Response to the Referees comments – 02

Comment on bg-2021-101
Anonymous Referee #2

Major criticisms:

- the model description has a few gaps that might benefit from filling and improved clarity
- the experimental design involves several highly idealised simulations and it could be clearer which hypotheses are being tested when these are being framed; for instance, rather than use a single experiment that scales river inputs, a suite of scaling runs could instead assess the strength and saturation of feedbacks
- the results are generally clear enough, but there are omissions or odd choices in the results presented; it would be helpful, for instance, to have tables which bring together the major N-cycle processes across the different runs

RESPONSE

Thank you for the comments and suggestions.
In the revised manuscript we address the major criticisms as followed:

- we have included more details in the model description and included the full description of the NPZD model as shown in Figure 1.
- In the revised manuscript we will add the hypotheses to the description of the experimental design. While the first run, where we add the riverine DIN from the NEWS model is a test run, to evaluate first, how the model reacts to the riverine nutrient input and second how the nitrogen cycle responds to it, the following simulations, where we increased the amount of nitrogen added to the ocean, are already scaling experiments to test the mechanisms and feedbacks described for example by Landolfi et al. (2017) and Somes et al. (2016).
  We do not have the capacity to include new model runs in this study, but for a following study we are considering doing some more explicit scaling runs.
- Table 3 in the current manuscript brings together river supply in N, global N2 fixation, global benthic and global water column denitrification for the simulations CTR, NEWS, DIN+DON and 2xDIN. We can provide this table also for the simulations with riverine nitrogen supply from different regions.

Minor comments:

Pg. 1, ln. 1: I might be inclined to make a distinction between the natural and the anthropogenic nitrogen cycles; we have so radically modified the N-cycle that negative feedbacks may have been swamped; in any case, definitely: make it clear in the abstract whether you consider that you are dealing with the natural or modern N-cycle
→ Yes, that is correct. We mention it in the revised abstract and in the main text, that we consider the natural N-cycle. Nevertheless, as stated in line 118, the NEWS dataset is based on river supply in the year 2000 and also includes natural and anthropogenic biogeophysical properties. See also response to next comment.

Pg. 1, ln. 18: “steady state” - I'm not enough of an expert in riverine supply to be sure, but my first reaction is that the scale of anthropogenic inputs of nitrogen to the ocean must make this assumption questionable. It's an assumption I'm happy to make in my own tangential work for simplicity, but where a study is addressing it head-on, I'd expect something on the anthro perturbation to the N-cycle

→ We will address this very relevant remark in the introduction starting l. 44: “At this place it is appropriate to include some remarks about the anthropogenic perturbation of the N-cycle. In the Anthropocene, human activities have led to increased inputs of fixed nitrogen from the atmosphere and through different sources of runoff from land (e.g. Somes et al., 2016; Kim et al., 2014, Lamarque et al.,2013) which also indirectly impacts N_2 fixation (e.g. Krishnamurthy et al., 2006). At the same time, warming and deoxygenation can lead to increased N loss (Oschlies et al., 2019). The question has been therefore raised, if the global nitrogen budget could still be considered to be in steady state. While these combined effects on the nitrogen budget are still very uncertain, some studies suggest, that imbalances could be limited due to internal feedbacks of the N cycle (Landolfi et al., 2017; Somes et al., 2016; Krishnamurthy et al., 2006).”

Pg. 2, ln. 25: “slowly” - to assist less familiar readers, please expand on why we might expect (or why we know) diazotrophs to be slow-growing

→ New text has been added stating that: “Previous studies have shown, that at least the most common diazotrophs, especially Trichodesmium, have low growth rates relative to many non fixing phytoplankton (e.g. Capone et al., 1997). Indeed, while these organisms are able to fix N2 when reactive nitrogen is scarce, this turns into an disadvantage in regions where N is more abundant, because N2-fixation requires more energy (Tyrell, 1999). The slowly growing diazotrophs are then, rapidly out-competed by non-fixing organisms, if enough P and other nutrients are present (Tyrrell, 1999). Note that part of our knowledge of N2-fixation and most modern models concepts are still based on the original limited assumptions based on given species especially Trichodesmium.”

