Rebuttal

We thank both reviewers for their positive and globally supportive comments on our manuscript. Both reviewers's suggestions allow us to improve the clarity of the manuscript and the readability of the figures.

Both reviewers have raised the issue that figures in section 4 were difficult to understand because they only have units (e.g., "mol m^{-3}") as labels (variables names were then referred to in the legend). In our revised manuscript, we've added the name of geochemical species and fluxes in all figures. We also agree with the suggestion of Reviewer #1 to plot fluxes going out of the Arctic as negative in Fig. 11, and we have updated the figure accordingly (now Fig. 13). In addition, a new figure (i.e. Fig. 14) has been added to help readers with the understanding of orbital driven processes occurring in the Arctic basin and providing a clearer picture ofArctic oxygen variations .

As for Reviewer #2's comment on "multi-million years", it was meant to refer to the timing of the evolution of geochemical cycles (oxygen and sulfur), rather than the geodynamical evolution of Earth, such as continental drift or oceanic basin opening. These processes will require further model development to be taken into account.

Specific reviewers' comments (written in purple and indented) are addressed hereafter.

Reviewer #1

Main comments:

> The manuscript 'GEOCLIM7, an Earth System Model for multi-million years evolution of the geochemical cycles and climate' by Maffre et al. provides a complete overview and description of the components in the GEOCLIM earth system model. Several new components are outlined in this updated version that allow users to modify the set-up to the desired GCM input fields and/or paleogeography and climate states. Lastly, the authors demonstrate the applicability of GEOCLIM7 with a simulation of the Turonian forced with transient changes to the orbital configuration.
>
> I am very happy to see a concise summary of the GEOCLIM model that, I am sure, will serve as an informative reference for those interested in interpreting as well as running GEOCLIM models. The extensive list of geochemical and physical equations and their parameterizations that form the basis of GEOCLIM provides transparency and clarification. I am also pleased to see an extensive explanation of the methods used for model calibration and tuning. The new components (e.g customizable ocean boxes, flexible GCM-derived forcing fields) are noteworthy improvements, particularly for the construction of paleo-configurations as demonstrated in Section 4.
>
> I am very positive about this manuscript. It is well-written, clearly organized and I support publication after the comments and suggestions below are addressed:
>
> In Sec. 2 (model description) and Sec. 3 (boundary conditions), the authors outline equations and interpolation methods used in the model. I would like to see a clearer indication of what components/equations are new to GEOCLIM7 and what

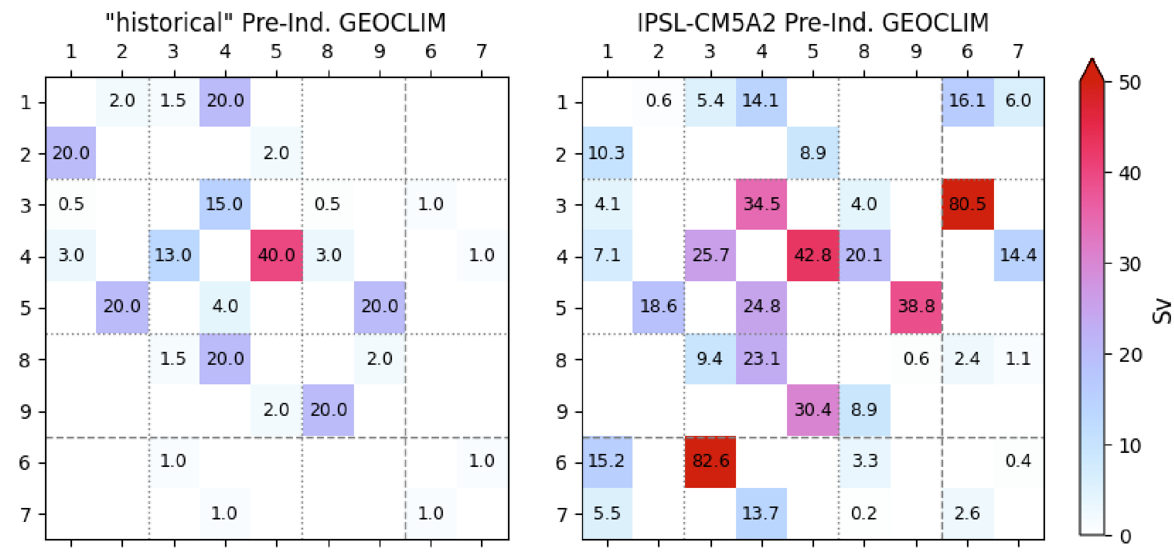
components were previously published, and where. The sections that contain descriptions of pre-existing GEOCLIM equations lack referencing. I recommend adding citations to papers and/or GEOCLIM versions where the components are first described, tested, and applied. Such complete referencing will not only serve as a historical record of GEOCLIM development but also to track the steps that have been taken to validate the model and evaluate its performance.

We agree with the reviewer. In section 2, we have added one or two sentences at the beginning of each sub-sub-section, indicating if the schemes and equations presented are new to GEOCLIM7, and if not, giving the references (lines 151-152, 224, 281-283, 318, 321-325, 350, 372-373, 380-382, 393, 421-423, 474-477, 533, 568, 587-588, 744-746, 757, 773-774).

In Section 3 (boundary conditions), we added this information line 813 (section 3.2), lines 920-921 (section 3.3), and line 958-960 (section 3.4.2). We considered that it was not needed in the other subsections, or was already explicit (as in section 3.4.3).

The flexible ocean boxes are a novel addition in which ocean circulation is obtained from GCM output and converted into exchange fluxes between GEOCLIM boxes using a new tool. Can you show that the tool is indeed able to accurately reconstruct the large-scale ocean circulation, in particular the vertical component (WV) that is indirectly calculated? Perhaps a figure that compares the ocean fluxes in the GCM with that of an older standard GEOCLIM configuration and that using the new version? Would also be useful to reference Section 4 (where the ocean boxes specification is tested) in Section 3.2.

