Biogeosciences Discuss.,
https://doi.org/10.5194/bg-2017-559-RC1, 2018

Interactive comment on "Modelling the processes driving *Trichodesmium* sp. spatial distribution and biogeochemical impact in the tropical Pacific Ocean" *by* Cyril Dutheil et al.

Anonymous Referee #1

Received and published: 8 March 2018

General comments The main goal of this study is to model the processes driving the spatial distribution of Trichodesmium N2-fixer in the tropical Pacific Ocean in order to better understand their biogeochemical impact in this oceanic basin. The modelling tool used to achieve this goal is a 3D coupled dynamical-biogeochemical model applied in condition of long-time simulation (20 y). The main innovative point of this study is to develop and validate from field data an explicit formulation of N2 fixation process associated to a Trichodesmium compartment. This formulation is related to the iron intracellular quota in a sophisticated way. In such a configuration the coupled model is able to reproduce, at spatial large scale, the main physical and biogeochemical patterns observed in the dedicated dataset. The assessment of seasonal cycles as that

Printer-friendly version

Discussion paper

of N2 fixation rates is less efficient by the model, may be owing to the use of a climatological physical forcing and spatial low resolution of physical model. At the end of this study, a set of sensitivity tests is presented (i) to check the added value arising from an explicit formulation of the N2 fixation process in the coupled model and (ii) to assess the potential roles of iron fluxes from island sediments on the spatial distributions and biomasses of Trichodesmium in the WTSP. On the whole these sensitivity tests are interesting because they enable to increase our knowledge on the biogeochemical roles of Trichodesmium in the tropical Pacific Ocean and can suggest interesting goals for future field cruises. While this paper is of significant scientific interest, it appears uneven in its writing and its quality. Some sections as introduction, discussion and conclusion are clear and well-written, other ones (i.e. methods, results and appendix especially) show many unclear points or they lack of information crucial to a clear understanding. Therefore, I recommend this manuscript for a publication in BG only following major revisions and thorough answers to my requests. Hereafter I give a set of comments, which will somewhat help Authors to improve their manuscript.

Specific comments

1. Title.

The second part of the title does not really make sense. The biogeochemical impact of what on what exactly? Thus I suggest a slight change on this title as for example 'and the biogeochemical impact of N2 fixation on primary productivity in the tropical Pacific Ocean'.

2. Abstract.

- L24-25: I suggest replacing the word 'compartment' by 'parameterization' or 'formulation'.

- L27: Replace the word 'conditions' by 'fields'. Used in this context, the former term is ambiguous. I guess the author would rather mean 'field data/observations'.

- L34: Replace the sentence '...the spatial distribution and the abundance of...' by 'the spatial distribution of Trichodesmium biomasses in...' Your model does not provide abundance (number of cells) of Trichodesmium but rather its biomass.

3. Introduction. - L52-53: redundant reference to Zehr and Bombar (2015).

- L57: missing word (may be 'known'?) in '... is consequently better (Bergman et al., 2013...'

- L77-79: Author writes about numerical models without indicating references on these models. I would like to see some references in the revised version of the ms.

- L103: '... implications for and...'

On the whole, the state-of-art on how the N2 fixation process is represented in marine plankton models is lacking in this introduction. Some words on this point are given at the beginning of discussion but I think this piece of text should be rather put and developed also in Introduction. Major point to be addressed.

4. Methods.

- L106: the terms 'primary production model' for PISCES are too restrictive. PISCES is a plankton community model. I suggest replacing 'primary production' by 'biogeochemical'.

L115-127: What are exactly the initial, boundary conditions and forcing of the simulation? This point is not clear to me. Indeed, a climatological forcing strategy seems to be used while it is written in the section 2.2. that the reference simulation is launched over twenty years from 1993 to 2003 suggesting the use of realistic physical (and biogeochemical ?) forcing. No information is given on the types of biogeochemical forcing at the model boundary (e.g. atmospheric deposition of nutrients?). This point needs to be clarified. Major point to be addressed.

- L128-155 on the description of the PISCES model and the formulation of the N2-

fixation process. This section has numerous unclear points and omissions weakening the paper as a whole. This section needs to be reworked and strengthened. Major point to be addressed.

- L137: The reference of Kwiatkowski et al. (subm.) is missing in the bibliography while it is a crucial reference to see the so-called quota version of PISCES model. On the web, this reference cannot be found. Is this paper published now?