Pg. 2, ln. 29: “most marine organisms” - except diazotrophs, of course

→ “for most other marine organisms”

Pg. 2, ln. 32: “consumption of O2” - this statement is perhaps confusing as people will be aware that growth of phytoplankton *produces* oxygen; I know what you mean here, but others might not

→ Thanks for your remark, we will change the text to: “heterotrophic O2 consumption during organic matter remineralization”

Pg. 2, ln. 34: “work together” - do they really "work together"?; might it not be fairer to say that they work independently, but between them the nitrogen cycle is balanced (which it inevitably must be)

→ Text updated to read: “These two processes, N2 fixation and denitrification, both contribute to regulate the global marine N budget.”

Pg. 2, ln. 38: “considered in this study” - it might be useful to indicate the size of the deposition sink so that it's clear why it's ignored here; also, my first thought here was that you didn't want to have to add an additional low importance process, but it looks like someone has already done this for your model; so perhaps be very clear here why you're ignoring it

→ Thanks for your comment. We will address this and delete the sentence in l.38, but add some more explanations in l.68-75 (see comment to that line) "~~Other sources of N include atmospheric deposition, which will not be considered in this study~~.

Pg. 2, ln. 39: "highly" - how high is "highly"? do you have an estimate of how much the riverine flux has been affected by human activities, or is this still highly uncertain?

→ We will tone down this somewhat and include: "Although riverine N is not the main source of N for most marine environments, it can become important, as it is directly influenced by human activities. Seitzinger et al. (2010) e.g. estimated that global nitrogen export by rivers to the coastal waters increased by 17,7 % from 1970 to 2000. Nitrogen is known to impact…"

Pg. 2, ln. 47: "2008" - 2008 doesn't seem very "current"; perhaps reword or find a better Example → "However, often global biogeochemical ocean models still omit…"

Pg. 2, ln. 50: "as on" - "as on" -> "*than on* the actual quantity of nitrogen nutrient" → thank you, we have corrected this.

Pg. 2, ln. 55: "long enough" - you should say what you think the relevant time period is; I believe Tyrrell (1999) puts an estimate on this based on input flux and ocean inventory

→ "likely not long enough to study the feedbacks of the nitrogen cycle in the open ocean, considering that the mean residence time of fixed nitrogen in the ocean has been estimated to be a few thousand years (Gruber, 2004)."

Pg. 2, ln. 57: "estimations of" -> "estimated" → will be corrected

Pg. 3, ln. 62: "EMICs" - a passing mention of computation cost would help explain the underlying attraction of EMICs → Yes thanks, this has been lost from a previous version of the manuscript and will be added again, with the new text stating: "EMICs allow the integration of a large number of processes, more than conceptual models, using often coarser resolution and simplifying assumptions e.g. describing the atmospheric circulation compared to ESMs, but thus avoiding higher computational costs".

Pg. 3, ln. 75: the rationale for this omission of atmospheric deposition needs to be made very clear; I was previously assuming that you were ignoring it (1) because it was one more process to add (... but Landolfi et al. seem to have already added it to this model), and (2) because it's much less important than riverine input (... but you imply otherwise here); please be clear on the rationale here

As announced above, we add some explanations on our omission of atmospheric deposition in this paragraph starting l.68:

"Atmospheric deposition is known to be another important source of N to the ocean. Although it is estimated to add nitrogen at the same magnitude as the rivers and will also become more important with increasing anthropogenic activities (e.g. Tyrell, 1999; Cornell et al., 1995), it will not be considered in this study. Previously, Landolfi et al. (2017) and Somes et al. (2016) used UVic to study the response of the marine N cycle to atmospheric N deposition and its impact on marine productivity. While Somes et al. (2016) performed a series of idealized sensitivity experiments to evaluate the spatial and temporal scales of N cycle feedbacks, Landolfi et al. (2017) used an atmospheric N deposition forcing reconstructed using the multimodel mean of the Atmospheric Chemistry and Climate Model Intercomparison Project (Lamarque et al., 2013). Both found that N cycle feedbacks stabilize the model's marine N inventory and limit changes to the marine N cycle and productivity. But none of these studies included riverine N supply. In order to disentangle the effects of the different sources of N, we are using UVic without atmospheric N deposition, to focus on…"

Pg. 3, ln. 79: "series of simulations" - it would possibly be helpful if these experiments were given a scientific rationale in addition to their description (e.g. "this experiment simulates increased anthropogenic emissions", "this experiment simulates differential waste water management between geographical regions", etc.)

