Review of “Sediment fluxes dominate glacial-interglacial changes in ocean carbon inventory: results from factorial simulations over the past 780,000 years” by Adloff et al.

In this study, the authors use the Earth System Model of Intermediate Complexity Bern3D to simulate the carbon cycle dynamics of the last 800,000 years. They present multiple simulations, with or without interactive sediments, and with a diversity of simplified physical and/or biogeochemical forcings. Their results are evaluated using multiple proxy reconstructions. Thanks to this approach, they are able to assess to some extent the “potential and plausibility of major contributions” (L197) of several processes to the observed glacial-interglacial atmospheric CO_2 change, and notably the importance of the sediment fluxes. As such, it should be recognized that this study tackles an outstanding scientific question, and provides and analyzes long transient simulations (albeit with simplified forcings) of coupled climate-carbon variations during glacial-interglacial cycles, which are infamously difficult to simulate, especially considering an open carbon system.
I find the manuscript structured and well-written, although some clarifications are occasionnaly needed. Hopefully, some of my suggestions in the specific comments will be of use to the authors in this respect. Still, I would say that the main flaw of this ambitious study is that it is attempting too many things at once. A sign of that is that the main text is about 36 pages long, with additional analysis being provided in the first 14 pages of the Supplementary (before extra figures are included). As a result, and despite the clear language, it is often a dense and long read (some would even say tedious), with the reader having to navigate the large amount of information presented in the main text, in the main figures, in the Supplementary... which results from the great number of simulations and analyzed variables presented. In particular, it seems to me that there are instances in which the information contained in the Supplementary is critical to understand the simulations results, whereas some sections in the main text do not seem very central to the main messages. I would therefore encourage a reconsideration of the elements presented, and a reselection of some in the main text from/to the Supplementary (or a removal altogether), to a greater or lesser degree. I am providing below a number of suggestions to hopefully help guide the authors in this direction, but other choices from the authors could be welcomed. Considering the manuscript length, this review also ended up rather lengthy.
In summary, I therefore consider that this palaeo-modelling effort is well worthy of publication and that it will be of interest to the Climate of the Past readership, and may especially pave the way for modellers using less computationnaly-efficient models. To my knowledge, it is the first to tackle coupled climate-carbon simulations of multiple glacial-interglacial cycles including an interactive sediment component, and examining the effects of various sensitivity tests using multiproxy analysis. Still, some of the presentation choices are not ideal and are shaping this study into a long, dense and comprehensive paper. I believe that some trimming (ranging from marginal changes to more substantial reorganization) may lead to a more focused, concise and therefore impactful paper.

General comments

1. Making <u>all</u> connections explicit in order to spell out the knowledge gap, and linking it to the methodological choices
The scientific reasoning is most of the time explicit to the reader, but there are a few instances in which all of these connections could be improved. I am pointing them out here.

- L5 : “Yet, it is unclear how much they affected carbon cycling during transient changes of repeated glacial cycles, and what role burial and release of sedimentary organic and inorganic carbon and nutrients played.”. I believe the knowledge gap could be more clearly spelled out, including why you are considering specifically repeating cycles, and the role of sediments. A more explicit link to the

chosen scientific approach (justifying the choice of 1/ using “various” forcings, 2/ for simulations of the “last 780 kyr”) could then be made.

- L45-49 : I think that the knowledge gap could be more clearly spelt out. As it is now constructed, the L45-47 sentence doesn’t really justify the case for including sediments. Maybe you could explain that the increased marine carbon storage in closed atmosphere-ocean system is not sufficient to simulate the low glacial atmospheric CO_2 without carbonate compensation (e.g. Kobayashi et al., 2021), and cannot be of the right amplitude without the inclusion of land processes as well, as carbon is lost in the terrestrial vegetation. Without clearly showing the limitations of previous studies, the “little is know” and “it has long been assumed” formulations are not exactly impactful as a knowledge gap.

