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Ocean Science
Discussions

Interactive comment on "An explicit estimate of the atmospheric nutrient impact on global oceanic productivity" *by* Stelios Myriokefalitakis et al.

Anonymous Referee #2

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In this manuscript, Myriokefalitakis and coauthors quantify the regional and global effects of atmospheric deposition of nutrients to the ocean, consistently derived from a novel, comprehensive atmospheric chemistry model in conjunction with a framework for ocean biogeochemical cycling and marine productivity. Present day atmospheric deposition of the biologically-essential elements nitrogen (N), phosphorus (P), and iron (Fe) appears to be at a peak largely driven by anthropogenic activity, such as emissions from air and sea transport and land use changes (e.g. biomass burning), which has heterogeneously increased N, P, and Fe sources to the ocean since the preindustrial era. These sources are projected to decrease into the future. Despite significant changes in atmospheric nutrient deposition (around 2x more deposition in the present than the preindustrial, and 10-20% decrease in the future) primary production and

nitrogen fixation rates remained relatively consistent over the entire period, although regional changes are more significant. Inclusion of organic deposition, in addition to inorganic deposition, resulted in a similar magnitude of nutrient, primary production and nitrogen fixation anomalies compared to inorganic deposition alone. This is an interesting topic with a novel approach that merits investigation, and eventual publication in Ocean Sciences.

General points:

While the authors systematically validated their present day simulation against observations and described the effects of their new atmospheric nutrient deposition fields on surface ocean nutrient concentrations, as well as the rates of primary production and nitrogen fixation, I found that the quantity and organization of the material eclipsed crucial results, and that the depth of the analysis that was presented was somewhat limited.

Since the title emphasizes global oceanic productivity I was expecting significantly more discussion about the emergent rates of primary production and nitrogen fixation (currently ∼1 page combined). That biological productivity/nitrogen fixation is relatively stable at the global scale while more significant changes occur regionally implies a compensatory mechanism, which is not really explored. I was looking for more information supported by encompassing and generalizing diagnostics than the numerous supplied maps could provide.

- How does the ratio of the atmospheric supply of nutrients change regionally/globally

- How does the combination of these resources promote or inhibit production vs diazotrophy?

- Are phytoplankton (or diazotrophs) consuming critical resources "upstream" that inhibit "downstream" productivity via scarcity or changing nutrient ratios?

- Are unutilized nutrients (e.g. Southern Ocean iron in the future) transported away from the surface to reemerge elsewhere and stimulate productivity remotely?

- Are there teleconnections associated with regions of enhanced export and enriched deep water nutrients upwelling elsewhere?

- What about silicate (Si) fluxes?

- How did the composition of phytoplankton functional groups (diatoms vs other phytoplankton) change?

- Is production limited by a top-down grazing pressure, or a bottom-up resource limitation?

Some of these issues were touched upon when explaining the counterintuitive higher oceanic P concentrations simulated for the preindustrial era despite lower P deposition, which I found really interesting. There are many “moving parts” associated with this study that some idealized experiments might help disentangle the mechanistic role of atmospheric nutrient deposition on ocean biogeochemistry and production. Perhaps substitution experiments with the newly derived N, P, or Fe deposition singly swapped with remaining “standard” PISCES inputs (or combinations of two substituted out of three).

I appreciated the signposting of the manuscript structure at the end of the introduction, but I thought the paper would benefit from having separate “results” and “discussion” sections, with integrative diagnostics in the former, and more emphasis on explaining the changes in emergent ocean properties and comparisons with previous studies such as Krishnamurthy in the latter. At the moment, the key messages are very much buried within the qualitative/semi-quantitative description of the results. I also think that the model-data comparisons, although reassuring, interrupted the flow of presentation. One could create a new section on model validation, but I would recommend moving the material and figures to Supplementary information. One comparison that may have been really interesting to include is a comparison of the “CTRL” run to a vanilla PISCES

simulation, with the standard atmospheric nutrient fluxes.

