We thank the reviewer for the careful reading of the manuscript and the fruitful comments. Please find below our point-by-point replies:

I. General Comments

GC1. 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. 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.

- PISCES model is run off-line for this work, with the physical transport, temperature, and salinity fields taken from a physical ocean NEMO simulation. This NEMO simulation was driven five times (310 years) for the years 1948 to 2009 (Skyllas et al., 2019). For all PISCES offline simulations, we used the same 1-year physical ocean forcing. This one year was calculated as a multi-year daily mean over the 5th iteration 1948-2009 of the 310-year NEMO simulation. This way we removed interannual variability and any long-term trend in the physical forcing but conserved the full seasonal cycle on a daily basis. To avoid confusion, we rewrote Sect. 2 adding further analyses by including separately a description of the forcing data (Sect. 2.1.1) and a description of the PISCES model setup (now Sect. 2.1.2):
  "2.1.1 Physical Ocean forcing
  The dynamical physical outputs used to force PISCES for this study are produced by the physical ocean model NEMO, following the protocol of the OMIP simulation (Ocean Modelling Intercomparison Project; Orr et al., 2017). OMIP aims at harmonizing forcing fields of boundary conditions, as well as validation and analysis procedures among different ocean models. Atmospheric forcing fields are from the CORE II forcing (Coordinated Ocean-ice Reference Experiments - Phase II; Large & Yeager, 2009). CORE II provides a 62-year interannual forcing for the period 1948-2009. The physical model is initialized with gridded observational data from the World Ocean Atlas 2013 and then ran for 310 years by repeating the 62-year CORE II forcing. The necessary physical variables to force the offline biogeochemical model PISCES (see Table S1) are taken from the last 62-year iteration. To avoid, however, any long-term trends from spin up, the multi-year (1948-2009; i.e., the 5th iteration of the 310-year run) mean of daily forcing fields was calculated. The resulting mean 1-year forcing thus contains the mean seasonal cycle and was (repeatedly) applied to drive all simulations with the biogeochemical PISCES offline model. All biogeochemical simulations were initialized and forced with the same physical fields from the average 1-year forcing derived from the OMIP run. Thus, all the PISCES offline simulations are drift-free in physical variables. More details of the OMIP protocol can be found in Orr et al. (2017) and a first validation of the OMIP run is provided by Skyllas et al. (2019)."

"Table S1. Physical forcing fields provided at a daily time step.

Water flux into seawater	*$kg/m^2/s$*
Mixed layer depth	*m*
Surface net downward shortwave flux	*W/m^2*
Wind speed	*m/s*
Ice concentration	*%*

Water flux due to freezing/melting	*kg/m^2/s*
Tracer diffusive fluxes along the bottom boundary layer	*m^3/s*
River runoff	*kg/m^2/s*
Ocean vertical salt diffusivity	*m^2/s*
Horizontal divergence transport	*1/s*
Seawater salinity	*g/kg*
Seawater potential temperature	*°C*
Effective ocean transports	*m^3/s*

GC2. 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.

- We thank the reviewer for attracting our attention to this issue. Indeed, all simulations of this work were performed with the same forcing data. The only difference among the simulations was the atmospheric nutrient inputs to the ocean; thus, the differences in oceanic surface concentrations and productivity between PAST, PRESENT, and FUTURE periods are only due to the respective deposition fields considered by PISCES. We agree with the reviewer that a true control run that would correspond to a simulation with constant PI deposition and identical forcing as the transient simulation with time-varying deposition presented in the paper would be the appropriate way to show that the model drift in our simulations is low. For this, we now include a control run (i.e., with constant preindustrial deposition and identical forcing as the standard transient runs) that used for the drift correction of model results, i.e., the nutrient concentrations and marine productivity fields that are presented in the manuscript. The model drifts are calculated here using the linear detrend operator of the CDO (climate data operators) software. Although the impact is minimal thus without changing at all our conclusion, we have also updated all figures based on the resulted drift corrections and we added the following part in the new Sect. 2.1.2, i.e., "*To account, however, for potential drifts, a control simulation as for STD but using only preindustrial (i.e., the year 1850) atmospheric nutrients (N, P, and Fe) inputs into the global ocean is also performed. For example, Fig. S1 demonstrates that for the main ocean basins the drift is minimal after 1850 and clearly below the signal imposed by the altered nutrient deposition. This holds even for the Southern Ocean where the impact of atmospheric deposition is typically weak due to the absence of neighbored emission sources. Nevertheless, all model results presented in this work have been adjusted by subtracting the drift of the control run from STD.*"

