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Interactive comment on "Biogeochemical fluxes and fate of diazotroph derived nitrogen in the food web after a phosphate enrichment: Modeling of the VAHINE mesocosms experiment" *by* A. Gimenez et al.

Anonymous Referee #2

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This manuscript by Gimenez et al., is a modelling study based on experimental data from the VAHINE mesocosm study where dissolved inorganic phosphate was added to support the growth of diazotrophic (N2-fixing) organisms. The particular focus of this study was to track nitrogen fixed by diazotrophs through the food web over longer time periods that could not be studied through experimental work, in addition to quantifying the flux of carbon, nitrogen and phosphorus in the mesocosm system.

Gimenez et al. used a biogeochemical mechanistic model based on the Eco3M-MED for the Mediterranean Sea where N2-fixation was included as a function of enzyme

activity and diazotroph abundances. Using the results of the model, this study reports that diazotroph-derived nitrogen was initially released to the dissolved organic nitrogen (DON) and ammonium (NH4+) pools. Then it was assimilated into the plankton biomass with the majority (43%) in non-diazotrophic plankton after 25 days in the simulation. It is pleasing to see work on incorporating diazotrophy into models, particularly when supported by in depth information from experimental work such as the VAHINE study. Furthermore, phosphate enrichment enhanced N2-fixation, primary production and the export of carbon by 201, 208 and 87%, respectively compared to the non-enriched simulation. However this enhancement effect from phosphate enrichment had a lag of around 10 days, hence the authors highlight this long time period compared to common methods used to determine nutrient limitation which are usually on much shorter time scales. This is a result that I feel will likely be of interest to others working on nutrient limitation and dynamics in aquatic ecosystems. Thus, I recommend publication of the manuscript once issues detailed below have been addressed.

GENERAL COMMENTS:

In this mesocosm study, there was no control mesocosm where no dissolved inorganic phosphorus (DIP) added (Bonnet et al., in review). This is also described in the Methods section (P3, lines 88 – 91) with the non-enriched simulation is "considered as a proxy of the planktonic dynamics outside the mesocoms in lagoon waters" (P9, lines 251 – 252). However from previous large-scale mesocosm experiments, there have been considerable differences between the control mesocosms and the surrounding waters, primarily due to entrainment of different water masses, which cannot occur within the closed mesocosm system (see for example recent ocean acidification studies as Special Issues also in Biogeosciences, http://www.biogeosciences.net/special_issue204.html, http://www.biogeosciences.net/special_issue120.html). Hence while the different phosphate concentrations are acknowledged in the Discussion (P14, lines 424 - 426), potential phosphate inputs into the sampled lagoon waters is not quantified and the jus-

tification for the use of the non P-enriched simulation as a proxy for the lagoon waters is currently weak.

Despite high variability between the mesocosms in the initial conditions through the DIP addition (see for example Fig. 3(a)), it appears as though the DIP addition simulated is not an average of the amount present in all mesocosms, instead more closely fitting the concentrations in M1 and M3. Indeed, the simulation of DIP concentrations in SIME follows closely to the dynamics in M1. This is a potentially interesting result that is currently not given much attention in the manuscript. In addition, there are also some notable deviations in the temporal evolution of various parameters between the model runs and the experimental data e.g. dissolved inorganic phosphate, UCYN-C abundances, and the abundances of small phytoplankton and bacteria (see also Figs. 3-5). Both of these points could be discussed more in depth in the manuscript.

Language: In general, the manuscript reads well and has a logical structure based on the headings and sub-headings, however it would benefit from proofreading by a native speaker as some phrasings and incorrect grammar hinder readability. Some sections are very long and including paragraphs would make it also easier to read as there is a lot of information to take in in one chunk. Brackets are frequently used, but it would make smoother reading if these were better incorporated into the text. Some of these grammatical errors, and others have been highlighted in the specific comments section below.