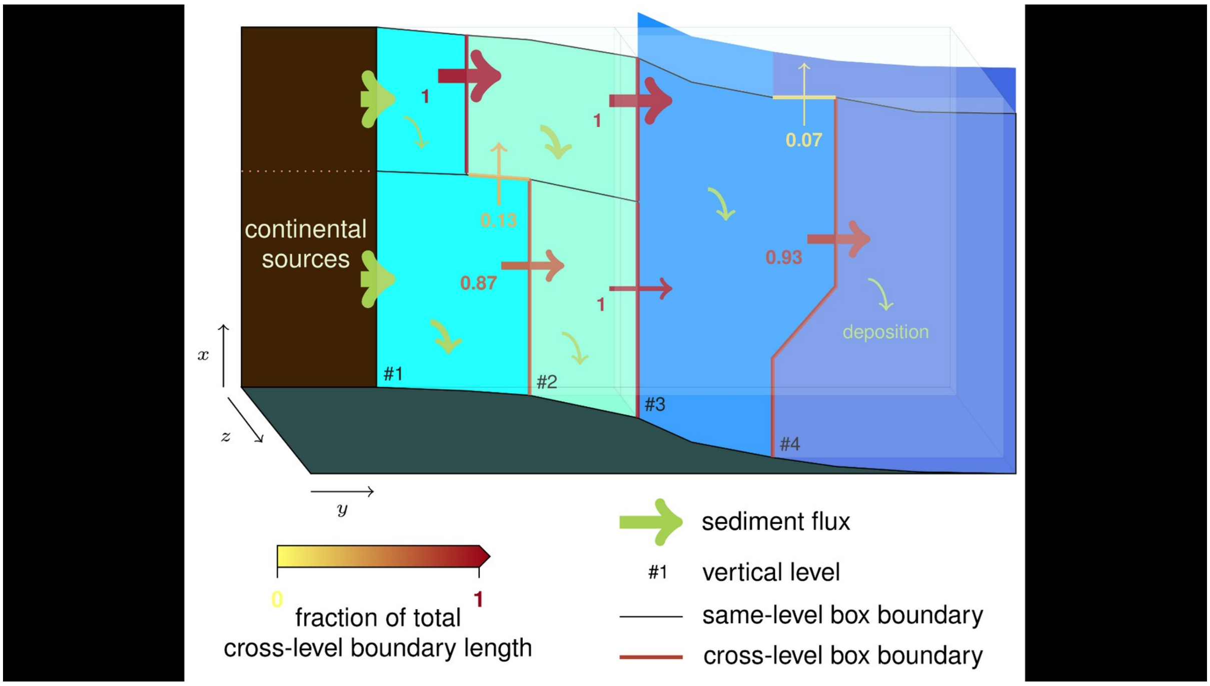
Indeed, the “cutting” of the oceanic boxes within the oceanic grid of IPSL-CLM5A2 needs to be well thought out and is achieved owing to pre-existing general descriptions of the ocean dynamics . In order to answer these specific points, a the following figure has been added in the appendix. It shows a comparison of the default exchange matrix, used in previous GEOCLIM versions, and the exchange matrix computed from IPSL-CLM5A2 outputs. While in the default version, deep water formation intentionally occurred in both hemisphere with the same intensity, one can envision the modern ocean dynamics as calculated by CM5A2 and adapted in the box model. Specific fluxes at deep water formations sites are 10.3 Sv and 8.9 Sv (respectively), which is consistent with the values observed in the IPSL-CLM5A2 pre-industrial simulation. A reference to Appendix C and Section 4.3 was added at the end of Sect. 3.2 (lines 912-914).



Exchange matrices representing oceanic circulation between GEOCLIM boxes. Fluxes are oriented from COLUMN *i* to LINE *j*. The numbering of boxes is the default GEOCLIM one (#1-2: N hemisphere high-latitude (surface and deep), #3-4-5: mid-latitude (surface, intermediate and deep), #6-7: coastal oceans (surface and deep), #8-9: S hemisphere high-latitude). However, the positions of the box pairs {6,7} and {8,9} have been swapped on the figure, to better illustrate the distinction between “open-ocean” and “coastal” boxes.

> Another innovation is the seafloor sediment routing scheme (Sec. 2.3.2). A schematic representation of the routing scheme would be helpful to visualize the mechanics of transport and deposition fluxes, or if this methodology was developed in a different study, cite the appropriate paper. The manuscript is missing a test case scenario that demonstrates the implication of this innovation. How does this improve the model?

A schematic representation of this routing scheme was a good suggestion. We added the following schematics in Sect. 2.3.2. Why undertake this development? In the past, sedimentation rates in various epicontinental and deep basins were fixed parameters, based on typical values from today's world. We aimed to make the model more consistent—the advancements in the weathering model, after all, enable the calculation of continental erosion rates. It then made sense to derive potential sedimentation rates from the continental sediment flux, derivation that was implemented in Maffre et al. (2021). The redistribution model between shallow and pelagic zones was subsequently refined to achieve typical sedimentation rates (10 to 20 cm/ky for epicontinental, 5 to 10 for margins, and around 1 for the deep ocean). The benefit is that we can now observe the impact of orogeny not only on continental weathering but also on marine sedimentation system. The main reason to develop the routing scheme presented in this study was to avoid potentially large biases of the previous routing scheme. Previously, the sediments are spread uniformly to all deeper boxes, regardless of connectivity. This could likely generate erroneous results in cases where one ocean, represented by one column of boxes, is essentially disconnected from the other oceans (e.g., the Arctic basin in 90Ma geography).