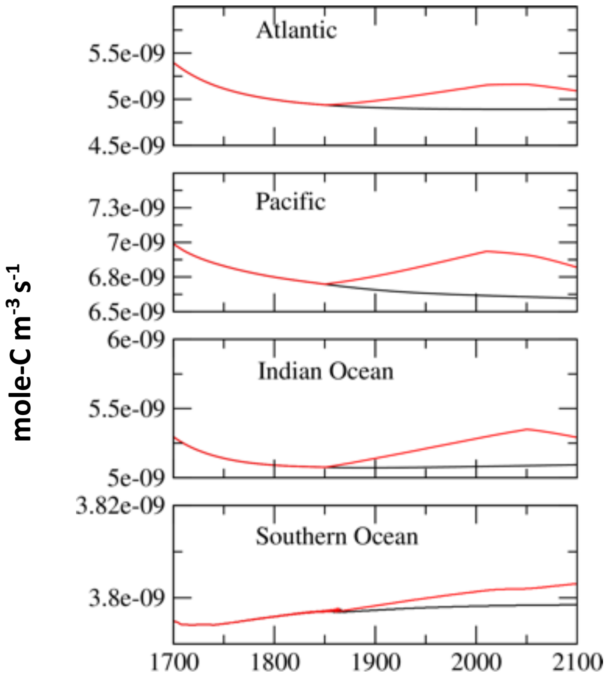


Figure S1: Area averaged annual mean primary production in mole-C m^{-3} s^{-1} for the main ocean basins. Red lines indicated primary production rates for the STD simulation and black lines the CTRL simulation, respectively.

GC3. 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.

- We agree with the reviewer that the model evaluation part may distract the reader from the main conclusions of this work. For this, we now moved all model evaluation figures and the respective discussion to the supplement.

GC4. 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.

- Indeed, to really distinguish the local depositional forcing and the second-order effect of advection of unutilized nutrients on the local productivity would require several additional sensitivity simulations, not currently feasible with available resources. However, we note that in areas where a specific nutrient is not limiting, advection to remote places is highly likely. For the revised version, further analysis has been carried out to investigate and demonstrate this for e.g., the North Pacific in the new section 3.2.1 as well as in the new Discussion Section (i.e., Sect. 5 in the new version of the manuscript). Besides, an additional sensitivity simulation was performed with a constant P deposition, which demonstrated the importance of iron and nitrate compared to phosphorous.

GC5. I miss a figure that shows time series of the prescribed transient evolution of globally averaged deposition of N, P, Fe, from 1850 to 2100. Potentially one could show in this figure also the share of inorganic and organic forms or 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

- We agree with the reviewer that such a figure would be useful in case of a transient evolution of nutrient deposition fields and physical forcing. However, as we stated in Sect. 2, the nutrient inputs to the ocean for PAST, PRESENT, and FUTURE periods are due to an atmospheric simulation using the emission for the years 1850, 2010, 2050, and 2100, respectively. In more detail, the available atmospheric nutrient deposition inputs to the ocean are based on anthropogenic and biomass burning emissions along with the resulted atmospheric chemistry impacts on the gas- and particulate-phases for the single years 1850, 2010, and 2100. Note, however, that a transient simulation of atmospheric tracers from 1850-2100 would require extremely high computational power for the atmospheric chemistry and transport model (which is extremely cost-intensive due to a high number of chemical tracers to be advected and an extremely short time step required for atmospheric models). Therefore, we followed the coordinated CMIP protocol to investigate the effects of atmospheric chemistry AerChemMIP (Collins et al., 2017; https://gmd.copernicus.org/articles/10/585/2017/gmd-10-585-2017.pdf) which recommends only single year emission forcings defined for a limited number of 4 to 6 different year between 1850-2100. Furthermore, compared to the ocean, the atmosphere is comparably well mixed and biomass burning, as an additional source, will be in equilibrium within maximal a couple of months in the atmosphere. The applied physical forcing is kept constant for all simulations of this study, and the whole period (PAST, PRESENT, and FUTURE). This is now clearly stated in Sect. 2.1.1 (see also our reply in GS1).

