Review of Buchanan et al. (2025): “Oceanic enrichment of ammonium and its impacts on phytoplankton community composition under a high-emissions scenario”

Summary

In their study, Buchanan and coauthors use an ocean biogeochemistry model forced by output from a climate model to investigate the impact of changes in nitrogen speciation on phytoplankton community structure. They find a shift towards more ammonium under a future climate, which is accompanied by a shift towards non-diatom phytoplankton and mostly driven by changes in ocean circulation.

Overall, the presentation of the results is clear, and this paper will be a valuable addition to literature assessing the response of phytoplankton to future climate change. Besides the new scientific findings, the authors compiled existing data related to nitrogen cycling from the literature for this paper; all these datasets are already made publicly available by the authors, which is valuable in itself. However, before publication of the manuscript, the presentation of the methods in the main text could be improved in my opinion, to provide readers with all information necessary to understand the results, i.e., by providing a more thorough description of how the model simulates the oceanic nitrogen cycle. Further, the description of the model sensitivity experiments should be revised to enhance clarity on what processes are (not) included in each experiment. I have no doubt that these issues can be addressed by the authors during the revisions, after which the study will be suitable for publication in Biogeosciences.

Please see the detailed explanation of all major and minor points below.

Major comments

My only somewhat major comment concerns the description of the methods, in particular the description of nitrogen cycling in the model:

The authors have made modifications to the model code describing nitrification, which is now a two-step process in their model. They currently fully describe these modifications in the supplementary information, but since any change done to the representation of the nitrogen cycle in their model is of relevance to the study at hand, I suggest adding the changes to the main text for full transparency. I acknowledge that the authors already briefly mention the updates in the main text (L. 96), but I would encourage the authors to reconsider the location of the detailed description of code changes, as bringing those to the main text would give any reader a much better overview of how nitrogen is cycled through their modeled ocean.

Further, I think it would help readers to see the modeled distributions of ammonium and its ratio to total dissolved inorganic nitrogen from the preindustrial control simulation in the main text. Especially in light of the changes the authors have made to the code, I think it would help readers, who aren’t experts on modeling marine nitrogen cycling, to first demonstrate good performance of the new version of the model (by comparison with observations) and to show the baseline state in the main text before any sensitivity simulations or future projections are presented. Currently, the main text only shows relative changes for many properties (see Fig. 2, but also true for other figures), which makes it more difficult (than it has to be) for the reader to quickly evaluate what changes should be considered substantial.

In addition, I think it should be stated in more detail in the method section how biological nitrogen fixation is modeled, how atmospheric nitrogen deposition is treated, and how the two phytoplankton types differ in their affinity for different nitrogen species. Some of this information can be found throughout in the manuscript, but it would be more logical, at least for me, to have all this information presented in the method section for easier findability.

Minor comments:

L. 27: "~~an~~ a potentially underestimated"

L. 30: "fundamental ~~in~~ for the growth"

L. 67/68: Please add some references to support this statement: "numerous studies that showcase […]"

L. 100: How is biological nitrogen fixation parametrized?

L. 101: Can you elaborate on the atmospheric deposition of nitrogen? For example, how do you assume deposition to change in your future experiments?

L. 112: I suggest rephrasing to "forcing the *physical*-biogeochemical *ocean* model with monthly *atmospheric* output […]". Please state the variables you used from the IPSL model to force the ocean model. Also, ocean models are typically forced with atmospheric output at much higher frequency than monthly. Can you comment on why this choice was made and what impact you think this has on the results? What time step was used for the ocean model?

L. 122: For the sensitivity simulations, I suggest being more specific with respect to the model fields that were varied vs. held constant. For example, I assume you varied only all velocity components for the "Phys" experiment, but you could have equally varied only atmospheric wind variables. These two have different implications for what feedbacks are still possible in the ocean, and I therefore suggest stating this explicitly to avoid confusion. Similarly, for the "Warm" experiment, did you let temperature fields vary only within the biogeochemical subroutine to isolate the impact on biological rates or was it allowed to vary for the ocean model as a whole, i.e., thereby also affecting stratification and vertical nutrient supply? Or were atmospheric temperature fields varied? Lastly, for the "OA" experiment, did you hold pH fields constant (so that no feedbacks are possible due to, e.g., changing primary production) or did you vary atmospheric CO2 levels? I think it is important to be as specific as possible here and discuss the implications, so that the reader can better understand what processes are (not) captured in each experiment.