→ We have modified the text as follows:

~~"To test the mechanisms and feedbacks described in the two previous studies (Landolfi et al., 2017; Somes et al., 2016) but now with river nitrogen fluxes, we perform a series of simulations with different amounts of N input…"~~

→ "In order to test the N cycle mechanisms and feedbacks found and described before, we set up an experiment where we simulate differential riverine nitrogen supply to the coastal oceans."

Pg. 4, Figure 1: this diagram makes it look like PO4 might be added to the ocean as well via a fixed R_N:P parameter; is that right?

→ NO3 and PO4 are linked through exchanges with the biological variables by constant (Redfield) stoichiometry, but PO4 is not directly added while NO3 is supplied from the rivers. This has been added in the caption of Fig.1 (see below).

Pg. 4, Fig. 1: maybe identify the 7 state variables in the caption to help with clarity

→ The caption of Figure 1 has been updated to be: "Ecosystem model schematics for the NPZD model with the prognostic variables (in square boxes) and the fluxes of material between them, indicated by arrows. The prognostic variables include two nutrients, nitrate (NO_3) and phosphate (PO_4), two phytoplankton (nitrogen fixers P_D and other phytoplankton P_O) as well as zooplankton (Z), sinking detritus (D), and dissolved oxygen (O_2). Nitrate (NO_3) and phosphate (PO_4) are linked through exchanges with the biological variables by constant (Redfield) stoichiometry."

Pg. 4, ln. 97: "updates" - a little expansion on these updates might help readers understand if they are significant

→ We included: "… with updates of some of the equation parameters as noted in Partanen et al. (2016), where a small error in the code corrected".

Pg. 4, ln. 97: "prognostic variables" - you don't refer to the variables by the abbreviations used in Figure 1; nor does Figure 1's caption

→ Thank you for this reminder. The caption has been updated (see above) and the lacking abbreviations will be included in the text: "Seven prognostic variables are embedded within the ocean circulation: two phytoplankton classes (nitrogen fixing diazotrophs P_D and other phytoplankton P_O), zooplankton (Z), sinking particulate detritus (D), nitrate (NO_3), phosphate (PO_4) and oxygen (O_2). Nitrate (NO_3) and phosphate (PO_4) are linked through exchanges with the biological variables by constant (Redfield) stoichiometry. "

Pg. 4, ln. 100: “atmospheric” - technically, they're fixing dissolved dinitrogen which is in equilibrium with atmospheric N2
→ The text has been changed to: “Nitrogen gas dissolved in seawater “

Pg. 5, ln. 105: “benthic denitrification” - this could be described a little more clearly; I *think* you mean that a function based on a more sophisticated benthic sediment model turns receipt of organic matter at the seafloor into oxic and anoxic (with denitification) remineralisation of this matter; is that right?
→ Thanks for your remark, we agree that our original text was confusing. We changed the text to: “Benthic denitrification, in particular, is believed to be the major sink for fixed N (Voss et al., 2013; Galloway et al., 2004). It is included here through empirical transfer functions derived from benthic flux measurements (Bohlen et al., 2012). The functions are based on the rain rate of particulate organic carbon to the seafloor and bottom water O_2 and NO_3 concentrations.”

Pg. 5, ln. 107: “models” - "models" or "model"?; this is a little confusing → see above

Pg. 5, ln. 109: “subgrid” - how "subgrid" is subgrid here?; for instance, do you simply identify the fraction of a cell that is shelf and do calculations based on this basic split, or do you divide each cell into an N x M subcell domain that does the bathymetry better?
→ This information has been added: “For each cell near the coast this scheme calculates the sea floor area with the cell at a higher resolution following Somes et al. (2010b)”.