  However, we now provide a new figure in the Supplement that shows the globally averaged atmospheric deposition data simulated by CTM and applied for PISCES. The following text is added in the manuscript (Sect. 2.2): "*An example of the globally averaged N, Fe, and P atmospheric deposition data as simulated be CTM and applied in PISCES is presented in Fig. S2. Hence, the here discussed simulations should be considered as idealized sensitivity experiments to estimate the response on the ocean surface biogeochemical properties to changed atmospheric deposition.*"

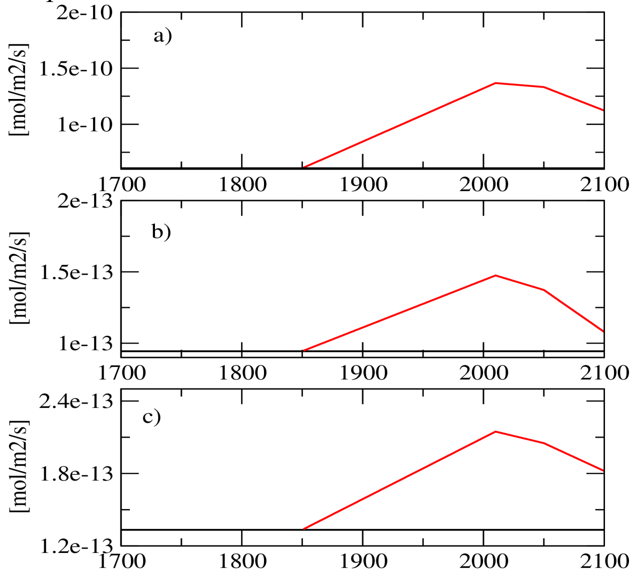


Figure S2: Globally averaged atmospheric deposition fluxes (red lines) of a) nitrogen, b) phosphorous, and c) iron in mol m^{-2} s^{-1}, as simulated by the atmospheric chemistry and transport model and taken into account in PISCES. The black line indicates forcing for the control run under preindustrial conditions (i.e., year 1850).

II. Specific Comments

SC1. 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.

- We propose to add the following sentence in the abstract: '*PISCES, as part of the EC-Earth model suite, runs here in offline mode using prescribed dynamical fields as simulated by the ocean model NEMO.*'

SC2. P1, l9: Please mention how atmospheric CO_2 and climate change is included.

- As our study focuses on nutrient cycling and productivity which in NPZD models are independent of atmospheric CO_2 (or dissolved inorganic carbon in the water), we think it's better not to mention the atmospheric CO_2 mixing ratio in the abstract since it is not a central topic of our study, i.e., the effect on acidity and carbon fluxes is not in the focus here. All the simulations are forced by preindustrial pCO_2 and this is now clearly stated in the revised manuscript, both in the model description and the Summary sections, i.e., "*Moreover, the atmospheric CO_2 mixing ratio is set to the preindustrial value of 284.7 ppm, to effectively isolate the impact of atmospheric deposition on the marine biogeochemistry parameters.*"
  Overall, climate change is not considered here. Our idealistic approach allows us to isolate the effect of atmospheric chemistry and transport changes on productivity undisturbed from any other physical changes and longer-term variability. With climate change included, the conclusion drawn in this study would not be possible or, less robust. However, a detailed discussion on how climate change would affect our results is provided in the new Discussion section now.

SC3. 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?

