: Please report time periods used for Holocene and LGM.

-Done.

P11, L2: What are the provinces?

-We reformulated the sentence

P11, L22: The assumption of constant shallow/deep partitioning is most likely not valid. E.g. the d13C data suggest more DIC in the deep ocean and thus a larger surface to deep DIC gradient during the LGM. What are the consequences of this assumption for the results?

-This assumption could slightly impact the d13C of the carbon bearing compounds formed at the surface, thus the isotopic composition of the flux out of the system. However, the difference is on the order of a few tenths of a permil, which is much smaller than the uncertainty in the organic carbon d13C composition, which may also

change with pCO2 and growth rates. This small effect would not have a significant impact on the results.

Fig. 3: What are the different lines? Labels OCEAN, SEAS, S. L. 1, S. L. 2?

-These lines correspond to the different provinces scenarios as described in figure A1. They will be clarified in the revised caption.

P12, L1-2: The fluxes between land and the ocean-atmosphere are neglected. What could be the consequences of this assumption?

-This is a good point. Our assumption is roughly equivalent to constant fluxes, though we will modify the text in order to add a caveat about this.

P12: Please provide the equations for the mass balance calculations.

-The equation has been provided in the manuscript

Table 2: Why do the numbers not correspond to those in Fig. 1?

-Thanks for pointing this out, the deep TOC was not updated from a prior estimate in the figure: corrected!

Fig. A1: Please include the numbers in the panels. I assume that 1 is the upper left and 4 is the upper right.

-Amended.

P13, L8: In the title please specify with MAR you’re considering. I think it is CaCO3 MAR in the deep ocean, right? As opposed to CaCO3 MAR in the shallow ocean.

-Corrected.

P15, L 7: Please provide definition of MIS5e.

-We have defined MIS5e as the period 119 – 124 ka, according to (Lisiecki and Raymo, 2005).

Fig. 5: Label in panel E has a typo “buk” should be “bulk”

-Fixed.

P18, L9: Syntax: replace “in” (first word) with “of”

-Corrected.

P20, L10: Typo: “in put” should be “input”

-Corrected.

P26, L5: I think the reference to Fig. 1 may be wrong.

-Corrected

P26, L15: Is there evidence for an enhanced soft tissue pump at the start of MIS4?

-Many studies have given evidence for a reduce deep circulation at mis4 which induces stronger soft tissue pump in the sense of more DIC stored at depth, concomitant with clue for enhanced production and export (see details in (Kohfeld and Chase, 2017))

P30, L9: I think the first who has suggested to use deep ocean d13C to reconstruct terrestrial carbon biomass was Shackleton (1977), Carbon-13 in Uvigerina: Tropical rainforest history and the Equatorial Pacific carbonate dissolution cycles, in The Fate of Fossil Fuel CO2 in the Oceans, edited by N. R. Andersen and A. Malahoff, pp. 401–427, Plenum, New York.

-Thanks for this, reference added.

P30, L12-14: This is an important conclusion, but it has already been suggested by Wallmann et al., (2016, Clim. Past., page 349). BTW this reference is listed twice in the references section.

-We have added the citation to Wallmann for this conclusion.

P30, L27: Where is this shown? Please refer to figure.

-Amended

P31, L7: Who and how was the quality control done?

-We have added a reference to the description of the quality control in section 3.3, rather than in the conclusion, which refers to (Cartapanis et al., 2016)

P31, L13-14: “due to enhanced soft tissue pump” I think it would be better to remove this attribution since other processes such as disequilibrium, solubility may also have been responsible for the reduction in carbonate burial.

-Good point, we have removed this aspect.

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