Biogeosciences Discuss.,
https://doi.org/10.5194/bg-2018-162-RC1, 2018

Biogeosciences Discussions
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Interactive comment on "Diazotrophy as the main driver of planktonic production and biogeochemical C, N, P cycles in theWestern Tropical South Pacific Ocean: results from a 1DV biogeochemical-physical coupled model" *by* Audrey Gimenez et al.

Anonymous Referee #1

Received and published: 4 May 2018

1 General comments

The authors compare two 1D simulations only differing in the presence of diazotrophy to examine the role of N fixation for plankton production and biogoechemical cycles in observations in the Western Tropical South Pacific. While this aim of the study is given at in the introduction and the simulations presented are well suited to address this aim,

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in their interpretation of the results the authors claim to show the control of preferential P regeneration (a model assumption that is not tested) on N fixation. In my opinion, the simulations necessary to justify this latter claim are not provided. The results are very interesting and relevant to the current discussion on N fixation, and the underlying processes identified seem reasonable, but either the interpretation needs rephrasing or additional simulations need to be presented to acknowledge the causality implied by the study setup. Affected sections are identified in detail below. Furthermore, some validation of the simulated physics (MLD) on the seasonal scale would be needed to give confidence in the validity of the results.

1. Does the paper address relevant scientific questions within the scope of BG?
   yes
2. Does the paper present novel concepts, ideas, tools, or data?
   yes
3. Are substantial conclusions reached?
   yes, but see below.
4. Are the scientific methods and assumptions valid and clearly outlined?
   partly. The design of the model study appears valid, but the way the interpretation of the results is phrased suggests a different cause and effect than the study design. Take, for example, the authors' claim in the abstract and the discussion that they "evidenced that the nitracline and phosphacline had to be respectively deeper and shallower than the Mixed-Layer Depth (MLD) .. [to create] ... favourable conditions for the development of diazotrophs" (p1,l8-11) and "concluded that a preferential regeneration of the detrital phosphorus (P) matter was necessary to obtain this gap between the nitracline and the phosphacline depths ..." (p1,l11-13). But neither the depth of the nutriclines nor the preferential P regeneration are manipulated in the simulations presented. Causality here

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goes the other way, in my opinion: the authors set up a system with preferential P regeneration and then show how N fixation creates different biogeochemical regimes.

5. Are the results sufficient to support the interpretations and conclusions?
   no. The simulations required to show the control of P availability on N fixation as claimed (e.g., additional simulations without preferential P regeneration), are mentioned in the discussion as preliminary (p11,l34 - p12,l1), but are not shown. Either the interpretation needs to change from "control of P availability on N fixation" to "effects of N fixation on nutricline and seasonality", or additional simulations need to be provided.

6. Is the description of experiments and calculations sufficiently complete and precise to allow their reproduction by fellow scientists (traceability of results)?
   no. The study refers to previous studies (Gimenez et al. 2016 - G2016, Alekseenko et al. 2014 - A2014) for all equations and parameter values, stating that "for all the non-diazotrophic features, TRI are parameteriyed as 100 PHYL cells ... and UCYN as PHYS." (p4, l20-21). Yet both references differ in some of the parameter values. Yet G2016 give specific mortality rates of UCYN as 1.16 10^{-6} s^{-1} compared to 1.16 10^{-7} s^{-1} for PHYS (A2014), and the max. growth rate of TRI as 2.08 10^{-6} s^{-1} compared to 2.3 10^{-5} s^{-1} for PHYL (A2014). This is confusing, although maybe not the fault of the authors: What is a typo, what is related to the conversion of values for TRI as 100 PHYL cells, which values are used in the present study?

7. Do the authors give proper credit to related work and clearly indicate their own new/original contribution?
   yes.

8. Does the title clearly reflect the contents of the paper?
   it reflects the study setup, but not the interpretation/conclusions drawn from it.

9. Does the abstract provide a concise and complete summary?
   it needs rephrasing to fit the study design

10. Is the overall presentation well structured and clear?
    yes

11. Is the language fluent and precise?
    yes, only in few sections a bit redundant

12. Are mathematical formulae, symbols, abbreviations, and units correctly defined and used?
    yes. some minor inconsistencies are pointed out below.

13. Should any parts of the paper (text, formulae, figures, tables) be clarified, reduced, combined, or eliminated?
    the abstract, discussion and conclusion should be brought in line with the simulations/results presented.

14. Are the number and quality of references appropriate?
    yes

15. Is the amount and quality of supplementary material appropriate?
    a supplement could clarify which parameter values were used (cf. no. 6 above)

2 Specific comments

1. p3, l24-27:

   (a) you use two simulations identical in forcing and physics to simulate two locations about 40 deg. longitude apart with different biogeochemical characteristics. Is it justified to apply the same physics and forcing to both locations?

If so, could you provide evidence for this, e.g. observations? It this choice compromising the fit between model and observations for station WGY?

(b) it confused me that the simulations were named after the different locations, e.g. simWGA, while applying the same physics/forcing. I was expecting different physics. In my words, I would say you used an idealized environment representative of station WMA to test the effect of N fixation, and got results in good agreement with the other location in the case of no N fixation. To me it would thus be more intuitive to call the simulations Nfixation and no-Nfixation or something similar.