- We agree with the reviewer. This part now reads as: '*However, the aerosols from natural and combustion sources tend to be deposited in different regions of the oceans. For example, the subtropical North Atlantic Ocean and the Arabian Sea receive the majority of Fe originated from natural dust aerosols, in contrast to the Pacific and Southern oceans where the Fe-containing combustion aerosols play a more important role compared to atmospheric dust (Hamilton et al., 2020; Ito et al., 2019b).*'

SC4. 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 . . .

- This part now reads as: "*The state-of-the-art biogeochemistry model PISCES (Aumont et al., 2015), enabled here within the framework of the European Community Earth System Model EC-Earth (http://www.ec-earth.org/), is here used in offline modus to investigate the impact of atmospheric deposition fluxes of N, Fe and P on the marine productivity. PISCES (Pelagic Interactions Scheme for Carbon and Ecosystem Studies volume 2), as a part of the Nucleus for European Modelling of the Ocean (NEMO), includes a detailed representation of the lower trophic levels of marine ecosystems.*"
  Moreover, a new section (i.e., Sect. 2.1.1) that describes in detail the forcing data used to run PISCES is now added (please see our reply to SC1).

SC5. P6, l5: please specify the physical output used to force PISCES.

- Please see our reply to GC1.

SC6. P6, l6: which OMIP simulation?

- Please see our reply to GC1.

SC7. 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.

- Please see our reply to GC1.

SC8. P6, l7: I guess this refers to the spin up for ocean model without PISCES? Please clarify.

- Please see our reply to GC1.

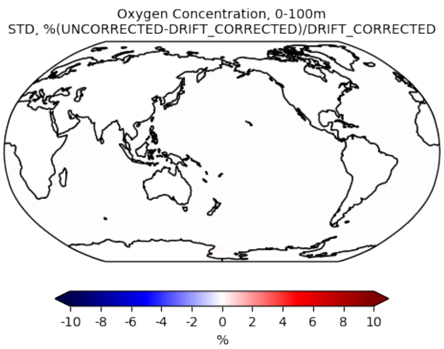
SC9. P6, l8-l10: again – to which simulation or model does this initialization apply?

- Please see our reply to GC1.

SC10. P6, l8-l10: I guess there are still substantial drifts in O_2, N, Si, P and Alk after such a short spin up of 300 years only. Please specify how large the drifts are.

- For this work, we analyzed the results from 1851 to 2100 (namely, PAST: 1851–1870 average, PRESENT: 2001–2020 average, and FUTURE: 2081–2100 average). For the revised version, however, we also performed a control simulation with constant preindustrial deposition inputs (N, Fe, and P) and identical forcing as the STD and ORG transient runs. This new simulation is used for the drift corrections of the model results (please see also our reply to GC2). According to the new control simulation, however, the resulted drifts in surface properties are low and can be considered in equilibrium, without changing our conclusions. Regarding the oceanic concentrations of O_2, N, Si, P, and Alk, we present below the relative differences between the drift-corrected and the uncorrected annual mean oceanic concentrations of i) O_2, ii) N, iii) Si, iv) P, v) Fe and vi) Alk, for depths from the surface up to ~100m.

i)



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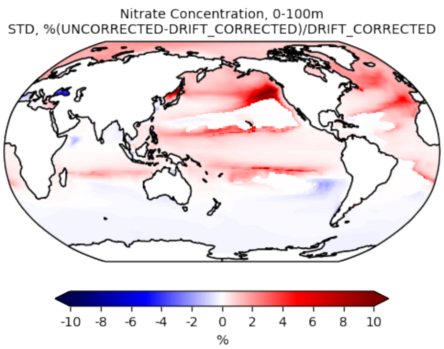

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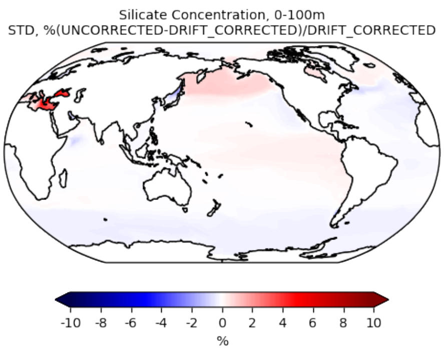

