

Reviews

We would like to thank the two reviewers of our manuscript for their thorough and careful review. We respond to their comments hereafter point by point and indicate what will be changed/updated in our revised manuscript.

Referee #1 (anonymous)

→ We thank Referee #1 for his/her comments and suggestions for improving our manuscript.

In this manuscript, Planchat et al. compare the representation of ocean alkalinity within a set of 15 marine biogeochemical models, across two generations, from CMIP5 to CMIP6. Besides alkalinity they also compare seawater dissolved inorganic carbon (DIC), as well as sinking fluxes of CaCO₃ and particulate organic carbon. They note an increased global export of CaCO₃, closer to observations, and a strengthened vertical alkalinity gradient.

This is a long and dense manuscript, but remarkably clearly written and structured given the number of results presented. Overall, this is likely going to be a key paper that will be useful to anyone using or interpreting results from CMIP5/6 models, and will pave the way to the next generation of ocean biogeochemical models. I enjoyed reading and reviewing it and praise the authors for putting such an important piece of work together.

One main comment I have after reading this piece is that what has changed within models between both generations is clearly presented in the main text, and as a result, we are left wondering what exactly needs to be done for models to better reproduce observed alkalinity patterns. I reckon that there are many models included in this analysis and that all had unique improvements that were not necessarily well described in the original publications/technical notes, so it is very challenging to attribute the improvement to any process. Section 3.1 does a great job synthesizing the different ways to represent various processes across different model groups; it would be useful to do the same synthesis effort but focusing on changes between CMIP5 and CMIP6. Section 4.1 touches this issue, but is very short, and seems to be summarized by the fact that no major trend emerges with regards to the modelling schemes. Could the authors develop this section further? What could be the main processes behind the improved, stronger alkalinity gradient? Inclusion of aragonite or of better diagenesis modules? Could the authors express their opinions on which processes need to be implemented in priority in the next generation of models?

→ We will work in the revised version of the manuscript to further discuss the possible consequences of the model developments in relation to the carbonate pump concerning the representation of alkalinity and its role in the carbon cycle.

Minor comments:

Abstract:

L31: avoid use of significant for non-statistical meaning

→ This has been corrected.

Introduction:

L87: remove extra "Fig. 1"

→ This has been corrected.

Methods:

L170: define "piControl" and "Historical" experiments

→ Now defined.

L180-181: just to make sure I understand: export at 100m from the 3D fields is in theory the exact same thing than export at 100m from the 2D fields?

→ Exactly, it should be the same, but they were manually calculated from the 3D-fields.

L182-183: why using MIROC models then? Can you precise whether there is no export in those models, or those quantities exist but were not saved and/or made available to you?

→ We included MIROC CMIP5 ESMs, since export data were effectively modeled although they were no longer available. We have added this specification in the text to make it clear.

L194: what are those constants?

→ Here we are referring to the seawater equilibrium constants (K1 and K2 for the CO₂ system for instance). We have added “seawater” before “equilibrium constant” to improve clarity. We have not mentioned specifically K1 and K2 however, since we think it can be a bit confusing for the reader without bringing much to the manuscript.

L196-197: are those two models the only ones that include exchanges at the seafloor? Discarding the lower layer would appear justified if the goal was to compare water-column processes amongst models and if only the lower layer was affected by non-water-column (i.e. seafloor). However, seafloor processes, e.g., dissolution or respiration, should also affect water-column chemistry far away from the seafloor. I would like further discussion regarding the role of seafloor processes in the current model intercomparison (see main comment).

→ We disregarded the scarce values given at 5500 m for MIROC ESMs because they were not plausible and likely affected by a bug in model processing prior to these outputs being provided to ESGF.

L203: define SSP and explain the difference with RCP

→ This has been specified.

L230-231: I believe that the Sulpis and Battaglia references are mixed up in this sentence

→ This has been changed accordingly.

L257: what is τ^{5m} ? the concentration of a given tracer τ at 5 m-depth?

→ Yes, it is. We have clarified this in the manuscript.

Results:

Section 3.1.1.: An explicit integration of calcification in models would look like a series of equations used to compute calcification for individual groups, as a function of variables such as light, saturation state, etc. If I understand correctly the implicit integration used by all models skip the production step and computes the PIC export directly, as a function of the same variables (light, saturation, etc.). Because this implicit calcification scheme misses “gut dissolution”, as explained in this section, models using it should all miss the shallow (in the couple hundred meters below the euphotic layer) but strong alkalinity production observed in the ocean, see Feely et al. (2004, DOI: 10.1126/science.1097329), Subhas et al. (2022, <https://doi.org/10.1029/2022GB007388>), Sulpis et al. (2021, <https://doi.org/10.1038/s41561-021-00743-y>). Could you show and/or discuss that somewhere?

→ The reviewer is correct. The export values at 100 m would also possibly be higher because we would not yet be in the zone where gut dissolution would come into play. We will add some elements in the discussion of the paper to address this bias. In particular, it could explain why the models represent only one dissolution peak, at the bottom globally, compared to two in the observational-based studies, with a sub-surface peak in addition (Feely et al., 2004; Sulpis et al., 2021 and Subhas et al. 2022).

Section 3.1.4.: the term “sedimentation” sounds simply like physical accumulation or burial, whereas in this context it should include other sediment transport processes and chemical reactions. Perhaps replace with “diagenesis”?

→ The reviewer is right to point out that we need to make a distinction here between models that take into account sediment processes. Some, like PISCESv2, only distinguish a dissolved and a buried fraction, while others, like COBALTv2, do consider diagenesis by using a sediment module. We have therefore added this clarification in the first paragraph. We will keep the same section title however

because, when we refer to sedimentation in the manuscript, we are only interested in the potential buried fraction of PIC.

L445: please add a reference for an observational estimate of the rain ratio

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Tables:

Table 1 define "MBG"

→ Now defined.

Figures:

Fig. 1 the arrow associated with (3) dissolution should be colored in green because, as for the arrow associated with sediment mobilization, it represents a flux impacting seawater alkalinity

→ This has been corrected. The schematic has also been slightly improved in order to be more consistent regarding the processes affecting alkalinity. We have added arrows for the circulation as well as the N-reactions. We have also modified the carbonate pump part and added the soft tissue pump in parallel to make both of them comparable.

Figure 2 why are the 6th and 8th rows for the MPI models not colored instead of being colored in grey (N/A)?

→ This is because we are still waiting for the input from the MPI developer team regarding this. Each time we show such a table in the manuscript, we have added in the legend "when information is missing, the cell is left blank." to be transparent and improve clarity.

Figures 2 and 6: all CaCO₃ in ACCESS-ESM1-5 is aragonite? Can you please elaborate on that?

→ ACCESS-ESM1-5 shares CaCO₃ as aragonite in its modeled outputs, although it is in fact a generic type of CaCO₃ that is modeled. Concerning this statement, we wrote in Sect. 3.1.1: "Certain groups represent a generic biogenic CaCO₃ (CSIRO with WOMBAT for CMIP6, MIROC with OECO1/2 for CMIP5/6 and MOHC with diat-HadOCC for CMIP5), but attribute it either to calcite or aragonite to conform to CMIP output requirements."