- L129-141. The description of the model is too succinct. Is there a term of natural mortality on Trichodesmium? What about the trophic interactions between zooplankton and Trichodesmium? Is zooplankton able to graze Trichodesmium or not? From the Table 1 and the parameters inside I can suppose yes but it is not clearly stated and discussed in this section. The two types of zooplankton seem to be able to graze Trichodesmium. No argument is given for this choice. I would like to see a text around this point in the revised ms. Major point to be addressed.

- L142-144: please be careful on the use of the term 'growth rate'. Here, I suppose it is the photosynthesis growth rate and it is not the net growth rate (gains minus losses) of Trichodesmium. Losses are for example grazing or exudation. Please clarify this point.

- L148-150: What is the form of nitrogen released by Trichodesmium? Is it nitrate and/or ammonium or dissolved organic nitrogen? No information is given on this feature while it is crucial to understand how Trichodesmium can be a source of nitrogen for the plankton community. Moreover, no further information is given on the ability of Trichodesmium to release other element like phosphorus, iron while they are also expected to be constitutive of this planktonic genus. Why are the explicit state variables of the Trichodesmium compartment in fact? To clarify all that points I suggest to add and comment, in this section, a new figure of schematic diagram showing state variables and processes of the Trichodesmium compartment. Major point to be addressed.

In this section, no reference to table 1 is indicated while it is the table of some parameters of the biogeochemical model. More problematic is that all the choices of parameter values are nowhere discussed in the paper. No references are given in the Table 1. On what criteria have been chosen the values of all these parameters? Is it an arbitrary choice as the exuded fraction of nitrogen by Trichodesmium or from literature? There is an urgent need to justify the values of each parameter presented. I have also some other remarks on the table 1 (see section on table comments hereafter). As another example we don't know why the preference of microzooplankton for Trichodesmium is higher than that of mesozooplankton. I would like to see in the revised version a detailed section on this point. Major point to be addressed.

Furthermore in the section 2.2 (L159-170), crucial information is lacking about the sediment iron flux while it is a key point debated in the present study. What is the value of this flux and its origin (i.e. literature)? Is it a homogenous spatial flux on the whole model grid or only around some islands? One can imagine for example a decreasing flux from coastal to offshore areas. Neither detailed information nor references to previous works are given. Major point to be addressed.

Section 2.3. An important question arises after reading this part of paper. Have the data of OUTPACE cruise been really used to validate the coupled model? This point is not clear to me because the OUTPACE cruise has been carried out in 2015 year while the reference simulation ends 2013 (L160). Several captions of the figures indicate that 'Model values have been sampled at the same location, the same month and the same depth as data'. So I can't understand how it is possible to use the OUTPACE dataset in this paper to validate model output as suggested l175-176 for iron. Major point to be addressed.

5. Results.

L193. Phosphate patterns. 'in qualitatively good agreement'. Assertion to be moderated. The model strongly underestimates the areas of high concentrations as within the Costa Rica dome and along the equator.

L197. 'Regions most favourable for Trichodesmium can be defined by temperature

Printer-friendly version

Discussion paper

within 26-29°C'. What is the criterion behind this statement? Is it from literature or from a model results (highest biomasses of Trichodesmium)? Author refers these two temperature limits to preferendums in the caption of Fig. 1. How are defined these preferendums?

L201-202. I don't agree with the sentence on the good reproduction of seasonal variability of SST along the equator by the model. The 26°C isotherm migrates by 15° eastward in the model from summer to winter but this migration is not observed in the SST field.

L205. Please indicate the type of statistical test (and probability) used to prove the result of 'no statistical differences'.

L214. 'with mean values higher than 0.3 mgChl m-3'.

L221. 'Those localized chlorophyll . . . effect'. This sentence should be in discussion.

L224-225. 'TRI simulation thus appears. . .' This sentence should be in discussion.

L232. 'SPG'. Acronym not defined.

L236-253. There is neither clear explanation nor associated analysis why the numerical N2 fixation rates are compared with data over two different integration layers (Fig. 4).

L244. Same as my previous remark on the use of OUTPACE iron data in the validation step of model (section 2.3.). How OUTPACE N2 fixation data '(Bonnet et al., this issue)' can be used to validate the model as the simulation ends 2013?

L247. 'In general, ... compared to data'. This sentence is vague and then confusing. Is an overestimation on the whole modelled domain, or only in one sector especially? Is this statement applicable for the rates depth-integrated 0-150m or 0-30m, or both?