Pg. 5, ln. 123: To avoid confusion in readers, perhaps mention that NEWS2 includes no runoff from the Antarctic continent → We will add to the section the sentence: “Note that NEWS2 excludes runoff from the Antarctic continent.”

Pg. 5, ln. 133-135: it sounds like this assumes a constant seasonal cycle in runoff; this is not unreasonable in an EMIC where the hydrological cycle may be in a long-term equilibrium → Yes, we can add this comment in the text: “Here, we assumed a constant seasonal cycle in runoff and that nitrogen concentrations in the discharged river water are constant throughout the seasonal cycle. We then distributed the annual load over the months, weighted by the fraction of monthly freshwater discharge.”

Pg. 6, ln. 136: can you clarify what happens with riverine P in this model please?; Figure 1 tends to imply there might be a link between riverine N and P; Global NEWS provides both, and the balance of N and P could be important for your model's N2-fixation response
→ “Phosphate is not added in this experiment and we generally assume a fixed marine P inventory, like in most previous studies with UVic. The inclusion of a dynamic P cycle (like in Niemeyer et al., 2017, Kemena et al., 2019) with riverine P supply from NEWS2 will be subject to a follow up study”.

Pg. 6, ln. 145: “bioavailable” - maybe add: "(or rapidly turned over to DIN)" → Yes, we can add that
Pg. 7, Table 1: add control experiment to this table → Thank you for this suggestion, CTR will be added
Pg. 7, ln. 149: “steady state” - it might be nice to see a figure (supplementary?) where the long-term balancing of the N budget took place; for instance, to illustrate the

timescales associated with addition and removal processes → Thank you for this suggestion. We had included the timeseries of N in a previous version and planned to prepare it for the supplements. We will submit it with the revised manuscript.
Pg. 7, ln. 155: “fairly well” - all runs omit the midwater maximum around 1000 m → “In comparison with observational data of the World Ocean Atlas (Garcia et al., 2019), the model underestimates the observed NO_3 in the whole water column by 3 to 4 mmol m^{-3} and especially omits the midwater maximum around 1000 m.”

Pg. 7, ln. 156: “global average” - what is the global concentration difference?; and what is this as a percentage of total observed inventory?
→ The text will be updated to read: “Global average NO3 concentrations only vary a little between the simulations (from 22.19 mmol m-3 in CTR to 22.48 mmol m-3 in NEWS and 22.84 mmol m-3 in 2xDIN) and the differences to CTR correspond to +1.1 %, +1.8 % and +2.5 % of the total observed inventory for NEWS, DIN+DON and 2xDIN respectively. However, the absolute error between model and observations decreases with higher riverine N supply.”

In addition to the numbers in the text and in Table 2 we will include some of these numbers here. Note that the differences relative to the observed inventory correspond to the last column in Table 2.

Simulation	**Glob. NO3 Conc. [mmol m-3]**	**NO3 surface**	**NO3 1100 m**	**Difference to CTR**	**Diff / WOA [%]**
CTR	22.19	5.71	24.64	0	
NEWS	22.48	5.93	24.94	0.288	1.1
DIN+DON	22.66	6.15	25.10	0.468	1.8
HIGH DIN	22.84	6.19	25.30	0.646	2.5
WOA	26.29	6.62	31.22	4.102	

Pg. 7, ln. 156: “misfit” - this is absolute error, right? → The text has been changed accordingly (see above)
Pg. 7, ln. 167: “not surprising” - well, not *entirely* surprising; it's possible, of course, that adding local sources of N might trigger strong balancing denitrification that could even offset the addition (though this seems unlikely) → Yes, we will attenuate this statement as you suggest.
Pg. 7, ln. 171: on this point, might it be possible to include some total of suboxic ocean volume (e.g. the volume of below some standard oxygen threshold concentration)?
→ We have calculated global volume of oxygen minimum zone (OMZ) for all simulations, so that we can include a sentence here: “Note, that global volume of ocean minimum zones, defined here as regions with O_2 concentration lower than 70 μmol kg^{-1}, are increasing with higher nitrogen supply, from 52 $*10^6$ km^3 in CTR, to 54 $*10^6$ km^3 in NEWS, 56 $*10^6$ km^3 in DIN+DON and 58 $*10^6$ km^3 in 2xDIN.”

