Ocean Sci. Discuss.,
https://doi.org/10.5194/os-2020-27-RC1, 2020

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 #1

Received and published: 22 May 2020

Myriokefalitakis and colleagues run transient offline simulations with the PISCE ocean ecosystem model forced with output for N, Fe, and P deposition from state-of-the-art atmospheric chemistry models. The forcing fields are interpolated from the available deposition fields for the preindustrial (∼1850) to present to end of 21st century. Changes in other drivers such as increasing atmospheric CO2 and 21st century global warming are not explicitly considered.

The authors investigate changes in surface nutrient concentrations in response to generally strongly increasing deposition over the industrial period and decreasing deposition over this century (their Fig 1). The authors compare the differences between three periods - the past (1851-1870), present (2001-2020) and future (2081-2100) (Fig 2 and 6) and between simulations with and without including organic forms of deposited nu-

trients (Fig. 8). The authors also compare simulated surface nutrient and productivity fields with available observational data (Fig 3-5 and 7).

The authors find that changes in inorganic and organic deposition of nutrients have a small influence on global marine productivity, but with regionally stronger effects. The value of the study is in using detailed deposition fluxes for inorganic and organic forms to assess their roles in influencing surface nutrient concentrations, primary productivity, and nitrogen fixation as simulated by a 3-d ecosystem model. This is a new and valuable contribution.

I have a few points for potential improvements:

(1) The current model setup is not clear to the reviewer and section 2.1 on the model setup needs to be improved for clarity. I guess the PISCES ecosystem model is run off-line with physical transport, T, and S fields taken from a physical ocean only simulation. I guess the ocean only simulation is forced with repeated time-varying surface temperature and salinity fields taken from observational data for years 1948 to 2009. This would include a strong warming over the 61-yr period, followed by a rapid "cooling" and again 61 years of warming, etc. In my opinion section 2.1 needs a rewrite and should be structured in a much better way. The description of the setup of the model used to get the forcing fields (circulation , T, S?) to drive PISCES and the spin up and drift of this part of the model chain should be clearly separated from the setup of PISCES.

(2) Simulated changes in nutrient concentrations and productivity are relatively modest. The question arises whether the difference in simulated surface nutrient concentrations (Fig. 2) and productivity (Fig. 6) between PI, present and future periods are only due to differences in the deposition fields as implied by the manuscript or also influenced by other factors. Namely, model drift and, potentially also very important, differences in the physical fields used to force PISCES between the three period of interests could be responsible for some of the differences. I am not sure and I may be wrong, but I have

the impression that the physical fields used to force PISCES are taken from different nominal years of the ocean only simulations and therefore different ocean circulation fields could explain part of the simulated differences in nutrient concentrations and productivity. I also miss the mentioning of a true control run with constant deposition and identical forcing as the standard transient runs with time-varying deposition. This would allow the authors to correct for drifts and changes related to physical forcing. As modelled changes are relatively small, this appears particularly important.

(3) a) Further, I am wondering whether the four figures with 27 maps used to compare simulated with observed fields are really that relevant for this study. They distract from the other, very nice and important figures. Surface nutrient and productivity fields for PISCES have been compared with observations in earlier studies. These simulated fields result predominantly from physical transport of nutrients within the ocean and from the PISCES model itself, whereas the role of atmospheric deposition is rather marginal. The comparison in these four figures tells us, in my opinion, not much about the topic of this study – atmospheric nutrient deposition. They may be included in an appendix and the corresponding text can, in my view, be drastically shortened.

b) On the other hand, I miss some assessment how changes in deposition influence surface nutrients or productivity regionally. Is this due to local effects/deposition? Or is there an influence of ocean surface transport in bringing deposited material to other regions? I have not firm recommendation, but some analysis would be useful. I could imagine to correlate changes in deposition fields with changes in simulated fields or to run factorial simulations with deposition varying only in certain regions, though this may be too CPU expensive.