L256. The Figure 6 should be numbered 5 instead 6 because it follows the description of Figure 4.

Printer-friendly version

Discussion paper

L261. 'PNG'. Acronym not defined.

L272. Why does Author indicate the term 'not shown' for the simulated N2 fixation rates and Trichodesmium biomass as they are presented on Figures 5 and 7, respectively?

L275. 'Figure (6 a,b)' instead Figure (7a,c).

L284. Same remark as L205.

L286. 'Figure 7a' instead 'Figure 5a'.

L303. Please replace 'in the sampling' by 'in the field observations'.

L306-308. What about the other factors (as grazing or natural mortality if existing)? What is the type of analysis exactly, leading to the conclusion 'the seasonal variability is mainly controlled by primary production'? Please replace the term 'by primary production' by 'by the levels of primary production'.

L312-316. Would it be possible to see (in a new table for example) a synthesis of the modelled values of Trichodesmium growth rates of and a comparison of them with those observed in the field if existing or in lab experiments.

L324-326. 'Indeed, ... temperature'. This sentence should be placed in discussion. Furthermore, this sentence is highly debatable. Please be cautious with the concept of 'ocean dynamics mainly 1D'! Is this feature really achieved anywhere in the ocean? Can the Author firmly prove this assertion in the simulations presented in this study?

6. Discussion.

L369. I would like to see a clear definition of the term 'bio-available nitrogen'. Is it dissolved inorganic forms of nitrogen and/or organic forms also?

L393. 'to that of' instead of 'than'.

L400-403. What is the actual reason for a better modelling of N2 fixation by using an explicit representation of this process in the model? At the place of the manuscript one can expect a deeper analysis of the results. A thorough comparison of the two types of formulation could be lead to explain clearly why using the explicit formulation is an improvement. Is it due, for example, to the inclusion of Fe internal quota in the formulation of Trichodesmium photosynthesis growth rate? Major point to be addressed.

7. Appendix.

In this section all the equations presented should be numbered for clarity. L L477. I suggest to write ‘phosphorus or iron’ rather than ‘phosphorus and iron’.

L479-480. On which basis (literature, experimental works?) the equations of phosphorus and iron limitations have been stated? Are they new formulations? What physiological processes drive the choice of this formulation? I would like to see information on that point in the revised ms.

L480. ‘Nutrient quota for Fe and phosphorus’ rather than ‘Nutrient quota for Fe and PO4’.

L492. Equation of Trichodesmium growth rate if iron limiting. Same remark as for L479-480. Why the N2 fixation growth rate in case of iron limitation is modelled in this way? It is very difficult to evaluate this formulation without explanation! Furthermore, please be careful in using the term ïĄą that can be confused with the term ïĄąI (initial slope of P. vs. I.). It is not clear to me if the value of μTRIMAX is of 0.25 d-1? If yes, please clearly indicate in the table of parameters (Table 1) its value and at l473. The term LI is used while undefined.

L518. While the limiting function by temperature is defined previously (L471, LT), the limiting function by light is not presented and it deserves to show it. What is the exact form of the term LI defined by the author?

L521. Please be cautious in using the terms of ‘new and regenerated production’ in this context. The growth rates of Trichodesmium based on nitrate and ammonium are not strictly speaking new and regenerated productions, respectively. Please reconsider this sentence and formulate your idea with accuracy.

8. Bibliography.

Please check carefully this section. Many typos and different formats.

9. Tables.

Table 1. Major points to be addressed. Why are only presented the parameters of Trichodesmium and nanophytoplankton? Are the parameters of other living biomass compartments remained unchanged (what is the reference in which the unchanged parameters can be found?)? If yes, why those of nanophytoplankton only have been changed? I would like to see in the revised ms explanations on this point. The column 'Name in the code' is useless (technical details) but adding a column with references for each parameter is essential. The important parameter μTRIMAX is missing in the table. Please check carefully the units of each parameter. According to the definition of ïĄć given in Appendix (L501-502), its unit cannot be in d-1. Replace the term 'excretion' by 'exudation' for parameters 'rTri' and 'rI'.

10. Figures captions.

Fig.1. Typo: 'preferendum'.

Fig. 2. Typo '0-150m'.

Fig. 5b. 'The green curve is the average of the seasonal cycle. . .' This sentence is not clear to me. How is this average built exactly? This is no more clearly explained in the corresponding section (l300-301).

Interactive comment on Biogeosciences Discuss., https://doi.org/10.5194/bg-2017-559, 2018.