Pg. 8, Table 2: “+1,12” -> “+1.12” → The typo will be corrected.
Pg. 10, Figure 5: normally, delta (or bias) plots are coloured blue (negative) to red (positive) with white in the centre –> Thank you for this suggestion, we have replotted all delta figures with a red-white-blue colourbar.

Pg. 11, Figure 6: why 850m?; from Figure 3b, the largest misfit seems to be at 1000m → Figure 6 has been updated with the blue to red colourbar. We chose 850 m and 1100 m depth, because although in the global profiles the largest misfit has been found around 1000 m, at 850 m depth the regions with decrease in No3 concentrations are most pronounced (compare also with Figure 5).

Pg. 12, Figure 7: it's obviously not possible to tell how realistic oxygen is here; perhaps compare NEWS and CTR directly to WOA instead? → If we would compare NEWS and CTR to WOA, we would find almost no differences between the two plots, because our UVic simulations show a somewhat different pattern and magnitude in O_2 especially in the Arabian Sea and the Gulf of Bengal. What we have done to address your comment is to add four plots to the Figure, showing the differences between WOA and CTR for O_2 and NO_3 in the two ocean basins.

Pg. 13, ln. 192: surplus "like" → Yes, corrected, thank you.

Pg. 13, ln. 193: assuming that N inputs fuel corresponding increases in productivity, it may be worth noting in passing how much production is also enhanced by river N in these regions → We haven't calculated total production in the Bay of Bengal but the analog Figure to Figure 9 and 10 for NPP shows, that productivity is enhanced only very locally near the coast of Myanmar (but up to +36 Tg C yr-1). In the rest of the basins, productivity is lower in the simulations with riverine N supply. We will include this Figure in the supplements and add the information in the text.

Pg. 13, ln. 197: see my earlier remarks about PO4 availability and riverine sources; if riverine P is neglected, this may skews where N2 fixation is favoured; ditto if PO4 is added in strict proportion to DIN → P is not altered by the additional nitrogen from river supply

Pg. 14, Figure 8: maybe it would be better to show CTR N2 fixation in addition to the deltas? → An additional plot has been added to the panel showing N_2-fixation from CTR simulation.

Pg. 15, ln. 223: "oxygen concentrations even though higher at the surface" - Is this elevated oxygen due to enhanced production?; typically surface oxygen is boring because it equilibrates quickly to saturation values (with ambient temperature)
→ We have added here: "In the Bay of Bengal, oxygen concentrations appear higher at the surface in NEWS than in CTR by around 1.5 mmol m^{-3} at least in the southern part of the Bay, which could be due to enhanced production. However, compared to WOA, the oxygen concentrations are still very low in the upper 800 m in NEWS like in CTR, and they are particularly low in the NEWS simulations in the subsurface waters and the whole deeper basin (Fig. 7)."

Pg. 15: per my remarks for Figure 7, making the model's relationship with observed oxygen clear might be useful → Thanks for this suggestion, a comparison with O_2 from WOA has been added to the panel plot.

Pg. 17, Table 3: add a column listing the balance at equilibrium?; I make the discrepancy about 0.5 Tg N / y for all of the model experiments; actually, what is this discrepancy?; is it the model just not fully equilibrated? → Yes, there are discrepancies between 0.4 and 0.5 Tg N / yr in the total sums depending on the simulation, although the model is equilibrated. Nevertheless, UVic has some natural variability and N fixation and denitrification oscillate with a difference in the range of the discrepancies you mention. We will add this information after the table: “Note that the global sums from sources and sinks do not exactly add to zero due to natural variability in the modeled N-cycle.”

Pg. 18, Figure 11: if possible, it might be an idea to include a map of observational estimated production; the total of the models here might be OK, but I think the patterns - particularly in the Indian Ocean - might not be; this is important given the amount of analysis that is focused in this region → We will provide a Figure with marine production estimated from satellite observations, based on data from Behrenfeld and Falkowski 1997.

Pg. 18, Figure 11: surplus colourbar on Figure 11b? → Figure 11 has been updated with a new (blue-red) colour scheme and without the colourbar on panel (b).

