Biogeosciences Discuss.,
https://doi.org/10.5194/bg-2017-559-RC2, 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.

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Received and published: 25 March 2018

The manuscript describes model simulations without and with two different parameterisations of nitrogen fixation in the tropical Pacific. Results are compared against observations in the ocean's surface layer, and the degree of realism of the two parameterisations employed is discussed. Inferences are made about the role of diazotrophic nitrogen fixation compared to primary production by ordinary phytoplankton.

Overall, the topic is scientifically very interesting and I found the title and also the abstract very promising, but was then disappointed by the material presented in the manuscript (and the often poor way it was presented) for reasons I will explain be-

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low. I am afraid I cannot recommend publication of the manuscript in its present form and think that a very major rewrite and additional and thorough analysis is required. This is beyond what I would normally consider as major revision (and would therefore recommend reject and resubmission). As the issue is tricky with special issues, and because the scientific topic is really interesting and it would be a missed opportunity of not analysing this very carefully, I'm still OK with recommending major submission, but want to stress that 'major' should be taken very seriously.

1 It is impossible to fully understand what has been done

The explicit description of N2 fixation by Trichodesmium is provided in the Appendix. I tried hard to understand it, but admit that I failed. There may be typos or unexplained terms (e.g., what is L^{N}_{Tri} in line 492? Why are there two different definitions of L^{Fe}_{Tri}, lines 479 and 499?). It does not help, that the notation in table 1 seems to be different from the one in the appendix. There are also steps that are not explained or justified. For example line 483 - why is this procedure applied to Fe but not to P? This makes it impossible to understand what has been done and why. There are other models of diazotrophs out in the literature. How does your model relate to these? Why have you developed a new one (is it new?)? To be useful to the scientific community, this has to be presented in much more detail and put into relation to the existing literature.

The authors claim that implicit parameterizations of N2 fixation are often used in biogeochemical models (line 32, line 154, in the final sentence of the manuscript they even say 'more commonly'), but do not provide a single reference to support this claim. I think this strong statement that is used and certainly requires references and also a detailed description of this implicit parameterisation in order to allow the reader to understand some of the results (see below), and possibly repeat what has been done here.

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The set-up of the physical model is unclear as well. line 111 states that it is based on a nested version. Is there a nested version used here? If so, what is the parent and what the child model? Then, in line 116 ff open boundary conditions are introduced. Do these replace the nesting? What does the sentence in line 118 mean "The use of similar ROMS configurations. . .is validated. . ."?

The configuration of the biogeochemical model is not well described. E.g., line 134: a modified version, which differs in the use of a full quota formation. How is it modified? How does it differ? 'variable' Redfield ratios. The Redfield ratio is always constant and always the same (i.e. the one that Redfield used). Replace by variable C:N:P (:Si : Fe:. . .?) ratios. Is the effect of N2 fixation (and denitrification) on alkalinity included in the model? This would be another biogeochemical impact of N2 fixation that should be reported.

In addition to an improved description of N2 fixation, there should also be a description of the growth of diazotrophs as well as their loss terms (grazing, mortality,. . .) and the fate of the fixed N (loss to DOM? Lifetime?)

line 163. Explain why 156E was chosen as western boundary of the test regions without sedimentary iron input? Doesn't this ensure that there is always iron being supplied from the western boundary of the Pacific Ocean?

2 The presentation of the results is often poor and not as convincing as is could and should be

Part of this a language problem. Despite the impressive author list, no careful proofreading seems to have taken place before submission. There are many typos, incorrect words, wrong grammar and incomplete sentences. This can (and should) be improved.

Some explanations are very vague and, at closer inspection, are not that convincing.

For example, line 231/232: The bias ‘beyond’ (presumably ‘eastward of’?) 170W is explained by a bias in iron concentrations, which, however occurs mostly west of 150W according to Fig.2.

Fig. 4 Why show the vertical integral and the vertical average in separate panels? The information looks very similar. Explain what differences the reader should see and understand.

One motivation mentioned in the introduction was the comparison of biogeochemical controls and impacts between implicit and explicit representation of N2 fixation. The only comparisons shown are for surface chlorophyll (quite different) (Is the implicit diazotrophic biomass of the implicit representation included here?) and primary production (very similar). Both variables are biogeochemically among the less relevant ones. Showing a comparison for N2 fixation rates, nutrient concentrations, export production, pCO2 and possibly oxygen would be much closer to the original goal of the paper. In my view, such a comparison is essential.

Fig. 2. Why does the run N2_imp have more chlorophyll along the eastern boundary and along the equator than run TRI? This is interesting and might point to some feedbacks in the system.

The comparison among modeled and measured iron concentrations in Fig.2 is very difficult to see. Try different figure types (larger blobs, overly observed ’blobs’ on modeled map,...) Same for Fig.4

Fig. 9. Are currents on panels c and d different?

3 minor points:

line 326 ‘cools temperature’ is wrong. either lowers temperature or cools the water. line 349. What is meant by high islands?

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