- L135-142 and L143-157: The choice of parameters to modify in the “simplified forcings” (wind stress, etc.) is not explicitly linked to the “glacial-interglacial carbon cycle drivers” described in the introduction. The “why” behind all of these simulations is not clear. I think some sentences should be added to make the connection (e.g. “as a modified wind stress significantly influences the deep ocean circulation in our model”) with the mentioned drivers or to other relevant purpose (e.g. Southern Hemisphere westerly winds seems to have varied over glacial-interglacial cycles according to Gray et al., 2023). Alternatively, to keep it short, you could add a column “tested drivers” (e.g. ocean circulation) in Table 1. Also, in L139-142, I feel like it could be (1) mentioned which forcing was expected to behave like which delta and why; (2) acknowledged that this is a strong assumption, not allowing for threshold effects for e.g. nutrients supply or terrestrial carbon release. Finally, it could be acknowledged that your set of simulations do not tackle some of the drivers mentioned in the literature (e.g. sea ice). Likewise, it could be recognized that you are testing out phosphate input and not iron fertilisation effects because – I assume – your model does not include a representation of the iron cycle.

2. Reorganizing elements from/to the Supplementary to make for a more standalone, understandable, and trimmed down main text

- Additional sensitivity tests in L165-169: There are already a great number of simulations. I feel like these additional ones don’t need to be part of the main text as they do not seem to closely relate to your main scientific question and message. Or do they?

- L193-196: It is odd to start the results section by refering to the extensive material presenting in the SI (which almost feels like a companion paper).
One sign is that the reader may struggle to understand the effects of individual forcings (see specific comments on Table 2, Fig. 3…) before seeing the results of individual simulations. In a way, to properly understand the paper, reading the first 14 pages of the Supplementary is necessary. This is where the processes explaining the changes in carbon storage in the different reservoirs are described. Yet it could be argued that at least one of the ΔC graphs in the SI should be part of the main text. Understanding the evolution of the distribution of carbon between the different pools seems important. But it seems that the weathering-burial imbalances are causing some issues at the end of transient runs (esp. SOWI, p.16), are they not?
Another sign is that despite the scientific question on the role of sediments, “a detailed analysis of the sedimentary changes” is part of the SI. I believe that since most readers won’t read the SI, the authors should strive towards a more standalone main text.

- Please consider briefly introducing the proxy reconstructions used in this study in the start of the results sections or in the methods. At the start of the results section when they are already used right off the bat, the reader only knows that many are “available” (L205) and that some were “selected” (L207),

with has no idea how these reconstructions were made, if they are robust, how they can be compared to the model results (thanks to the isotope-enabled feature), and on which basis some were apparently selected…

- L230: The first mention of a figure in SI is numbered S17. We jump to S22 in L234. The numbering of the figures in SI therefore do not seem consistent. This doesn't help the back-and-forth between the main text and the SI which is too often required to understand the main text properly. Despite the amount of materials, some of this back-and-forth could be prevented. For example, for L230-234 the authors could consider only relying on Fig. 3b, or a modified Fig. 3b, making the main text standalone by removing nuances which can only be explained by the extra figures. These few sentences could be kept in the SI, along with Fig. S17 and S18, in a numbered section addressing a specific question (e.g. "What are the lags between CO2 and the forcings?"), with the main text only commenting "lags observed in Fig 1 and 3b depend on the choice of forcings and are more fully explained in section S1 of the SI". Alternatively, the main information of Fig. S17 could be shown within Fig. 1, as a zoom on the last termination.

- Figure 4 and L240-251: It is unclear to me what this whole section on the MBE actually brings to the table except for the message in L250-251. In the introduction, the MBE wasn't exactly part of the main scientific question. While this is interesting, describing the lags between CO2 and the forcing in different sensitivity tests (previous comment), or the maximum amplitude of the interglacials before/after the MBE across different sensitivity tests seem like details when the main text has yet to explain which processes are behind the glacial-interglacial variations in these simulations. I would argue that this whole part could be moved to the SI (as a "please refer to section S2 "How is the MBE simulated?" in the SI for more.")

- L251-261: Some of the processes are here explained. However, they are just described and not *shown*. No figure is refered. Please consider whether some sort of modified Fig. S4 (incl. evolution curves for atmospheric CO2, DIC, alkalinity, CaCO3 deposition, POC deposition, and also the weathering-burial imbalance…) could be of use in the main text for the reader to properly understand the simulations. I was also curious to see organic carbon variations.

- Section 4.2: Fig. 5 is first shown but not described, only briefly refered to L298. The text goes on to explain the importance of the Southern Ocean (not shown, and we are wondering why we are starting by regional changes), then surface pH (shown, but in SI). Why are carbonate shifts described via pH changes and not alkalinity? So far, the link with DIC has not been made explicit. Perhaps the authors should consider transforming Figure 5a into a DIC-ALK diagram. Also, "The spatial patters of marine carbon uptake and release are thus driven by combinations of physical and chemical processes" (L282) are never shown, making the sentence feel out of place and unproven.