2. p4,l20-21. How does your parameterisation of the different PFTs compare to other parameterisations of diazotrophy in the literature?

3. p4,l29-p5,l12: you modify the published biogeochemical model parameterisation substantially. Why were those changes necessary? Did you perform any optimisation or did you tune the model by hand? Was the model particularly sensitive to any of the parameters? Are the physics simulated in agreement with observations?

4. p5,l22; p8,section 3.2.1.; Fig. 4: you mention winter and winter mixing. Is this appropriate for a tropical region at 17 deg S? As winter mixing I would understand much deeper and more rapid isolated mixing events as are characteristic for the temperate latitudes. Is the gradual deepening simulated for the MLD in Fig. 4 not mostly a result of increasing salinity in the dry season?

5. p6,l18: Could you mention here already that the shallower phosphacline than nitracline results from the model setup with preferential P regeneration? This aspect is not a result of the simulations presented here and would have made it significantly easier for me to understand this section.. I acknowledge that there are different writing styles. In the case of this ms, readability of the results section

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would in my opinion be greatly improved if short explanations for the model data and the model-data discrepancies that follow directly from the model assumptions (i.e., the 1D model accumulating additional N due to missing advection; the shallower phosphacline compared to nitracline because of preferental P regeneration), were given already in the results section. The discussion section would then focus on putting the findings into context/analysing more complex mechanisms.

6. Fig. 2: could you add e.g. a small inset showing the location of this map on the globe?

7. Fig. 3: you do not mention the identity of primary producers in the WMA simulation. With the confusing parameter value differences mentioned above I am not sure whether diazotrophs in these simulations only have net competitive advantages over the two other phyto types and would therefore be expected to dominate the WMA simulation? Moreover, instead of the fairly detailed description of the curves it might ease comprehension of the results to point out that in both simulations and observations the Chl a maximum is roughly located at the nutrient-limiting nutricline.

8. Fig. 4: I guess the black line is the simulated MLD. You point out the connection between the MLD and the nutriclines on p11,l1-2. So how realistic is it compared to observations? Moutin et al. 2018 (BGD) present observation-based values from de Boyer Montegut et al. 2004 showing maximum MLD in August (here October) and high Chl from April to August (here October to April). What causes this discrepancy and how does it affect your results?

9. causality between N fixation and preferential P regeneration:

    - p9,l29-p10,l2: "The ... role of DIP availability in controlling N2 fixation ... has been highlighted over the last decade ..., and the consistent results

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between the OUTPACE data and our model outputs, comparing simWGA and simWGY, reinforces this view of the biogeochemical functioning in the region." - You do not test the effects of P availability in the simulations presented, you assume high/sufficient P availability and manipulate N fixation. Your simulations thus show the connection the other way round: without N fixation you have P left over.

- p11,l10-11: as above: the simulations presented do not show that a shallower phosphacline than nitracline was needed to observe N fixation rates in agreement with observations. they show that assuming N fixation leads to this gap between phospha- and nitracline. Could it be that you still had the simulations without preferential P regeneration in mind, that are mentioned on p11,l34-p12,l1?
- p11,l23-25: I have to admit during the first read it took me until here to realize how the difference in phospha- and nutricline depths came about. The preferential P regeneration is a model assumption. Why not mention it already when describing Fig. 3 in the results? If you want to keep it as a result, then simulations without preferential P regeneration might provide evidence for the causality you describe here. The same applies to p12,l10-11.

10. p12,l13-31: This section could be formulated a bit more concisely. Does the 1D model consider any N losses (at the bottom boundary, mimicked advection, ...)? If not I don't find it very surprising that newly fixed N accumulates in a 1D simulation - where else should it go - and would emphasize more the other aspects mentioned in this section.

11. p12,l25: misleading wording: "Our ... results ... show an accumulation of N .. which can only be explained by the new N input ...". This to me suggests that you tested different mechanisms for N accumulation. maybe better: "... accumulation resultion from N fixation".

12. p13,l27: misleading wording: "DIP is never exhausted ... because the lack of iron is hypothesized to prevent N2 fixation." maybe clearer: "DOP is never exhausted ... because the model implicitly assumes iron limitation to prevent N2 fixation."

3 Technical corrections

- title: results from a 1DV biogeochemical-physical coupled model: 1DV, not 1D? a typo?
- Fig. 4: the seasonal Phosphate gradient in the surface layers is difficult to distinguish with the current color scale. Can you find a better one?
- p7,l6-7: excessive dots within the units
- p8,l5: wording: the seasonal variation of the MLD ... It clearly indicates ...
- p8,l29: significant seasonal variation (no s)
- p13,l7: misplaced parentheses: (Olsen et al. 2016) instead of Olsen et al. (2016)
- p13,l8: leave out "the": To date, seasonal variations were ...

3.1 reference formatting

- italics for species names, i.e. Trichodesmium
- N 2 without space and with underscore
- CO2 with underscore

3.2 additional spaces

- p1,l4: after considered
- p2,l1: after NH4+
- p3,l14: after sampled
- p5,l8: after regard

3.3 *Trichodesimum* should be *Trichodesmium*

- p8,l18 & 21
- p10,l23

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