c) I miss a figure that shows time series of the prescribed transient evolution of globally averaged deposition of N, P, F, from 1850 to 2100. Potentially one could show in this figure also the share of inorganic and organic forms or of different sources. This figure should also include a time serie(s) representing the evolution of the applied physical forcing (e.g. global mean sea surface temp or SAT). In this way, the reader could quickly

understand how and what is varied in the simulations and this figure would complement the table and current fig. 1

Detailed comments

P1, l9: Immediately when reading the abstract one starts to wonder what kind of physical ocean model is used to power PISCES. Please clarify that PISCES is coupled offline (?) to a forced ocean only simulation.

P1,l9: Please mention how atmospheric CO2 and climate change is included.

P3, l29: Is it correct to say that Fe-containing combustion aerosols are mainly deposited in the Pacific and Southern Ocean? Or do you mean that combustion aerosols play a larger role (compared to dust) in these regions?

P5 l19-22: I do not understand what the authors want to say here. Is PISCES now fully coupled online to NEMO and the EC-Earth ESM or rather forced offline with an OMIP simulation? I think it should read: “The state-of-the art biogeochemistry PISCES model is here run “offline” with prescribed transport and T, and S fields (see sec. 2.1). The version of PISCES implemented within NEMO and the European Earth System Model EC-Earth is used in this study. PISCES simulates the . . .

P6,l5: please specify the physical output used to force PISCES.

P6,l6: which OMIP simulation?

P6, l6: Please specify how the 1948 to 2009 forcing is aligned in the spin-up of PISCES and the transient simulation from 1850 to 2100.

P6, l7: I guess this referes to the spin up for ocean model without PISCES? Please clarify.

P6, l8-l10: again – to which simulation or model does this initialization apply?

P6, l8-l10: I guess there are still substantial drifts in O2,N, Si, P and Alk after such a

short spinup of 300 years only. Please specific how large the drifts are.

P6, l8-10: Is there any initialization of DIC or DOM?

P6: what is the role of global warming/climate change in these simulations?

P6: Has a true control run with constant dust deposition and same physical forcing be applied for the 1850 to 2100 period? The drift in critical variables should be quantified.

P6, l29: I find the labeling of the first simulation as “CTRL” very misleading. For me this is the standard simulation with time-varying deposition forcing. Please select another name for this simulation.

P7, line 5: Here a second spin-up is mentioned. The structure of the section is confusion, switching between a first spin-up (of the ocean model), transient PISCES simulation, a second spin up (for PISCES?) and again transient simulation. Please streamline the structure of section 2.1

P7, line 7: No indication is given how this drift is quantified and for which period it holds and whether this is for the global average or for each horizontal grid cell. Please specify.

P6,p7: Is the physical forcing to PISCES identical for the three periods (past, present , future). If not, what would be the implication of differences in the physical forcing?

Section 2: A figure showing the time series of global mean deposition of P, N, and Fe would be really useful.

Section 2.2 A note how these atmospheric deposition fluxes compare with the riverine input would be helpful.

Section 2.2. A note how these atmospheric deposition fluxes compare with export (or new) production of P, Fe, N (as particulate and dissolved organic forms)

P11, l29: Is production limited by light as suggested here or also be Fe?

P17, line 8: Please state how PISCES is forced and what circulation fields are used

P17, l14-20: Part of this text should be mirrored in section 2 where the model setup is described. For example, the mentioning of salinity restoring comes somewhat at a surprise as it is not clear from section 2 that restoring boundary conditions are applied. Similarly, you talk about a prolongation of the OMIP simulation using an RCP8.5 scenario run. Again, this seems not to be described in the method section. Please provide a complete description of the model setup in section 2.

P20: It would be very useful for the community if the deposition files would be made available, e.g. as netcdf files, to the community. I miss a corresponding data availability statement.

Interactive comment on Ocean Sci. Discuss., https://doi.org/10.5194/os-2020-27, 2020.