Also, aside from the routing from box to box, the sedimentation and early diagenesis computation was already developed by Maffre et al. (2021), which we hinted in the last paragraph of the Section (2.3.2). In addition to this paragraph, we now mention it at the beginning of the Sect. 2.3.2 (lines 474-477), and indicate at the beginning of Sect. 2.3.3 (line 533) that the remaining of the early diagenesis module was unchanged since Maffre et al. (2021).

> If I understand correctly, two tools are designed to (1) convert ocean water exchange fluxes from GCM to GEOCLIM (BC_generator.py), and to (2) generate a river routing scheme (basinmap_editor.py). Great that these are made publicly available along with the GEOCLIM download. The scripts are clear and well annotated, but it is not clear from the manuscript text how one would use these to re-grid their own GCM outputs. As highlighted by the authors, the main improvement of GEOCLIM7 is that boundary conditions from any GCM (not just FOAM) can be used so a step-by-step outline of how users would go about this seems indispensable. For instance, you could provide a readme file or short manual along with these scripts to improve user experience? Or indicate what scripts are needed to perform each step in Figure 3?

The reviewer understanding is correct. Some instructions to generate the specific boundary conditions of the Turonian simulation were already present in the README (Section “HOW TO REPRODUCE THE SIMULATIONS”) of the branch “Mil-90Ma” of Github repository. However, the scripts to generate the slope field and land-to-ocean routing field were missing. We added them in the dedicated branch of the repository, and refer to all of them in the README, in the section that was renamed “HOW TO REPRODUCE THE SIMULATIONS (AND BOUNDARY CONDITIONS)”).

As for Fig. 4 (former Fig. 3), we grouped those scripts in 3 “toolkits” (each toolkit actually contains several Python, or Fortran scripts, to perform one specific task): #1 to convert ocean variables (water exchange AND temperature) in GEOCLIM NC, #2 to generate the river routing map, #3 to generate the slope and lithology fields. Those toolkits now appear on Fig. 4, with more details in figure’s caption. We also refer to them at the beginning of Sect.3 (lines 793-797), where we also mention the template scripts that specifically generate the Turonian BC files.

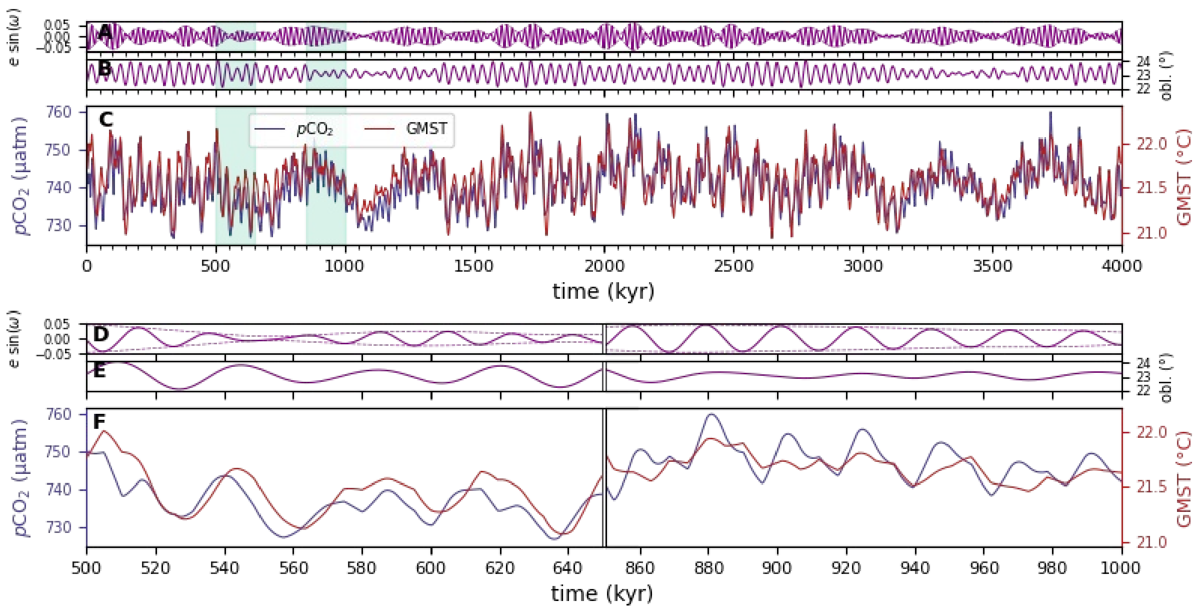
Is the lithological information (Section 3.4.2) a new addition to GEOCLIM7? If it is, this calls for a sensitivity test to check how sensitive results are to choice of lithology, especially important for paleo-configs in which lithology is often unknown. If this is not a new addition of GEOCLIM7, cite the appropriate manuscript in which it was developed and tested.

Lithological information is not a new addition of GEOCLIM7. It has been implemented and tested in “GEOCLIM-DynSoil-steady-state” (i.e., without the ocean-atmosphere geochemistry module) by Park et al. (2020), and already used in “full” GEOCLIM (v5.3) by Maffre et al. (2021), with the exception of strontium isotopic signatures (see our response to line-by-line comment below). We added this information in Sect. 3.4.2 (lines 958-960), and in Sect. 2.2.11 (line 422) to indicate that the lithology-dependent strontium isotopic ratios is a novelty, as well as in Sect. 3.5.4 (lines 1086-1088), to justify why strontium isotopic parameters needed to be tuned (cf line-specific comment of Reviewer #1).

The Turonian experiment nicely demonstrates the applicability of the new model components. However, I’d like to see an explanation or discussion about the multilinear interpolation between climate fields of variable pCO2 and orbits (as mentioned in Section 3.4.3). In particular, because the relation between pCO2 and global climate response is not linear and only two end-member CO2 values are tested. Considering this, it would be useful to know if (and by how much) the pCO2 in the ‘All processes’ experiment changes, i.e. how do modelled variations fall in the range of 560-1120 uatm?