Pg. 19, Table 4: this is a weird, single-column table → We will include more lines. The table will now appear as:

Source	**NPP [Pg C yr-1]**	**Method**
UVic CTR	54.9	model
UVic NEWS	55.3	model
UVic DIN+DON	55.5	model
UVic 2xDIN	55.7	model
Behrenfeld and Falkowski (1997)	43.5	Satellite data
Behrenfeld et al. (2005)	67	Satellite data
Carr et al. (2006)	51	Mean of 31 global models
Westberry et al. (2008)	52	Carbon based, spectral
Buitenhuis et al. (2013)	56	Model and observationsal database

Pg. 19, Table 4: these numbers are a bit higher compared to what I'm used to; e.g. the Oregon State University primary production website; there, I find ~40 Pg C / y; the 3 models their site includes are quite divergent, however
→ Buitenhuis et al (2013) shows an overview from 14 different approaches to calculate global marine primary production rates and the recent one’s range between 40 and 60 Pg C / y. From the studies cited by Buitenhuis, the global production as calculated by Westberry et al. (2008) and Behrenfeld et al. (2005) are also in Table 4. We have also analysed the dataset from Behrenfeld and Falkowski (1997), which gives a rate of 43.5 Pg C yr^{-1}. To be more complete, we will also include this study in Table 4 (see answer below).

Pg. 20, Table 5: this is a very strange way to organise a table; it's a single column when it should be three columns of numbers→ Yes, sorry for this. Originally, we hadn’t planned to provide this table and included it at the last moment. It will be reorganized in the revised manuscript as you suggest, including supplementary numbers (see responses below).

Pg. 20, Table 5: also, I might be inclined to include numbers (or deltas) for the other major N-cycle processes, N2-fixation and denitrification → Table 5 was originally only a supplement for Figure 11. We haven't extracted all the minima and maxima for the other N-cycle processes but for the revised manuscript, we have calculated the global totals for the regions presented in this table and will include them as suggested.

Pg. 20, Table 5: also, why the areal units here?; would it not make more sense to report global totals (i.e. Pg C / y)? → Table 5 was originally only a supplement for Figure 11, to show the extremes in vertically integrated primary production rates. For the revised manuscript we will include the regional totals in Tg C / yr for the Bay of Bengal, the Yellow Sea, the North Sea, the outflow of the Rio de la Plata and the East Mediterranean Sea to put the numbers into relation. This will then look like this (additionally we will include N-cycle processes):

Region	**Difference in NPP in the whole region [Tg C yr-1]**		
	NEWS - CTR	**DIN - CTR**	**2xDIN - CTR**
Bay of Bengal	-19	-23	-31
Yellow Sea	119	133	190
North Sea	35	43	62
Rio de la Plata river mouth	17	27	35
Eastern Mediterranean Sea	41	50	53

Note that local minima or maxima can be considerably higher than the global sums.

Pg. 24, ln. 352: "increase in marine primary production is small" - this analysis appears not to factor in that total riverine input of N is ~0.2% of the N used in primary production; so, contrary to the point you make here, the changes in NPP found between the simulations appear to actually be quite large; I guess the factor that makes the rivers more important is that they deliver N to shallow ocean areas (= shelves) where they will have a larger impact

→ Yes, thanks for your remark. Unfortunately, I could not find a reference for this number (0.2 %). But we will add to our text: "The absolute increase in marine primary production is small (between +0.7 % in NEWS and +1.3 % in 2xDIN). However, relative to the amount of N added to the global ocean, primary production increases yearly by 17.5 Tg C per additional Tg N in NEWS (16.0 Tg C per Tg N in 2xDIN). As we have shown, primary production increases mainly near the river mouths, where high nutrient loads are injected in shallow ocean areas, creating production "hot spots", while only small changes in production have been found in the open ocean. Other studies with additional N supply also found only moderate increase in global primary production rates…"

Pg. 25, ln. 387: "carbon export" - how is this defined?; e.g. 100 m export?; and does it include or exclude shelf regions where material is not properly exported? → We will add in the text: "Indeed, simulated carbon export, evaluated at the 82.5 m level and including all the shelf regions, increases globally by only 0.06 Pg C / yr in our NEWS simulation."