There is a mixed use of present and past tenses in the Methods section. I would suggest the authors change this to the past tense and describe what was done. The manuscript would benefit from a thorough check for consistent use of either 'mineral' or 'inorganic' and the use of italics for N2 fixation as well as for correct figure numbering. Care also needs to given to the correct and consistent use of capitals (e.g. Cyanobacteria on P2, line 38 and picoEukaryotes on P6, line 172 should read cyanobacteria and picoeukaryotes, respectively) throughout the manuscript, including in the references to figures, tables and equations. These are also currently do not follow clear guidelines

provided by Biogeosciences for figure and table labelling and referencing.

The term "export" is used in reference to the material collected in the sediment trap at around 15 m deep. This is quite a shallow depth considering the euphotic zone which is a commonly used depth for reporting carbon export data. Hence in this study, it appears to more reflect the "sinking flux" or "potential" export rather than export. I recommend that the use of the term "export" be reconsidered to see if this accurately reflects what was measured during study.

Units: Units of nmolN.m-2.d-1 are used and in some sections scientific notation of exponents is incorrect or is not complete (e.g. P12, lines 350 – 356 "5.108 cell.L-1"). The incomplete notation may be a formatting issue that occurred during the proof-reading process. Nonetheless, I would advise that SI units are used and suggest that all data in scientific notation is checked that it is correctly printed as this was distracting when reading the manuscript.

SPECIFIC COMMENTS:

P1, line 9: It is unclear what kind of P was added during this study. It would be clearer if "P" was changed to "inorganic P" in the abstract that to indicate inorganic P was added to the mesocosms.

P1, line 13: It is unclear what is meant by population scale here. Please specify.

P2, line 2: "Pacific ocean" should read ". . .Pacific Ocean. . .".

P2, line 41: The bracket before UCYN-A should be a comma i.e. ". . .Group B, UCYN-A . . .".

P3, line 59: It isn't clear what is meant by affected in ". . .a water mass affected by diazotroph development. . .". Please rephrase.

P3, line 69: The 15 in "d15N" should be as a superscript.

P3, line 75: A bracket is missing at the end of ". . . Group C (UCYN-C)".

P4, line 93: Please specify if “the sediment traps were collected” or instead “the material from the sediment traps were collected”.

P4, lines 112 – 115: Here the sinking velocity was set as constant as 0.7 m/day for the first 10 days but increased according to a polynomial function to up to 10 m/day. Why was this function used and why would the sinking velocity of the particles suddenly increase after 10 days? This appears to have a marked and sudden influence on the model output eg. Fig. 7(c), (f) and (m), that does not seem to fit with the experimental data in Fig. 4.

P5, line 130: Language error - should read ‘When the total N and P pools (Ntotal and Ptotal) were calculated from the model outputs...’.

P5, line 153: Is the zooplankton abundance and C, N, P data from this study?

P5, line 143: Language error – “autotroph phytoplankton” should read “autotrophic phytoplankton”.

P6, lines 167-170: The labile DON fraction is defined as the “. . .quantity consumed during the experiment in the mesocosms which was estimated at 1 umol.L-1”. How does this definition fit with the probable production of DON during the study period? Could this labile fraction be underestimated?

P8, line 222: The number of cells in a trichome for Trichodesmium sp. is variable rather than a consistent number between trichomes. Hence here, I would suggest using the word “assuming” rather than “considering” here.

P9, line 255: It is unclear what is meant by “diversity parameters”. Is this referring to the composition of the plankton community present? Please clarify.

P9, line 257 – 261: The distinction between P0, P1 and P2 is clearly defined in the caption of Fig. 3 but not in the body text. It would be helpful to also have this in the text here as well.

P9, line 273: Should “nmNH4+” instead read “mNH4+”? Is this correct or is this a typing error. Here “remains” should also read “remained”.

P10, line 278: The increase in mDOP in SIME described in the body text is very difficult to distinguish from Fig. 3(c). What was the magnitude of this increase?