We added the following figure (next page) at the beginning of Sect. 4.5, and added a paragraph to describes how pCO2 and temperature evolves with orbital cycles (lines 1212-1226). Overall, the CO2 variations are small (730-760 µatm), and the global temperature variations a bit larger (+/- 1.25°C).

We also realized that an important information was missing in the manuscript. The multilinear interpolation is performed with respect to the “raw” values of orbital parameters and of “log(pCO2)”, to account for the logarithmic sensitivity of climate to CO2. This information was added in Sect. 2.5 (lines 753-754).



Time-series of orbital parameters (panels A, B, D and F), atmospheric pCO2 and Global Mean Surface Temperature (panels C and F). First 4 Myrs of the 10 Myrs long GEOCLIM simulation with Laskar 95–85 Ma orbital solution, configuration o28’-APx0.25, and CO2 degassing set to 5 Tmol yr−1. Panels D-E-F show a zoom of the two time ranges indicated by the green shading on panels A-B-C, emphasizing the signal from obliquity (left block) and from precession (right block).

> I'm surprised to see how insensitive the system is to orbital forcing in the 'Cont. fluxes' experiment. The authors explain the weak response by the low absolute P weathering flux but this, in my opinion, is an incomplete assessment. Is it possible that orbital changes to the terrestrial P flux are subdued because of the annual averaged surface temperature and runoff fields, unable to capture nonlinear seasonal behaviours of weathering (e.g equations in Sec. 2.4)? Can you address the absence of seasons in the model and how that may impact the orbital results, for weathering but also ocean circulation?

The reviewer raised a point worth of discussion, although it could only be speculative. There is indeed no guarantee that weathering behaves linearly, and that integrated weathering flux throughout the year matches the flux computed with the integrated runoff flux. However, one cannot say if existing non-linearity will increase (convex runoff-weathering relationship) or reduce (concave runoff-weathering relationship) the annually-integrated weathering flux. In other words, if stronger seasonality, at same annual mean runoff, would increase or reduce the weathering flux.

When examining concentration-discharge relationships in time-series of monitored river, a dilution behavior has often been observed (e.g., Ibarra et al., *GCA*, 2016, 10.1016/j.gca.2016.07.006, Ibarra et al., *Acta. Geochim.*, 2017 10.1007/s11631-017-0177-z), pointing toward a concave runoff-weathering relationship. A more complex behavior, with a hysteresis loop throughout the annual cycle has also been observed (Moquet et al., *Environ. Sci. Pollut. Res.*, 2016, 10.1007/s11356-015-5503-6), but not pointing either toward a convex relationship. On the other hand, some studies have argued that stronger seasonality promotes higher weathering fluxes (e.g., Wirchern et al., *Clim. Past*, 2024, 10.5194/cp-20-415-2024; De Vleeschouwer et al., *Nat. Rev. Earth Environ.*, 2024, 10.1038/s43017-023-00505-x).

We added a paragraph about it at the end of Sect . 4.6 "Discussion and limitations of this study" (lines 1323-1338).

As for the oceanic circulation, one must keep in mind that, just like the continental climate field, the velocity fields are computed by a GCM, that represents seasonal cycles (and the intra- and inter- annual variability). The long-term average water fluxes are determined by seasons and variabilities. In GEOCLIM, he mixing fluxes are U*[X] (where U is the average water flux and [X] the concentration of a species); a non-linearity would only exist if U and [X] covary throughout the year. The residence time of all species species is determined by the residence time of water in each box "Vol / Σ U", which, in GEOCLIM simulation, is between 1.5 and 300 years for open-ocean boxes. One should thus not expect significant variations of concentration within a year. In coastal boxes, on the other hand, this water residence time is often less than a year (down to 0.1 year in the smallest boxes). We cannot exclude that a non-linearity exists in the exchange fluxes between coastal and open-ocean boxes. Yet, it is difficult to say how it would affect our results.

> I have also listed additional minor line-by-line comments in the attached file that require attention. Overall, the new model components presented here are a step forward for GEOCLIM. I'm looking forward to seeing future research to the long term biogeochemical cycling in a variety of (paleo)climate settings facilitated by these new

developments.

Line-by-line comments:

I found several typos throughout – see an (incomplete?) list below. Please check carefully. Plus, the list below contains some minor comments/suggestions that require attention.

We have paid attention to proofread the article, and found additional typos that needed correction.

L.82. 'It was one of our objective…' Unclear structure – rephrase

We changed it for "One of our objectives was to find how …" (line 82)

L.107. 'u-inside' → 'inside'

Done (line 107)

L. 127. 'surface'→ 'deep (depth >1000 m)' ?

Indeed, this box is "mid-latitude deep". It has been corrected (line 127). The same mistake was present line 131, and was corrected.

L. 130-131. 'latitude <60°S' latitude >60°S (or latitude <-60°N)

Agreed. We chose to say "latitude > 60°S" (lines 130-131)

Figure 1: Add number to each box to reflect the definitions on previous page

This is a good suggestion. We added it on the revised Figure 1.

L. 160. 'recifal' → reefal?

Indeed. It has been corrected (now line 161)

L. 161. 'force' → 'forced'

Done (now line 162)

L. 193. Should 15/8Fsulw be xFsulw as in Eq.1 and 12?

The factor 15/8 is not related to xFsulw. It comes from the stoichiometry of the sulfide weathering equation:

$(1/2)\ FeS_2 + (15/8)\ O_2 + (7/4)\ H_2O \dashrightarrow (1/2)\ Fe(OH)_3 + 2\ H^+ + SO_4^{2-}$

i.e., for each mole of S oxidated, 15/8 moles of O_2 are consumed. The factor xFsulw represents the fraction of generated H^+ that is neutralized by dissolving silicates (instead of carbonates), in a second reaction step.

