Comment on bg-2021-101

Anonymous Referee #2

Referee comment on "Riverine nitrogen supply to the global ocean and its limited impact on global marine primary production: a feedback study using an Earth system model" by Miriam Tivig et al., Biogeosciences Discuss., https://doi.org/10.5194/bg-2021-101-RC2, 2021

Summary:

The manuscript presents an adaptation of an existing EMIC model in which riverine fluxes of nitrogen from the Global NEWS2 dataset are introduced. These new fluxes are used in a number of experiments that investigate the resulting changes in primary production, N2-fixation, and denitrification. A focus of the analysis is the impact on the nitrogen cycle of this change on these balancing processes.

Overall:

The manuscript is generally straightforward to understand. My main criticisms of the current draft would that that:

- 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

My recommendation is that the manuscript should be accepted after revision that addresses these major criticisms. I include below a list of more minor comments on the manuscript, and additionally have added some suggestions that the authors may want to consider during revision – these are mostly stylistic, and I leave their adoption up to the authors.

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

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

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

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

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

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)

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

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?

Pg. 2, ln. 47: "2008" - 2008 doesn't seem very "current"; perhaps reword or find a better example

Pg. 2, ln. 50: "as on" - "as on" -> "*than on* the actual quantity of nitrogen nutrient"

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

Pg. 2, ln. 57: "estimations of" -> "estimated"

Pg. 3, ln. 62: "EMICs" - a passing mention of computation cost would help explain the underlying attraction of EMICs

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 Landolfini 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

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.)

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?

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

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

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

Pg. 4, ln. 100: "atmospheric" - technically, they're fixing dissolved dinitrogen which is in equilibrium with atmospheric N2

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 denitrification) remineralisation of this matter; is that right?

Pg. 5, ln. 107: "models" - "models" or "model"?; this is a little confusing

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?

Pg. 5, ln. 123: To avoid confusion in readers, perhaps mention that NEWS2 includes no 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

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

Pg. 6, ln. 145: "bioavailable" - maybe add: "(or rapidly turned over to DIN)"

Pg. 7, Table 1: add control experiment to this table

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

Pg. 7, ln. 155: "fairly well" - all runs omit 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?

Pg. 7, ln. 156: "misfit" - this is absolute error, right?

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)

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)?

Pg. 8, Table 2: "+1,12" -> "+1.12"

Pg. 10, Figure 5: normally, delta (or bias) plots are coloured blue (negative) to red (positive) with white in the centre

Pg. 11, Figure 6: why 850m?; from Figure 3b, the largest misfit seems to be at 1000m

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?

Pg. 13, ln. 192: surplus "like"

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

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

Pg. 14, Figure 8: maybe it would be better to show CTR N2 fixation in addition to the deltas?

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)

Pg. 15: per my remarks for Figure 7, making the model's relationship with observed oxygen clear might be useful

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?

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

Pg. 18, Figure 11: surplus colourbar on Figure 11b?

Pg. 19, Table 4: this is a weird, single-column table

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

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

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

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)?

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

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?

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-of-the-envelope calculations are right, river N is 0.2% of N-cycling through production, but you're finding several percent in NPP change

Pg. 25, ln. 390-392: this paragraph doesn't really say very much; I'd suggest deleting it

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?

Pg. 26, ln. 400: "hdl" - what is this?; could this be put onto Zenodo or something to get a proper DOI?

Pg. 26, ln. 402: I get a 404 error from this link

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?

Pg. 9, Figure 4: this colour scale looks more like a delta one to me

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?

Pg. 15, Figure 9: this colourbar implies negative denitification is possible in the model; is it?

Pg. 16, Figure 10: this colourbar is missing the extreme cyan colour that indicates "out of range" delta concentration

Pg. 23, Figure 13: could you try a clever log scale here?; this plot is otherwise not very informative

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!)