P12, lines 370 – 371: Are the growth rates reported in per second (s-1) as described here in the text? Indeed, these seem very high, with reported rates of over 200 cells L-1 s-1 in Fig. 6. Is this correct? Or is ‘x 10-4’ missing from the y-axis? Additionally, it seems that most of the model outputs are quite smooth with minimal variation on a time scale shorter than one day, apart from the turnover time of DIP in the SIME simulation (Fig. 4 (b)). What is the temporal resolution of the model? Is this consistent across all variables included in the model?

P13, lines 387 – 390: According to Fig. 5(b), UCYN-C was present throughout the study period. Why is the UCYN-C proportion of DDN zero at the beginning of the study period, whereas TRICHO starts with 100%?

P14, lines 444-445: “After benefitting diazotrophs, the DDN inputs benefited to non-diazotrophic organisms.” – How does the DDN benefit diazotrophs? This could be more clearly and explicitly phrased.

P14, line 447: The reference to Figure 5(e) appears to be for 4(e) here.

P14, line 452: Here, “synthetized” should read “synthesized”.

P15, lines 454 – 456: “As the model does not represent the diatom-diazotroph associations (DDAs), which were the most abundant diazotrophs in the mesocosms during P1 (Turk-Kubo et al., subm., this issue), the modeled export is probably underestimated during P1.” From looking at Figure 4 (d), (f), and (h), the modelled data appears to have good agreement with the raw data so I am not sure why the authors suggest that the modelled export is probably underestimated.

P15, line 484: The reference to Figure 5(e) appears to be for 4(e) here.

P15, lines 486-487: From Figure 5(b), it appears as though UCYN-C abundances were sampled on day 15, not day 16 as written here.

P15, lines 488-489: This question: “Which factor may explain the 10 days delay between the DIP enrichment and the large UCYN-C development?” seems like it should be a sub-heading rather than in the body text.

P17, lines 531-532: Is the study by Van Wambeke et al., subm. From this mesocosm study? If yes, “. . . confirming previous studies . . . “ does not accurately reflect this.

P19, line 614: The citation “Verity et al.” is missing the year of publication.

P19, lines 627 – 629: “Finally, the overestimation of UCYN-C abundance by the model also supports the idea that UCYN-C sinking is underestimated by the model”. Could underestimated grazing rates also explain this overestimation of UCYN-C abundances in the model?

P20, line 650: This observation of the majority of DDN in the DON and NH4+ pools from the model output is in good agreement with observations from experimental work which is cited in the introduction (P2, line 49). These observations would also fit nicely to the conclusions drawn from this modelling study but are not cited in the context of the discussion here.

Figure 1: Does the model include mortality of all diazotrophs or just Trichodesmium sp.? Is the arrow from N2 to DOM via mineralisation included in the model? If yes, is this mediated by diazotrophs or is this a separate process as indicated in this figure? How is the uptake of DOP by diazotrophs incorporated in the model? Typing errors: “morality” should be “mortality” and “Smal” should be “Small”.

Figure 3: The y-axis label of PO43- in Figure 3 is not consistent with the figure caption which abbreviates phosphate to DIP.

Table 1: How was the living fraction of POM (i.e. “POC-living”) calculated?

Table 2: The superscripts on NO3, NH4 and PO4 should instead be subscripts in both the “Parameter” and “Definition” columns. “Exsudation” should also read “Exudation”. I would recommend adding “cell” to the “minimum quota of . . .” to read “minimum cell quota of . . .”.

REFERENCES:

Bonnet, S., Moutin, T., Rodier, M., Grisoni, J. M., Louis, F., Folcher, E., Bourgeois, B., Boré, J. M., and Renaud, A.: Introduction to the project VAHINE: VAriability of vertical and tropHIc transfer of diazotroph derived N in the south wEst Pacific, Biogeosciences Discuss., doi:10.5194/bg-2015-615, in review, 2016.

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