In the revised manuscript, we indicated "the factor 15/8 comes from stoichiometry of sulfide weathering reaction" (line 196).

L. 215. Add reference to origin of this equation

There is actually no reference for this equation. This formulation was unchanged from the first published version of GEOCLIM (Godderis & Joachimsky, 2004), whose strontium model comes from François & Walker, *American Journal of Science*, vol. 292(2), pp. 81-135 (1992).

However, neither Godderis & Joachimsky (2004) nor François & Walker (1992) indicated the mathematical functional form of the carbonate burial Sr sink.

In the revised manuscript, we indicated (lines 217-220):

"With $[Sr]_{ref}$ the pre-industrial mean Sr concentration. Eq. 13 should be seen as a tuning equation: given that the proportion of Sr input from the ocean is approximately 72% from silicate weathering and 28% from carbonate weathering, setting the Sr:C ratio of PIC this way ensures that the mean oceanic Sr concentration at which the carbonate burial Sr sink balance the input fluxes is $[Sr]_{ref}$."

> L. 223. Are Fadv and Fsink missing in Eq.15? They are described on L.224-225

Fadv and Fsink are implicitly included in Eq. 15 because they are both based on the same generic form described in Eq. 15 (i.e., $F(X)*(\delta^n X_{flux} - \delta^n X_i)$, cf Eq. 51 and 52. The same remark can be make for Eq. 16. We included that information lines 229-232 and lines 239-240.

> L. 243. '…isotopic ratio THAN the surrounding…'

Done (now line 251)

> L. 266-270. Eq.21-22. Just checking, no isotopic fractionation associated with dissolution and remineralization?

The $\delta^{13}C$ of the new productivity is indeed slightly lighter than the $\delta^{13}C$ of the sedimentary organic carbon. But the difference is small (about 1.5 ‰). It appears that this difference may fluctuate, for instance when non-photosynthetic producers are incorporated within the sediments, such as sulfate-reducing bacteria. Such conditions cannot be simulated within our large scale model. For this reason, the difference between primary productivity and sedimentary organic carbon is assumed to be constant, and is thus incorporated within the biological fractionation.

Similarly, the $\delta^{13}C$ of sedimentary carbonates can be slightly different from the $\delta^{13}C$ of CO_3^{2-}. Again, this is a small effect, and fluctuations cannot be simulated.

Ref: J.M. Hayes, H. Strauss, A.J. Kaufman. The abundance of ^{13}C in marine organic matter and isotopic fractionation in the global biogeochemical cycle of carbon during the last 800 Ma. Chemical Geology, 161, 103-125, 1999.

> L. 276-280. Is there an upper limit to the dependence of primary productivity on P flux/inventory? E.g. in environments where the P inventory is extremely high, is there still a dependence on P input flux?

This equation is an approximation, as only one nutrient (P) is represented in GEOCLIM. Hence, the behavior of phosphorus, in the model, may be viewed as the average behavior of all nutrients. Primary productivity in modern ocean is either limited by nutrient,or by light (at high latitude). Regions with high P inventory are either regions where another nutrient is limiting (which cannot be simulated by GEOCLIM), or high latitudes, limited by light (which is already parameterized in GEOCLIM).

L. 297. ‘dissolved’ → dissolve

Done (now line 308)

L. 351. ‘similarly than’ → ‘similar to’

Done (now line 364)

L. 369. ‘…sinking TO THE seafloor are lost from THE oceanic…’

Done (now lines 384-385)

L. 404. ‘…all other cations than…’ → ‘…all cations other than…’

Done (now line 419)

L. 417-420. Check subscripts. Should $A \rightarrow (i)$ and $(i) \rightarrow A$ be swapped in the text on L.417 and in the equations on L.419-420?

They were indeed swapped in the text. The equations, however, were correctly labelled. This has been corrected (now line 437).

L. 435. ‘… same timestep AS in…’. Also, might be worth mentioning the time stepping here or earlier instead of waiting until Section 3.

Corrected (now line 454). Rather that mentioning the timestep here, we decided to move that information to Sect. 2.5 (lines 765-766) so that timesteps are solely discussed in Sect. 2.5.

L. 441-442. Unclear sentence, please re-write

We changed the sentence to "‘raining’ fluxes (Frain) are fluxes of settling particles that have just reached the seafloor (while ‘sinking’ fluxes refer to the settling particles that are still in the water column)" (now lines 458-460).

L.443. ‘… elements that have UNDERGONE…’

Done (now line 462)

L. 451. ‘…material lost FROM the oceanic…’

Done (now line 470)

L. 455. ‘exceed’ → ‘exceeds’

Done (now line 474)

L. 467. Define ksed

ksed is a parameter, that is listed in Table 3. We do not consider that all parameters must be defined after every equation. In most of the other equations, parameters are not mentioned immediately after.

L.480. ‘… threshold when THE sedimentation flux reachES the…’

Corrected (now lines 501-502)

L. 492. Explain ‘k’ in the subscript. I and j are adjacent boxes, what is ‘k’?

The subscript ‘k’ (in the denominator of the fraction in Eq. 62) is the index of the sum term. The automatic LaTeX formatting within a fraction put that subscript after the Σ term instead of below.

L. 549. Degree of ‘anoxia’ (DOA)?

Corrected (now line 572)

L. 564. ‘…same geographic grid AS the GCM…’

Done (now line 589)

L. 587. ‘… the slope WAS calculated […] and/or PALEONTOLOGICAL data taken from the literature but in GEOCLIM7, a new method is added (see Section 3.4.1)’

These corrections and suggestion were added (now lines 610-612)

L. 617. ‘abondance’ → ‘abundance’

Done (now line 642)

L. 698. So the terrestrial Corg export is independent of terrestrial productivity?

Indeed. This approximation is based on the work from Galy (2015), that identified the erosion and export of bulk material as the limiting process for organic C export. We added a sentence (lines 725-726) to indicate it.