- Fig. 14: Is it really essential to include in the main text?

- Please note that L524-542 somehow already feels like a conclusion, and L543-559 like a discussion. Perhaps this should be acknowledged in the subsection title.

- L560-564: Is it really essential to include in the main text?

- Page 32, incl. Fig. 15: Is it really essential to include in the main text?

- L593-598: Is this discussion relevant? Increasing the vertical diffusivity is not what you did, and KGAS has only a minor effect.

- Fig S23-S24 + Fig S36-39: Considering your outcomes of Section 4.2 and the fact the REMI seems to strongly imprint on the direction of the nutrient changes during deglaciation in BGC and ALL, and on the model-data agreement for delta13C, I am wondering whether you have considered a BGC simulation excluding REMI?

3. A couple of reccurent inadequate terms

- In L2 and other instances, it is unclear why the reactive layers of sediments are considered "ocean sediments" whereas the inert layers are part of the "lithosphere". From a geological point of view, this is disputable. Other instances include L48-49, where "marine sediments and the lithosphere" are mentioned as a unique "fourth reservoir", which makes it confusing why this distinction is introduced in the first place.

- L171 and many other instances: The term "simulated process" is often used to designate the simplified forcings prescribed in the simulation. I would be careful with this term. Processes may refer to the mechanisms of the carbon cycle which contribute to explaining glacial-interglacial variations (e.g. changes in water mass distribution, carbonate compensation…). We can achieve process-understanding (to some extent at least) by examining the model's response to different forcings. But prescribed parameters are hardly processes by themselves. Same for e.g. L202 "We investigate the isolated processes."

- L192 and many other instances: I believe that the term "biochemical" refers to molecular biology and that "biogeochemical" should be used.

Specific comments

1. Abstract

- L3 : I think that carbon transfers don't directly result from different "sensitivities" of the carbon reservoirs to the forcing, but result from the different responses to the forcing of the carbon reservoirs, which depend on their sensitivities ?

- L3 : It is unclear what "many of which" refers to.

- L10 : It is unclear what "associated" refers to.

- L10 : The importance of considering "isotopic shifts" is not introduced.

- L12-13 : "In our simulations the ocean inventory changed by 200-1400 GtC and the atmospheric inventory by 1-150 GtC over the last deglaciation." It is unclear which conclusion the reader should draw from these results.

- L18 : It is unclear what "needs to be considered" refers to. Does this refer to isotopic drifts?

2. Introduction

- Introduction : Please consider mentioning the permafrost as a carbon reservoir of relevance.

- L24 : It is a bit unclear which variable is delayed with respect to the others.

- L39 : Although the “enhanced stratification due to brine rejection” is classified in “other physical processes”, stratification closely relates to “changes in ocean circulation” (L34).

- L50 : I suggest changing “on continental shelves” by “considering continental shelves”. “On” is slightly odd as the sentence refers to altered “seawater carbonate chemistry”, but no seawater remains “on” continental shelves when they are emerged.

- L61-63 : A lot of new notions are introduced here, which could be hard to follow for non-specialists. Please consider briefly explaining (1) why carbon isotopes are used in model simulations, (2) what is the burial-nutrient feedback, (3) what you mean by dynamic sedimentary adjustment, and (4) the relevance of imbalances in weathering-burial fluxes when considering an open carbon system. It should also be mentioned whether this imbalances in fluxes refer to carbon fluxes, or also include nutrients. I note that sentences in L65-68 and L69-73 are much more explicit, so it is just the first occurrence of theses processes which I find not easily understandable. A small reorganization of these sentences could therefore be considered. To this end, I should also mention that the L62-64 about equilibration time feels out of place in a paragraph whose main message is to underline the importance of including sediment for its impact on the carbon cycle.

- L74-75 : So far, the main messages that I am getting are “Sediment fluxes are important to consider / they are difficult to simulate”. I think that the long equilibration time (L62-64) and memory effects now mentioned could be used to explicitly make the case for transient simulations of multiple cycles. As of now, the connection with the proposed study (L90-93) is not clearly made, as the main message from paragraph L76-89 seems to be “no one has done it in this way before”.

- L84 : Please consider detailing what “partially” exactly means, especially if it helps make the case for dynamic sediments.