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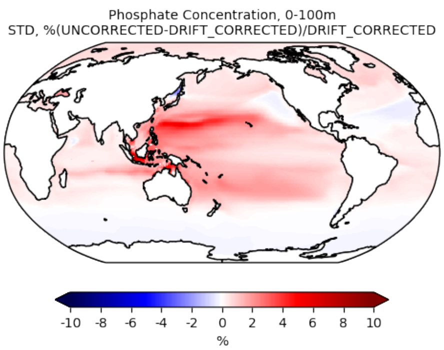
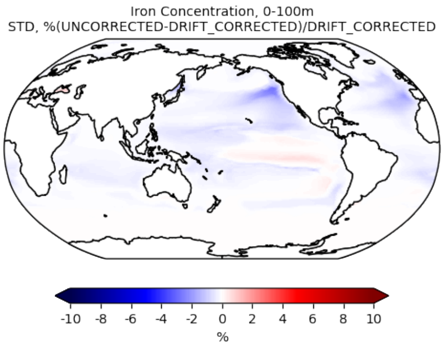
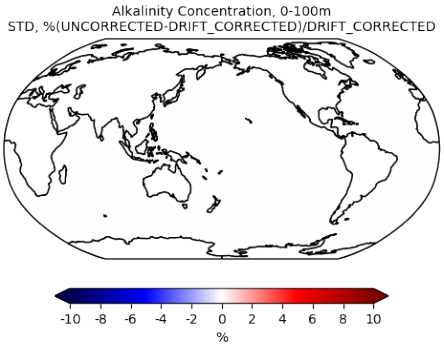


v)

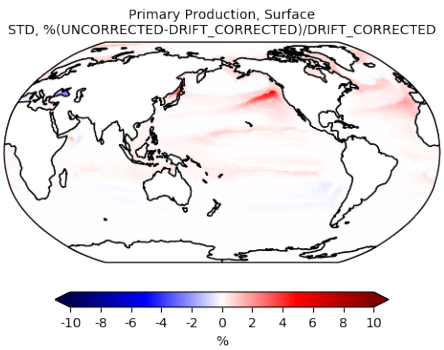
vi)



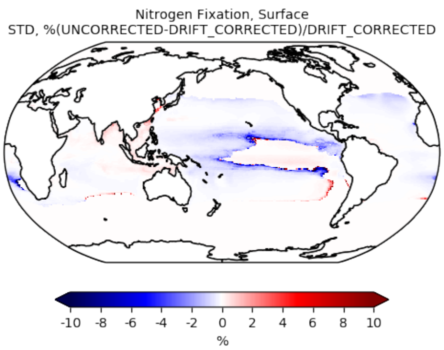


According to the above plots, it is obvious that for O_2 and Alkalinity the drifts are minimal, i.e., ~0.01% and -0.15%, respectively for their global average concentrations. For Si and Fe, the drifts are also very low i.e., ~ -0.15% and -0.07%, respectively, for their annual mean concentrations, whereas for the (closely related in the model) N and P, the drifts are calculated somehow higher compared to other fields (regionally up to 10% in the Pacific Ocean). Nevertheless, for their global average concentrations, the drifts are calculated equal to ~ -0.08% and 0.04%, respectively. Note that we present here the mean relative differences of the oceanic concentrations for the period 1851-1870 (hence after 200 years of spin-up) and 0-100m depths.
For the revised version, we also calculated the drifts for vii) Primary Production and viii) Nitrogen Fixation. Again, the drifts as relatively low for the PAST period (i.e., after 200 years spin-up). The global annual mean primary production and nitrogen fixation change due to drift corrections of about 0.11% and -0.04%, respectively. In more detail, the "uncorrected" primary production equals to 45.64 Pg-C yr^{-1}, and the drift-corrected equals to 45.59 PgC yr^{-1}, thus a difference ~ 0.05 Pg-C yr^{-1} for the PAST period. Respectively, the "uncorrected" N-Fixation equals to 111.87 Tg-N yr^{-1}, and the drift-corrected equals to 111.92 Tg-N yr^{-1}, a difference roughly 0.05 Tg-N yr^{-1}.