Pg. 25, ln. 387: You might want to consider these changes in light of how much nutrient is being added to the ocean relative to N cycling through production; if my earlier back-ofthe-

envelope calculations are right, river N is 0.2% of N-cycling through production, but you're finding several percent in NPP change
→ We modify our conclusion to: "We have seen that in the coastal regions and especially in some hot-spot regions near the river mouths, riverine nitrogen input leads to higher primary production rates. Globally, NPP rates increase up to 17.5 Tg C per Tg N added to the ocean. However, the biogeochemical feedbacks of the ocean buffer further increases in global N concentrations and global NPP."

Pg. 25, ln. 390-392: this paragraph doesn't really say very much; I'd suggest deleting it
→ We will shorten it to: "~~Our study emphasizes the importance of understanding the feedbacks of the marine biogeochemistry in general and the N cycle in particular in order to predict the response of the system to changes in riverine nutrient supply~~. We have found, that likewise to atmospheric deposition, river supply of nitrogen is not only relevant for the coastal system but also for marine biology in the global ocean. But while atmospheric deposition provides only N, …"

Pg. 26, ln. 397: if riverine phosphorus is not included here, does that not skew the model's balance between N2-fixation and denitrification since the N being added is not balanced by P? → Yes, this may be true, which is why we will investigate P and N additions in a following study

Pg. 26, ln. 400: "hdl" - what is this?; could this be put onto Zenodo or something to get a proper DOI? → The correct link was https://hdl.handle.net/20.500.12085/59977a36-e8e7-4348-a4e8-2b13f3913590 and will be corrected in the revised manuscript. We are still waiting for the OPeNDAP access. Thank you for the suggestion of Zenodo. I have planned to use this server for the next data set.

Pg. 26, ln. 402: I get a 404 error from this link → I tested the link again and it worked for me. Perhaps we could just include the "Home" link which is http://icr.ioc-unesco.org/? In the index on the left it is also possible to find the link to the "Global NEWS".

Style points:
Pg. 8, Figure 3: can you make the lines in the key thicker so it's easier to tell them apart?; also, the choice of colours is rather unhelpful in this regard; also, why are they ordered in this strange way in the key? → Thanks for your suggestions. We have changed the lines and the colours of the Figure.
Pg. 9, Figure 4: this colour scale looks more like a delta one to me → We have changed the colours of the Figure.

Pg. 10, Figure 5: it might be better not to stretch the smaller Indian basin to the same size as the other basins here; perhaps just plot the same latitude range on all three panels? → As this would mean a lot of work to change the range of all Figures with the ocean basins, we have let the plot of the Indian Ocean like it is. But we will include a note, indicating that the ranges are different.

Pg. 15, Figure 9: this colourbar implies negative denitification is possible in the model; is it? → We have changed the colourbars of this Figure, using a delta colourbar (blue-red) for panels b-d and have rearranged the colourbar for panel (a) in order to avoid this impression. There are no negative denitrification rates in (a).

Pg. 16, Figure 10: this colourbar is missing the extreme cyan colour that indicates "out of range" delta concentration → Thanks for you remark. The colourbar has been changed to blue-red and the cyan colour is now shown correctly.

Pg. 23, Figure 13: could you try a clever log scale here?; this plot is otherwise not very Informative → Unfortunately we haven't found another way to plot these results.

Pg. 25, ln. 369: purely as a style point, I would suggest thinning your conclusions section to 5 or 6 bullet-point statements of your findings; this makes it very easy for readers to understand the main findings (and decide whether to read more!) → Even though I am not very fond of bullet points in a text, I will follow your suggestion for the revised manuscript.

Additional literature to the manuscript

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Cornell S., Rendell A., and Jickells T.: Atmospheric inputs of dissolved organic nitrogen to the oceans, Nature, 376, 243-246, 20 July 1995.

Kim I.-N, Lee K., Gruber N., Karl D. M., Bullister J. L., Yang S., and Kim T.-W.: Increasing anthropogenic nitrogen in the North Pacific Ocean, Science, 346, Issue 6213, 1102-1105, 28 November 2014.

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