- L90-96. All the originalities of the scientific approach of this study should be justified. Here we have :
    - transient → justified by “to avoid biases resulting from steady state assumptions”, even though it is unclear what those biases exactly are. I think that it is unclear what the precision “so that all carbon stores at the beginning of the last glacial cycle are achieved dynamically rather than being prescribed” brings to the table. You haven’t said that the last glacial cycle is of specific interest and why ?
    - 780 kyr long → not explicitly justified. Why several cycles? Why not shorter/longer?
    - factorial simulations → justified by “to understand how various processes…”
    - “with and without sediments” → justified by “to distinguish the role of interactive sediments”
    - isotope-enabled → it is not said in the introduction that one strength of this study is that it will provide a multiproxy analysis, even though it is part of your main results (e.g. L16).

3. Methods

- It might be worth mentioning whether the model simulations include dynamic land-sea mask and bathymetry change (and hence varying ocean volume).

- L107-108: I don’t understand. Is this to correct a specific bias?

- L118-119: Is this compensation done uniformly at the surface of the ocean? It should also be recognized that weathering isn't explicitly represented in the model, but that the loss to sediments is compensated by an input flux which we assume could represent a source from weathering fluxes brought to the ocean by rivers, thus allowing for an equilibrium of whole-ocean inventories. In this respect, perhaps it is more relevant to speak of "terrestrial solute supply" as in L124.

- L122: "in three stages, sequentially coupling all modules". Please consider briefly mentioning why. Also, it is never clearly said how many years the model was spun-up?

- L125-126: "is prescribed to balance loss to the lithosphere over 50 kyr". I am not sure I understand properly. When is the loss to sediment calculated? Is it a each step over a 50 kyr spin-up? Is it done once at the end of the 50 kyr? Why us the input flux kept constant thereafter and not diagnosed over each cycle or so?

- L133: It should be explained why your approach includes the design of "simplified forcings".

- Table 1: (1) It is never explained why no PO4 simulation without sediments was performed. (2) It is not explained why the forcing values (e.g. -40%, -2.5 W/m², etc.) were chosen. (3) Note that the combination simulations PHY, BGC and ALL, as well as CO2T, have not been explained in the text before reading the Table. (4) It could be interesting for the reader to see the remineralization profiles of BASE compared to REMI.

- Fig. 1: I think that the legend should mention (1) the placement of each subpanel with indications (e.g. top, bottom, or panel numbers), (2) the abbreviations (e.g. RF), (3) that the grey background indicates MIS.

- L143-144: "to achieve an older glacial deep ocean". Please consider mentioning right away that "younger deep water masses" are in disagreement in proxy reconstructions. It is mentioned but later, in L146.

- L148-149: I understand the rationale behind KGAS but considering the results (as I remember them now), it doesn't seem like this simulation bring much to the table…

- L160: "adjust external alkalinity fluxes": is it the same as the "terrestrial solute supply" which is alternatively refered to as "weathering"? Also, I am curious as to how your model is able to effectively restore CO_2 variations: how are you prescribing alkalinity fluxes which results in the right CO_2 change, on a practical level?

- L164 and L165: Both of these sentences somehow feel out of place. Maybe you should explain the rationale behind a different CO_2 in the radiative code and its connection to sensitivity test 1.

- L169 : Why do drifts occur in these simulations specifically?

- L178 : I feel like the interest of these simulations to determine non-linearities could have been justified earlier, in the introduction. As of now, these simulations feel like an afterthought.

- L186-187 : This also feels out of place. It could have been mentioned earlier in the Methods.

4. Results

- Figure 2: (1) The gray shading is barely visible in print format. (2) Please consider explaining or simplifying the data legend (I don't understand "cubic spline Bereiter 2kyr cop"). (3) I don't understand why some simulations are selected to be represented here (e.g. PO4, REMI) and not others (BGC, ALL, etc.). (4) It could be nice to add in a value of the different LGM drawdowns on this figure (as text annotations). (5) Figure 1 would fit better in subsection 4.1 than in the start of the results section, which otherwise contains clarifications of the approach of the study (L196-205).

- L189: Note that differences "their timings" are not explained here but much later in the text.

- L191-192: This seems to be true for all biogeochemical forcings except for LANDC though.