L. 724. What interpolation method is used for the climate fields? Add reference.

Good catch, this information was only given in section 3.4.3 (line 922). We now indicate that it is a multilinear interpolation (line 752) and we added the missing information about log(pCO2) versus pCO2 (lines 753-754).

Table 4 caption. Include meaning of abbreviations and explain that where one value is given, it applies to all lithologies (is that correct?)

The reviewer is correct. Both elements are now added in Table 4 caption.

L. 744. ‘…time interval AS the …’

Done (now line 777)

L. 745. Since you provide a complete overview of GEOCLIM, can you also add information about the model run/computation time along with the info about time stepping?

Adding information on model run time at the end of Section 2.5 is a good suggestion. We added lines 778-783, and renamed the section “Climate fields, interpolation, numerical solver *and computation time*” (line 743).

In a nutshell, GEOCLIM runs at 30 minutes per Myr in its lowest resolution, and up to 5 hours per Myr at the highest resolution tested here.

L.756. ‘…geographic resolution AS the…’

Done (now line 799)

> L.775. by 'cutting depth', do you mean 'cut-off depth'? If so, please correct here and throughout the rest of the manuscript.

We indeed meant 'cut-off' (depth or latitude). We corrected it (lines 819, 821, 825 and 997)

> L. 788. '…Lij sums up the lengths…'

We would rather changed it for "Lij is the sum OF the lengths of the edges of all grid cells" (now line 832)

> L. 859. 'Finally, THERE are…'

Corrected (now line 903)

> L. 865-866. Reference the origin of these equations.

This "old" parameterization comes from a log(CO_2) fit that was conducted with FOAM pre-industrial temperatures in the first version of GEOCLIM coupled with a GCM (Donnadieu et al., 2006). We indicated it lines 908-909.

> L. 879-880. 'This needS to be manually […] this guess ignores all […] hydrographic networkS that…'

Corrected (now line 927-929)

> L. 906. 'Another' → 'Other'

Done (now line 955)

> L. 907. Define nx and ny.

We indicated that (nx,ny) is the size of the 2-D grid and Nlitho the number of litholigical classes (lines 949-950)

> L. 910. '…categories AS for slope…'

Done (now line 957)

> L. 933. In addition to silicate weathering, and volcanic/anthropogenic CO_2 outgassing, does fossil Corg weathering not contribute to the CO_2 fluxes to the atmosphere?

It does contributes to CO_2 fluxes to the atmosphere, though indirectly as it generates DIC first (see response to Reviewer #2). But the reason why it is absent from this calibration step is because the calibration is meant to achieve a steady-state of all geochemical cycles, which implies that fossil Corg weathering is balanced by Corg burial (with zero net C flux). Therefore, at steady-state, the silicate weathering flux should only balance the magmatic degassing flux.

> Figure 3. Clarify if 'paleo-geology' refers to the lithology or to the geological categories? Make sure to include both in Fig.3

Paleo-geology only refers to the geological categories (each of them would contain a set of different lithological classes). We opted to indicate in Fig 3. caption "We note that paleo-geology (an a priori information, cf Sect. 3.4.1) can also be used to reconstruct the lithology fraction field, but this was not done in this study".

> Figure 4. Can you add the modern mean profiles of these basins so readers can visually compare GEOCLIM pre-industrial output to the modern?

This is a good suggestion. We used the data from GLODAP database and computed their horizontal averages and distributions on the areas of definition of GEOCLIM boxes. Figure 5 (former Fig. 4) now shows the superposition of GEOCLIM profiles and GLODAP data. We also added a paragraph at the end of Sect. 3.5.3 to comment on this data-model comparison (lines 1072-1083).

> L. 996. '…vertical profiles AS another…'

Done (now line 1047)

> L. 1020. Why not include tuning of δ13C? The equations are listed in Section 2.2.3 so it leaves the reader wondering how these are resolved.

Less attention has been paid to isotopic tracers, in this manuscript, as it is not its main focus. The tuning of Sr isotopic cycle was less a purpose than a necessity: with the addition of silicate lithological classes, a strontium isotopic signature has to be assigned to each class. Although the lithological classes were already added by Maffre et al. (2021), they did not update the strontium isotopic signatures in a way consistent with the global value from continental fluxes. The carbon isotopes, on the other hand, are not concerned by this issue since all carbon associated with silicate weathering comes from the atmosphere. All parameters associated with carbon isotopic cycle are unchanged from Godderis & Joachimsky (2004).

We renamed the corresponding section (3.5.4) "Isotopic tracers", and rewrote it as follows (lines 1084-1092):

"Isotopic tracers are not the main interest of this study, and we did not tune the isotopic parameters any further than previously done (Goddéris and Joachimski, 2004; Donnadieu et al., 2006), except for continental strontium isotopic ratios. Indeed, with the implementation of silicate lithological classes (Park et al., 2020; Maffre et al., 2021), it was no longer realistic to only keep the two ratios ("basalt" and "granite") of previous GEOCLIM versions. To achieve a mean Sr isotopic ratio from continental weathering (from both carbonate and silicate) of 0.712, and therefore an oceanic ratio of 0.709 (François and Walker, 1992), we set the isotopic ratios of felsic silicates, intermediate silicates, and siliclastic sediments to (respectively) 0.718, 0.710 and 0.718, while keeping the isotopic ratios of metamorphic silicates, mafic silicates and carbonates to their typical values of (respectively) 0.720, 0.705 and 0.708 (François and Walker, 1992)."

> L. 1110. Has this acceleration technique been published or tested before to yield accurate results? If so, please cite reference. If not, include a comparison between an accelerated and non-accelerated run to demonstrate the accuracy of final results.