vii)



viii)



All in all, we conclude here that there is not a substantial drift in our simulations. However, we agree with the reviewer's comment that it is scientifically more appropriate to account for any potential model drifts when present our results, and for this, in the revised version all results are corrected (please see also our reply to GC2). The following part is added in the model description section: "*To account, however, for potential drifts in the deeper ocean layers, a control simulation as for STD but using only preindustrial (i.e., the year 1850) atmospheric nutrients (N, P, and Fe) inputs into the global ocean is also performed. For example, Fig. S1 (see Sup. Mat.) demonstrates that for the main ocean basins the drift in vertically integrated primary production is minimal and clearly below the signal imposed by the altered nutrient deposition after 1850. This holds even for the Southern Ocean where the impact of atmospheric deposition is typically weak due to the absence of neighbored emission sources. Nevertheless, all model results presented in this work have been adjusted by subtracting the drift of the control run from STD.*"

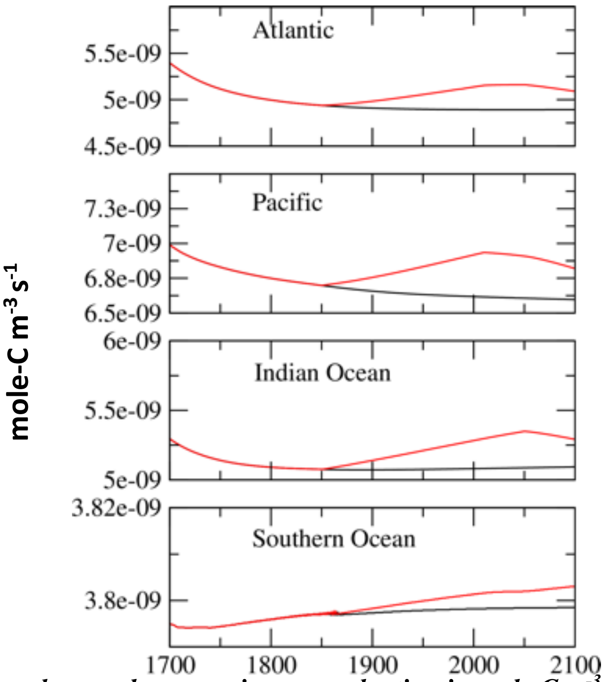


Figure S3: Area averaged annual mean primary production in mole C m^{-3} s^{-1} for the main ocean basins (Atlantic, Pacific, Indian and Southern Oceans). Red lines indicate primary production rates for the STD simulation and black lines the CTRL simulation, respectively.

SC11. P6, l8-10: Is there any initialization of DIC or DOM?

- The following part is now added: "*For the initialization of the ocean biogeochemical fields, the climatological fields of oxygen, nitrate, silicate, and phosphate from the World Ocean Atlas 2009 (WOA; Garcia et al., 2010a, 2010b) along with dissolved inorganic carbon (DIC) and alkalinity from the Global Ocean Data Analysis Project (GLODAP; Key et al., 2004) were adopted.*"

SC12. P6: What is the role of global warming/climate change in these simulations?

- To effectively isolate the impact of atmospheric deposition into the ocean, we did not account for any global warming/climate change. This is now clarified in the new Sect. 2.1.2.

SC13. 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.

- We now include a "true" control run with constant preindustrial (the year 1850) deposition fields to remove any potential drifts from our results. Please see our reply to GC2.

SC14. 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.

- We agree with the reviewer. We changed the name of the first simulation from CTRL to STD.

SC15. P7, l5: 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

- We improved this section and added further analyses. Please see our reply to GC1.

SC16. P7, l7: 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.

- Please see our reply to GC2.

SC17. P6, l7: 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?

- Yes, the physical forcing is identical all periods as stated and for all runs. This is now clearly documented in the revised text. Please see our reply to GC1.

SC18. Section 2: A figure showing the time series of global mean deposition of P, N, and Fe would be really useful.