L. 129: This threshold reads somewhat arbitrary to me. Can you explain your reasoning, in particular why you didn't use a definition that I would consider more typical, i.e., defining the euphotic zone based on a light threshold (e.g., 1% of incoming PAR at the surface)? Are the two depths (very) different in your model?

L. 141/142: How often was this necessary? By neglecting nitrite in the equation, isn't the importance of ammonium inflated? Or did you in these cases also neglect nitrite from the model output? This is unclear. Please clarify and elaborate on the expected impacts on the evaluation and the importance of ammonium.

L. 146: In my opinion, “broad agreement” is a very subjective term. Can you provide a little more information in the main text and be more quantitative if possible (see also my general comment above)? Looking at Fig. S3, I am wondering why you didn’t color the points for the model output. As it is, one can quickly see the agreement in magnitudes, but to assess spatial patterns more easily, using the same color code in both panels would help. Maybe reducing the number of colors/regions would help?

L. 179: Was the statistical model also built only with the data from the numerical model?

L. 183: I believe alpha is missing in the equation.

L. 188: Also from a PI-control simulation? Why was only the NH4:DIN ratio from a different model used but not the corresponding iron fields? Without having read the result section yet, I think the motivation for this choice (and its implications) could be better motivated in the method section.

L. 194: Please add an explanation what values below 3 mean.

L. 208-213: To streamline the text, I suggest deleting this part. This should be clear after the method section. Also, in section 2.2, there is no mention of sea ice. Can you check which text portion is correct?

L. 213-215: Please see my major comment above. I think you need to elaborate here and potentially move the evaluation figure to the main text. I think it would help readers who are not familiar with NH4 distributions to see the fields from Fig. S1 in the main text as a reference point for the change plots you show later in the paper.

L. 217: Can you explain what the “+- 6%” is? Spatial variability? Temporal variability?

L. 246: I am not convinced based on what you show that you can make this conclusion so easily. I realize that a quantitative attribution to the individual factors is complicated in fully-coupled simulations, but there might be important feedbacks and compensating effects between the factors that, by design, your relatively simple model setup cannot resolve. As such, I find your conclusion too strong here and suggest rewording. As a first-order test, have you run sensitivity experiments changing two out of the three factors instead of one at a time? This could help support your argument of linearity within the model framework of your study.

L. 258: What do present-day distributions of phytoplankton types look like in the model? How do they compare to observations?

Fig. 3: Please define the contours in the maps in the caption and/or directly in the figure. In addition, “model_control” and “model_compete” should be introduced in the method section.

L. 263: Following my above comment on the linearity, I suggest changing the language here. As it is currently written, this implies that the red, blue, and green curves in Fig. 3b perfectly add up to the black curve – I doubt this is the case.

L. 269: Why “in some ways”?

L. 271: I am not sure I am fully up-to-date, but my impression is that many (most?) ocean biogeochemical models do differentiate between nitrate and ammonium (but not nitrite). I was first wondering this here, but this might be something to mention in the introduction to provide a better context for the reader.

L. 280: In my opinion, this information should already be given in the method section (where you describe the model).

L. 286: Is zooplankton grazing temperature dependent in your model? The method section leaves it unclear which processes are included in the term “biological metabolism”. I think it would be helpful to be more explicit about that (see also comment above).

L. 289: thi~~d~~s

L. 296-304: To me, this belongs in the method section (see also comments above).

L. 307: I am not sure where to look to see the 70%. Is this a global integral? Can you clarify in the text?

L. 308: who~~m~~

L. 365: Which model data are shown? From the preindustrial control?

L. 368: higher affinities than NO3? Please specify.

L. 388-393: Redundant with method section? Suggest deleting or shortening here to focus on the results.

Fig. 5: Could you comment on the difference in shape between the model-based fits in black and the Tara-based ones? As someone who is not too familiar with GAMs, the difference in the shape of the curves is what stands out to me. Looking at the underlying data, it seems like there are relatively fewer data points at the very low end of the x axis and around the 4% value – do you think there is room for data availability to impact the mismatch in the shape of the curves?

L. 463: You have not actually shown the circulation changes anywhere. For completeness, I suggest adding information on changes in the drivers in the supplement.

L. 493: albiet → albeit

L. 496: elaborat~~u~~ing

L. 511: strong → strongly

Text S1: In Eq. 6, I believe it should say $[NO_2^-]$ → $[NO_3^-]$