- Table 2: Many comments and questions. (1) Legend could be more precise: "summary model-data comparison" → "Quantified metrics of the carbon cycle according to reconstructions and our set of simulations with sediments". (2) Legend indicates "global preformed nutrient concentration" but the variable in the column is the change of [PO4, *reg*]. (3) Why are you only using carbonate ions reconstructions for the deep Pacific and not other basins? (4) Why are dates and anomalies (esp. "PI – 18 ka") inconsistent with each other and with the legend ("LGM-Holocene") and in L.209? Are the same dates and anomalies used for simulations? (5) Why are the effect of individual forcings presented before/instead of the overall model-data comparison of different simulations? E.g. I can read that fLAND has an opposite effect on pCO2, but I only have a vague idea of the LGM CO2 of the LAND simulation (by adding up fLAND + fBASE). As such, this table isn't exactly a "summary of data-consistency" but rather a "summary of the model response to forcings", isn't it?
Note that I understand the choice on focusing on the model response to a specific forcing, which is justified in L196-205, but it is surprising to start out the results with that, when the reader doesn't know yet which simulation(s) are consistent with proxy reconstructions.

- L212: "non-linearities [are] still small compared to the effect of individual process" → This is arguable, as they seem of the same order of magnitude as fBASE…

- L215-217: Should this be discussed in terms of processes (e.g. physical pump)?
L220: "other processes": other than what? One could for example argue that the right processes are simulated but that the amplitude of their effect is wrong.

- L220-222: Consider justifying the choice of assessing CO2, export production, biological pump, sedimentary fluxes and carbon isotopes. Can't an explicit link be made to what is said in the introduction? It is also unclear why we are examining these variables in this specific order.

- Figure 3. (1) Again, by showing the effect of individual forcings, the reader doesn't really see e.g. the good match of simulation CO2T with the ice core data , and you are forced to add in L225 that the sum of fCO2T and fBASE is what is line with the represented reconstructions. It feels like a first graph should first show the simulation results in terms of glacial-interglacial CO2 variations as well, so that the reader clearly realises that adding up all the physical forcings in PHY is still not enough, whereas combinations of some biogeochemical forcings may produce a signal of the right amplitude. (2) Legend should mention that this is the "effect of individual forcings on atmospheric CO2 changes". (3) Why is it only for the last 5 glacial cycles? (4) "selected factors" : on which basis are the effects of some factors represented and not others? e.g. the timing of LAND is mention in L228 but not

represented. (5) Note that the difference between the two green-yellowish are not very visible on print format.

- L226: “produce the most consistent CO2 difference”: with Fig. 3 only showing factorial effects, the reader can’t see that and only gets the impression that fREMI, fPO4 and fPIPO “produce the largest CO2 difference”, none of them being remotely close to producing a 80-100 ppm drawdown by themselves. Same for “is not necessary produced in our idealised simulations with simplified forcings (Fig. 3b)”: the reader cannot directly visualise this affirmation.

- L238-239: I think that the authors should elaborate on this. Why is this the case? Is this a good thing? What does this imply for models which do not consider imbalances? Alternatively, they could refer to a section in which this is discussed.

- L228: You could refer to Fig. 2.

- L259: You could consider briefly explaining why POC dissolution (and also increased alkalinity) raise atmospheric CO2.

- L261-262: “good match with various features”. This is rather vague. Do you mean in terms of amplitude? Timing?

- L263-265: You could refer back -to the Fig. where this can be observed.

- L266 : Please elaborate. Are you implying “and so we can rely on multiproxy analysis to disentangle the different mechanisms in play”?

- L280 : “This mirrors”. It it unclear what “this” refers to, with REMI described in L278 but PO4 described in L279.
- L283-285: Does this mean that over a long period, we have a rough equilibrium, with total fluxes at zero, and the carbon fluxes which goes in the Southern Ocean compensating the carbon outgassed elsewhere?

- L292: Doesn’t the fact that weathering is kept constant also causes imbalances?

- L294: I don’t understand the causality link (‘consequently’).

- L301-302: I am not sure I understand properly why the DIC/CO2 lag in simulations with sediments causes uncertainties in DIC reconstructions. As for “do not necessarily imply a comparable CO2 drawdown”, isn’t this obvious already when considering the carbon reservoirs on land?

- L305: “additional proxy data”. “additional” with respect to which data? I remember of no proxy data which was used in this section.

- L317-318: It is not obvious to me when observing Fig. 6 that AERO simulation has the best model-data agreement, and I expect it is because it is fAERO and fBASE (to add in mind then) which are represented again… Same for L324 “which is consistent with the reconstructions” (there seems to be many black points in these regions as well)

- L323: This had me wondering where the extra PO4 was put in the PO4 simulation. Is it uniformly distributed at the surface ocean?