- The global mean deposition of P, N, and Fe are kept constant before 1850. However, after the 200 years spin-up period (1651 until 1850), the atmospheric deposition input data for the STD and ORG simulations were linearly interpolated from preindustrial to present-day conditions (i.e., the year 2010) to smoothly capture the transition from past to the modern conditions (e.g., Krishnamurthy et al., 2009). Respectively, the deposition data from the present day were linearly interpolated to the projected estimates (i.e., the years 2050 and 2100). Please see also our reply to GC5.

SC19. Section 2.2 A note how these atmospheric deposition fluxes compare with the riverine input would be helpful.

- The following sentences are added in the manuscript:
  - Sect. 2.1.1*: The river supply of N in the model is 36 Tg-N yr^{-1}.*
  - Sect. 2.1.2*: For comparison, we note that the total riverine Fe supply in the model equals 1.45 Tg-Fe yr^{-1}.*
  - Sect. 2.1.3*: Note also that in the model, DP of roughly 3.7 Tg-P yr^{-1} is also delivered to the ocean by rivers.*

SC20. 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)

- The export production in the model for the present-day (STD simulation) is calculated to ~7.54 Pg-C yr^{-1}. Accounting however for the Redfield ratio used in the model (i.e., C/N/P = 122/16/1), the export production for N and P, it would correspond to 1.15 Pg-N yr^{-1} and 0.16 Pg-P yr^{-1}, respectively. Moreover, according to the explicit model calculation, the export production for Fe is about 0.79 Tg-Fe yr^{-1}.

SC21. P11, l29: Is production limited by light as suggested here or also be Fe?

- Yes, the production is also limited by light, as we also stated in the manuscript, i.e., "*PISCES includes two types of phytoplankton, namely nanophytoplankton and diatoms, and it simulates the chlorophyll concentrations and the growth of phytoplankton based on the availability of nutrients (i.e., DP, DN, and DFe for nanophytoplankton and DP, DN, DFe, and DSi for diatoms), temperature and light.*" Moreover, we now provide a new figure which presents the factors that are important in limiting productivity in our model (Fig. 4), i.e., the limitations for nanophytoplankton production by nutrients (N, P), light, and iron.

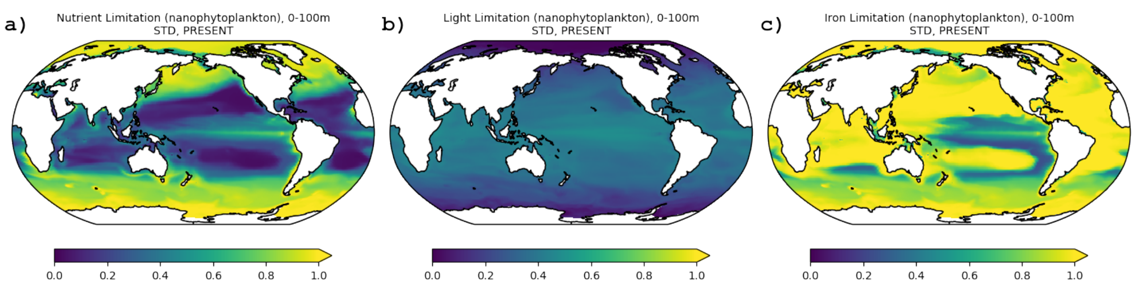


Figure 4: Limitation for nanophytoplankton production by nutrients (N, P right), light (middle), and iron (left). Low values indicate the high limitation imposed by the property.

SC22. P17, l8: Please state how PISCES is forced and what circulation fields are used.

- Please see our reply to GC1.

SC23. 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.

- We acknowledge that there is a basic misunderstanding in this part. For this work salinity restoring was only applied during the OMIP run from which the forcing for the offline model was generated (please see also our reply to GC1). The salinity of the biogeochemical offline runs is kept, however, constant, representing only a seasonal cycle on a daily basis. Since the prolongation of the run for the RCP8.5 scenario is not that relevant for this study, this statement is now removed from the text to avoid any further confusion.

SC24. 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.

- We thank the reviewer for this suggestion. The deposition fields will be freely accessible to the community through the Zenodo. A relevant statement is added to the Data availability section at the end of the manuscript.