Specific comments:

P3, line 20: “no-linearly” typo.

P4, line 13: “It has also been suggested…” citation needed, unless it’s Krishnamurthy et al. (2010) in which case rephrase for clarity.

P6, line 7: “five iterations” I think it would be more precise to say you ran the model for 300 years, repeating the 60 year physical forcing five times. Five iterations could technically imply a spin up of 5x2700s.

P6, line 9: Which versions of WOA and GLODAP did you use (if not WOA2013 and GLODAPv2). How was DIC initialized? Also from GLODAP?

P6, line 25: “no extra optimizations for the iron scavenging parameters” The specific iron cycle configuration is of critical importance to understanding the effect of changing iron input, please can you give more details about this? Did you use particle dependent scavenging? How is organic ligand complexation parameterized (constant or variable ligand concentration)?

P7, line 1-12: Timeseries of the model nutrient sources would clarify how the experiment was run i.e. I think you did one transient run 1651-2100 and analyze the nutrient concentrations/ecosystem response at three 20-year average periods. In addition, it would be great to show the temporal evolution of globally/regionally-averaged nutrient concentrations and emergent diagnostics during this run. This got me thinking about whether the “present day” actually represents the peak in atmospheric nutrient deposition, or if that was earlier (70’s, 80’s or 90’s)? There was no real justification for choosing the 2001-2020 average given.

P7, line 12: Will these datasets be available online for ESM groups to experiment with?

P11, line 18: How do dust and aerosol emissions, that are not considered, vary over the time period in question? I think this is touched upon in the “summary”. . ..

P11, line 26: “cooler water temperatures. . .” caused also by high latitude upwelling?

P12, line 5: “leads to more efficient export. . .” N supply may certainly lead to increased rates of export in nitrate-limited oligotrophic regions, but if the nutrients are drawn down to the same low levels, for example in the gyres, is the export actually more efficient?

P12, line 33: It would be an even more convincing model-data comparison if the authors took advantage of the extensive GEOTRACES iron dataset (https://www.geotraces.org/geotraces-intermediate-data-product-2017/) with ∼6 years of cross calibrated additional data from a concerted international effort.

P13, line 21-25: side note about Redfield ratios might be better placed with the model set up.

P14, line 14: Why does nitrogen fixation decrease?

P14, line 22: “with the projected decrease of the global inorganic nitrogen and iron inputs. . .” Nitrogen fixation should be promoted by lower N:P ratio (i.e. decreased N and increased P) so is the lower fixation rate due to iron limitation? Is it possible to show maps of resource limitation from the model for phytoplankton/diazotrophs, e.g. the limiting terms in Equation 6 in Aumont et al. (2015)?

P15, line 32: “all dissolved organic matter is assumed to be instantaneously remineralized. . .” I think this is incorrect. Equation 32 in Aumont et al. (2015) shows how dissolved inorganic matter (for carbon and other species related by fixed Redfield ratios) is separately modeled as a pool supplied by phytoplankton and zooplankton exudation and remineralized aerobically or anaerobically by bacteria over a timescale of the order of months to years.

P16, line 19: “in contrast to the rather balanced nitrogen fixation rates. . .” a 2% change in primary production also sounds rather balanced to me.

P16, line 20-28: 15-20% increases occur relatively widely in the ocean, so what causes the counterbalancing decline in productivity? Why are the decreases confined to these bands in the Pacific?

P17, line 13-20: Salinity restoring and mixed layer dynamics was never mentioned in the main text, so surprised to see it prominently in the “Summary” section.

Figures: It would be preferable to use a perceptually uniform color palette for the CTRL maps, as opposed to the rainbow/jet colormap currently shown (see here for details: https://blogs.egu.eu/divisions/gd/2017/08/23/the-rainbow-colour-map/, not to mention the accessibility issue surrounding red/green vision deficiency).

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