Author's response

Dear Professor Joos, dear reviewer(s),

Thank you for your final feedback on our revised manuscript. As we made no further changes to the manuscript, this author's response is limited to presenting the reviewer comment and our reply.

Yours sincerely, Anne Morée and co-authors

Reviewer comment

I have gone through the authors' responses to the my comments from the second review. All in all, the responses appear to be satisfactory. The required additional information (weathering fluxes etc.) have been provided. The $\delta^{13}C$ of the weathering flux at −11 ‰ (from +14‰) is still somewhat peculiar: as mentioned in my previous review, the total sedimentary carbon subject to weathering has an average $\delta^{13}C$ of −5 ‰, wherein the most abundant source are carbonate rocks with a $\delta^{13}C$ of around 1.8 ‰. Perhaps the amount of organically derived weathering DIC is larger than in reality (this would be the case the shelf carbonate sink is not considered). At least −11 ‰ can be more easily explained than +14 ‰. The model apparently included some errors that have now been corrected, making the results more plausible now. I regret that the authors still have not changed their mind about the duration of the perturbation experiments. I am, however, truly disappointed about the fact that even this second major (!) revision is not devoid of its share of confusion. The previous version included in its Supplement Figures S3 and S4:

• Fig. S3 represented the surface ocean ΔpCO2 for the control run;

• Fig. S4 represented the specific air-to-sea exchange flux of CO2 for the control run, the Fast gas and the Slow gas experiments.

These two figures presented several deficiencies:

1. inconsistent colour scales

2. physically incompatible results

According to the "Author's response", the incompatibility between the results (deficiency (2)) was due to a plotting error during the production of Fig. S3. Regarding deficiency (1), we read on the 12th page of the authors' response (page numbers in the authors' response would have been the reviewers best friend . . .) that "We corrected this in the new Fig. S4 so that both figures now have the first positive interval in green." Unfortunately

• the revised Supplement contains only one figure with these informations (which other one could possibly be the second of "both" having the "first positive interval in green"?);

• this is the new Fig. S4, which represents the surface ocean ΔpCO2 for the control run (formerly shown on the former Fig. S3) and the Fast gas and the Slow gas experiments.

So, the new Fig. S4 actually includes the old Fig. S3 as its left panel and has equivalent panels added for the two perturbation experiments. The old Fig. S4 has been discarded. At least the old Fig. S4 (specific CO2 exchange rates) and the new Fig. S4 (ΔpCO2) are compatible (at first sight), but to check this, one has to compare graphs carrying different information in different revisions of the Supplement. None of these comments is meant to be a showstopper and I am ready to give green light for the publication. I leave it to the editor to decide on whether the inconsistency between the figures in the Supplement and the Author's response needs correction or not.

Author's reply

Thank you again for your time and effort spent on our manuscript.

Regarding the $\delta^{13}C$ of the weathering flux, we do not consider fractionation during $CaCO_3$ formation, so a direct comparison with natural riverine $\delta^{13}C$ of DIC is not possible. As $\delta^{13}C$ of $CaCO_3$ is less depleted than $\delta^{13}C$ of organic matter, one would expect a more depleted signature (−11 ‰) in our model setup as compared to actual total sedimentary carbon (which the reviewer reports a typical $\delta^{13}C$ signature for of −5 ‰).

We apologise for the remaining confusion about Figures S3 and S4. About the colour scales, 'both figures with a first positive interval in green' refers to the new Figure S4 (old Fig. S3, the pCO2 difference), which indeed has its first interval in green, as well as the equivalent of the old Fig. S4 (the air-sea fluxes), which we only presented in our 'Author's Response' (on page 12). The Figure provided in our last response on page 12 was provided for exactly that reason of comparison of the old Fig S3 and S4 with the new Fig. S3 and S4, but we decided it could be left out of the final text. As the final SI and manuscript texts and figures are not affected by this, we have chosen to make no further changes to the manuscript.

Manuscript changes

None