- L417: What do you mean by “sensitivities”?

- L422: Note that this is the first occurrence of the formulation “alkalinity nudging” for simulation CO2T. You might want to consider to use consistent terminology in the Methods/Results.

- L435-437: According to model simulations or data?

- Figure 11: Please consider providing quantifications of the model-data agreement such as a RMSE (here and in other instances).

- L484: REMI results are commented but not shown in Fig. 11.

- L524: It is unclear which model-data comparison “previous” is refering to.

- L553: “the bias in simulated global carbon fluxes and reservoir size changes”. Why bias do you mean? Or do you mean “biases” in a general sense?

- L558-559: as well as changes in alkalinity input fluxes, right?

- L565: Could these imbalances in weathering-burial (in terms of carbon, nutrients, alkalinity) be quantified or represented somewhere? Maybe this could lead to a more affirmative sentence than “it seems likely that”.

5. Discussion

- L617: Why “in addition” and not “such as”?

- L635: “Our simulations with increase organic carbon burial”. Consider citing the simulation names again.

- L683: Please explain how. Same for L645.

- L646: “especially those which reproduce the reconstructed increase of organic carbon burial during glacial maxima” → Looking again at Fig. 10, I would say none of the simulations do that…

- L651: “big sedimentary changes”: Does this exclude the fraction lost to inert sediments which is compensated by weathering inputs?

- L652: You could refer to a figure again.

- L655-656: But didn’t Morée et al. perform equilibrium runs? (I could be mistaken, I haven’t checked the paper.)

6. Conclusion

- L673: “other processes must have operated”. It is unclear what “other processes” are refering to. Do you mean physical processes (e.g. circulation changes)? Do you mean “compensating processes”?

- L678: This was not really shown in the text previously.

- L680: Do you mean “ocean carbon inventory” or “ocean inorganic carbon inventory”?

- L691-692: And so, what are the perspectives which can be infered? For example, would you be able to provide recommendation for less computationnaly-efficient models which cannot afford to run 200 kyr?

Technical/small comments

- L2 : Consider putting “atmosphere” last in the list.

- L2 : “… preserved biogeochemical evidence” → “… preserved indirect biogeochemical evidence”?

- L7 : “uncertainty” → “knowledge gap”

- L16 : Consider removing “likely”.

- L21 : “Earth’s carbon cycle” → “the Earth’s carbon cycle” ?

- L32 : Consider quoting Kohfeld and Ridgwell, 2009.

- L33 : “last glacial maxiumum” → “Last Glacial Maximum”

- L37 : “added CO2 back” → “tend to counteract this effect by stimulating CO2 outgassing”

- L42 : “as well as increased nutrient supply” → “as well as increased biological pump from increased nutrient supply”?

- L81 : “combinations” or “combination” ?

- L83 : Why isn’t “shallow water carbonate burial” not included in the list of biogeochemical processes with the others?

- L87 : “models” or “model”?

- L98 : “The Bern 3D 2.0 model”?

- L100 : “41x40” → why not provide the resolution in °?

- L130 : Please consider avoiding mentioning the simulation names before they are properly defined.

- L149 : “Finally” is inadequate as you are not describing the final simulation, merely the last simulation with different physical forcing.

- L152 : “added” → “mimicked”, as it is not this terrestrial sink/source is not explicitly resolved by the model.

- L210 and L211 : “three” → “four”

- L257 : “under” → I think you mean “subsequently to”, since forcings (e.g. PO4 supply) would be close to 0 during interglacials and we are seeing a memory effect.

- L261 : “processes” → “forcings”

- L269 : typo “productivity”

- L270 : “played” → “plays”

- L275 : I may be wrong, but I think “towards” indicates a spatial direction (and may not be adequate for a time direction). Same for L410.

- L278 : “lags behind” typo

- L284 : “40 °S” typo

- L303 : “marine carbon storage” → I think you mean DIC, to exclude organic carbon.

- L315 : invert “the LGM and the Holocene”

- L342 : typo citep not citet

- Figure 10 : Gray shading is barely visible in print format.

- L465 : typo “reproduce”

- L467 : typo “increased”

- Fig. 12 : Dotted light green-yellowish line is barely visible in print format.

- L516 : typo citet no citep

- L550 : “the real processes they represent” → “mimick”

- L661 : missing preposition

- L663 : typo “ice sheets”

- L686 : CO_3^{2-}