The acceleration technique of the geochemical cycles has not been published before, though it was used for all steady-state simulations of Maffre et al. (2021), and likely other GEOCLIM studies.

It simply consists in applying a factor "1/A" before the time derivative in Eq. 7 (atmospheric oxygen) and Eq. 8 (oceanic sulfate). Were "A", the acceleration factor, is typically 30 or 100, being only limited by the stability of the numerical resolution.We argue that it does not require a demonstration of the accuracy, because it is mathematically exact. The steady-state value is not affected by the factor “1/A”, since the time derivative term is null at steady-state, this factor will disappear from the steady-state equation.

As for the regolith inertia, it was described in P. Maffre PhD dissertation (chapter 5). It is built on a similar principle, that reduces the characteristic timescale while mathematically keeping an identical stated-state.

In our revised manuscript, we added a paragraph (lines 1176-1181) to provide this information.

> L.1120. Use ‘approximately’ or ‘~’, not both.

We removed “approximately” (now line 1192)

> L. 1127. Checking Laugie et al. (Fig.5), it seems like panel H yields the best match with Laugie et al. (2021)?

We do not agree with the reviewer. GEOCLIM has a resolution to coarse to simulate strong local anoxia like the vertical profile #5 of Fig. 5 of Laugie et al. (2021). Instead, Fig. 2 of Laugie et al. shows large horizontal variations in oxygen concentrations (GEOCLIM also cannot reproduce the anoxic layer at 700 m, Fig. 5C of Laugie et al., due to its coarse vertical resolution).

On average (horizontally and vertically), the difference of O2 concentration, in Laugie et al.’s simulation, between proto-North Atlantic and Pacific is ~ 100 $mmol/m^3$, which is consistent with panel G, and less consistent with panel H where that difference is ~ 200 $mmol/m^3$.

> Figure 8 caption. ‘…projections AS IN Fig. 5 ….’

Done (now Fig. 9)

> L. 1136. ‘…goal of this paper IS to present…’

Done (now line 1208)

> L. 1139. ‘…presented here start from a steady state achieved in a similar manner as explained in section 4.4…’

Corrected (now line 1212)

> L. 1164. ‘… time ranges AS in…’

Corrected (now line 1250)

> L. 1169. ‘…surface O2 variations (~8 mmol/m3) are much smaller than the deep variations (~XX mmol/m3).’

We agreed with that suggestion (added line 1255). Surface variations are ~8 mmol/m3, deep ones are ~100 mmol/m3.

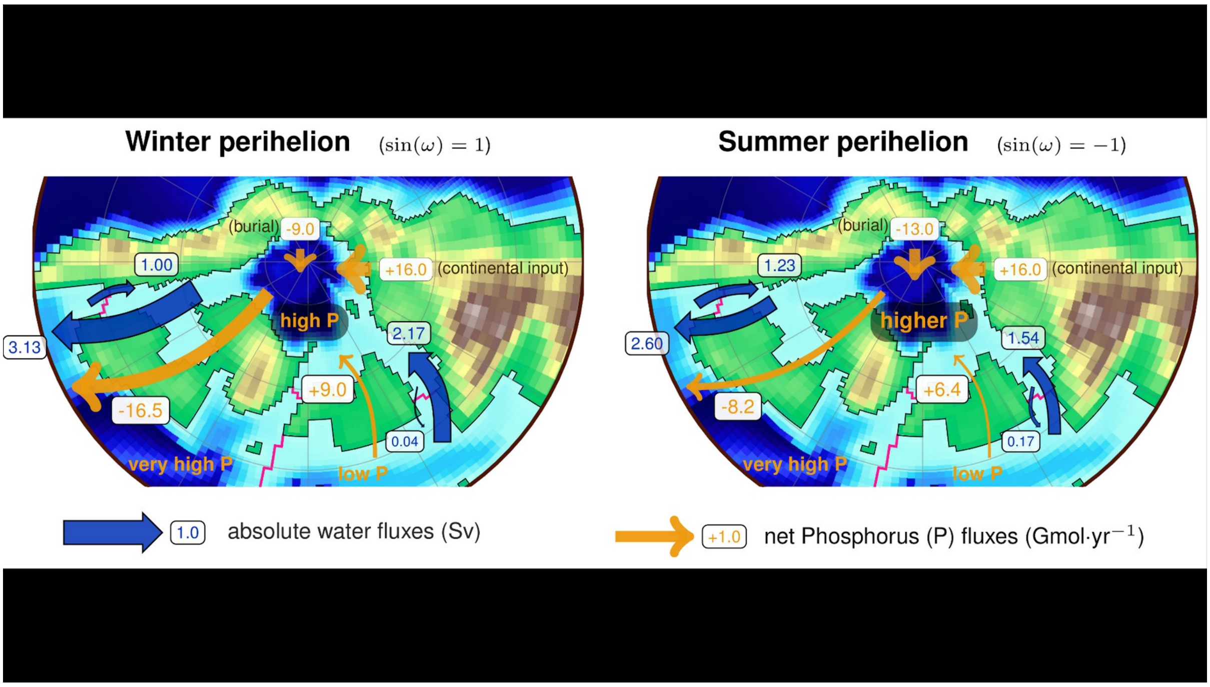
> Figure 11. It is difficult to interpret the results in this figure because the reader has to switch between the caption and the subpanels to figure out the variables that are plotted. Can you add the variable names to each subpanel to improve legibility? Plus, both the influx and outflux of an Arctic box are plotted as positive values. It would be more intuitive to plot influxes as ‘positive’ and outflux as ‘negative’ to reflex the result

of the ‘net’ flux.

See our comment at the beginning of present document.

L. 1183. Evacuate → ‘eliminate’ or ‘confirm’? This entire paragraph (L.1180-1205) needs some re-writing to present a clearer explanation, it is difficult to follow in the current state, partially because Fig.11 is not immediately intuitive. Perhaps a schematic of the proposed mechanism that drives Arctic [O2] changes can help, showing changing P fluxes in and out the Arctic boxes.

We rewrote this paragraph (lines 1266-1296). Fig. 13 (former Fig. 11) was reorganized in more intuitive “groups” (concentrations, local P fluxes, water budget, P budget), with more explicit axis legends, indicating wich fluxes are drawn on each panels, and plotting incoming/outgoing fluxes as positive/negative (see other reviewers’ comments). We also added the following schematics (Fig. 14) to synthesize the changes in water and P fluxes responsible for oxygen variations. We hope that our analysis is now easier to follow.



L. 1214. (des)oxygenation → (de)oxygenation

Done (now line 1305)

L.1225. ‘…not be the same AS in PISCES…’

Corrected (now line 1316)

L. 1227. ‘totally inexistent’ → nonexistent

We preferred to spell it “non-existent” (now lines 1317-1318)

Reviewer #2

This paper presents a technical description of the latest GEOCLIM model. It demonstrates the model calibration for present day and explores orbital-timescale changes during the Cretaceous.

The paper is informative and generally well explained. It will certainly be useful as an accompaniment to shorter form papers presenting specific tests of this model.

I found no major issues in the paper, but the figures for the orbital-timescale runs were difficult to interpret as they lacked y axis labels. I also thought it was a little disappointing that the title mentions multi-million year evolution but we do not see any results for multi-million year dynamics, or explore the effects of changing ocean basins over deep time on the model chemistry. But presumably these tests will come in future papers.

I found the following corrections or questions when reading through:

Line 6: suggest to give an idea of the timescales here

We indicated "(several Myrs)" at line 6.

Line 35: suggest "nondimensional" rather than "low-resolution"

Good suggestion. We replaced it (line 35)

Line 106: typo "u-inside"

Done (line 107)

Line 127 and 131: both should be "deep" rather than "surface"?

Indeed. It has been corrected (line 127 and 131).

Line 164: "sulw" should be "silw"?

"sulw" is the correct subscript, it stands for "sulfide weathering".

Line 174 and 194: "Ffocw" - organic carbon weathering – is not included in the CO_2 mass balance but should produce CO_2? Some further explanation of the oxygen cycle mass balance would also be useful, e.g. how it differs from the GEOCARBSULF.

Indeed, fossil organic carbon weathering is assumed to generate DIC, but not directly gaseous CO_2. It is therefore included in Eq. (1) (line 163), but not Eq. (2). The idea behind this representation is that this reaction occurs in river, or in water-saturated soils. Degassing of CO_2 is delayed, and occurs when surface oceanic boxes exchanges with the atmosphere, as GEOCLIM does not represent rivers: continental fluxes are instantaneously transferred to the ocean.

Line 295: "This 10-fold reduction in coastal boxes was tuned in order to avoid massive precipitation of carbonates in coastal surface boxes". Does this hint that the formulation is not very accurate in the first place? Why is the initial prediction so much higher than in the real world?

The reviewer is probably right that the formulation is not very accurate in the first place. Setting a global, uniform fraction of carbonate producers is a simplification. Although it may be true that the fraction of carbonate producers (excluding reef bioconstructors) is indeed lower in coastal, estuarial and epicontinental environments than in pelagic environments, there are also some clues that GEOCLIM is missing some processes. Sulpis et al., *Nat. Geosc.* (2021, 10.1038/s41561-021-00743-y) showed that a significant part of carbonate particles dissolve in shallow waters, above the aragonite lysocline, driven by metabolic CO2 production within marine organisms or aggregates. A process that is ignored in GEOCLIM. That same study also showed that a large fraction of “deep” carbonate dissolution occurs at water-sediment interface. In GEOCLIM, all carbonate particles reaching seafloor are assumed to be preserved and buried. Carbonate is not tracked by the sediment advection scheme, the way organic carbon is. Therefore, GEOCLIM cannot represent the dissolution of carbonate particles produced in coastal environments, but exported and deposited in deep ocean seafloor.

In summary, this formulation is an approximation to compensate for processes that are either poorly constrained, or that cannot be represented in the current state of GEOCLIM, and are left for future development.

Line 557: hydrothermal burial is with iron oxides?

Hydrothermal burial is indeed meant to represent the “scavenging of phosphorus by ferric oxyhydroxides formed within hydrothermal systems” (quoting from Goddéris & Joachimsky 2004), process is described with more details in Benitez-Nelson, Earth Sci Rev (2000): 10.1016/S0012-8252(00)00018-0

However, we did not think necessary to include that information, since this representation is highly simplified, and, for tuning purpose, lumped with phosphorite precipitation into a “inorganic phosphorus burial” flux.

Line 617 “abundance”?

Corrected (now line 642)

Line 698. But what does “E” represent here?

We agree that it is clearer to remind here that it represent the erosion rate, and have done so (now line 724)

Line 939 “to reproduce” typo

Corrected (now line 990)

Line 978: suggest replace GEOCLIM’s with GEOCLIM

Done (now line 1029)

Figure 4: it would be nice to have labels on the subplots saying which tracer they are

This figure (now Fig. 5) has been largely modified, following other reviewer comments. Its axis labels now indicate which species are plotted, and in which boxes.

Line 1007: suggest "found it necessary"

We agree with the suggestion (now line 1058)

Line 1010: suggest replace "free" with "behave freely"

We replaced it by 'behave "freely"' (now line 1062)

Line 1122: repetition of the model names could be removed here

We replaced "performed with the Earth System model IPSL-CM5A2 including the marine biochemistry module PISCES" by "performed with IPSL-CM5A2 and PISCES" (now line 1194)