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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 #2

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The study entitled "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 mode" by Gimenez et al. examines the role of diazotrophy in the western south Pacific ocean. The authors use a 1DV ecosystem model that is run in two different configurations at the same station that differ by the representation of diazotrophy. Their main conclusions are that nitrogen fixation sustains a significantly higher productivity, explains the consumption of DIP and induces

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significant seasonal variations. Furthermore, they claim that a decoupling between the depth of the phosphacline and nitracline is necessary to induce nitrogen fixation. This manuscript addresses a very important scientific question: The potentially critical role of nitrogen fixation in Low Nutrient-Low Chlorophyll areas. It is relatively well written, clear and thus it deserves publication in Biogeosciences. However, some important issues have to be addressed before. In particular, some of the major conclusions proposed in that study are not sufficiently demonstrated and thus, require more analysis.

A strong point is made on the decoupling between the phosphacline and nitracline to explain nitrogen fixation. In fact, this is not clearly demonstrated in the study. It only shows that nitrogen fixation explains the lack of DIP accumulation at the surface. However, the impact of this decoupling on nitrogen fixation is not analysed. This would require to manipulate the depths of the nitracline and phosphacline independently and to study the consequences on nitrogen fixation. In the discussion section, the authors mention that they have done some sensitivity tests by altering the degradation rates of P-rich organic matters (DOP, POP), but the results of these tests are not shown. Yet, this would support their conclusions.

My second concern is on the model setup. First, they use the same physical conditions and state that the differences between WMA and WGY are only explained by the presence or lack of diazotrophy. That's quite a strong assumption that should be better discussed. Furthermore, the physical setup is not sufficiently described. For instance, they explain that they used the output of a WRF configuration to force their 1D ocean model without clearly describing the atmospheric model setup, the region that has been selected in this atmospheric model, how they have averaged (or not) the atmospheric forcing fields, . . . I don't expect a full dynamical validation of the physical state predicted by the physical 1D ocean model but some additional information are necessary. Finally, they have made some significant changes in their ocean biogeochemical models (two particles size classes, modified parameter values). They should explain why these changes have been made and how the parameter values have been

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chosen (fine tuning, assimilation, basic assumptions, . . .).

My third concern is more subjective. I find the paper too descriptive and not quantitative enough. I would have liked to see a better quantification of the impact of nitrogen fixation on the system. Some budgets would have been interesting to present. For instance, how much of the PP is being sustained by nitrogen fixation directly (PP by UCYN and TRI) and indirectly (fresh input of N excreted by diazotrophs). How does N input from nitrogen fixation compare to export production? How sensitive are the model predictions to the parameter values, to UCYN and TRI descriptions, to DOP/POP dynamics? The latter question is partly related to my previous concerns.

To conclude on my mains concerns, I think that the authors should improve the analysis, perform some sensitivity tests, and better justify their choices and conclusions before this manuscript becomes suitable for publication.

Specific concerns:

P3, lines 24-27: This relates to one of my major concerns. The authors chose the same physical conditions to study two different sites. This is quite a strong assumption and this should be better justified. At present, I would consider that the study investigates the role of diazotrophy at a single location (which remains to be clearly stated, see below) rather than the differences between two sites.

P4, lines 20-21: I don't really understand what means TRI are equivalent to 100PHYL. I think that some more explanations in that paper would help the reader.

P5, lines 18-22: This is not clear enough. What is the model setup of WRF? Over which region and what time period have been averaged the atmospheric fields? How well does the physical model perform compared to actual in situ conditions?

P6, section 3.1.1: A major difference between the two simulations is the lack of DIP accumulation in the top 200m or so of the water column when diazotrophy is activated. I understand that in the top 50 or 70m of the water column where nitrogen fixation is significant. However, below that depth range, DIP consumption is being increased in WMA without any significant N fixation. This should be explained.

P7, section 3.1.2: In the WGY setup, a very deep but intense DCM is predicted which is as strong as in the WMA configuration. Yet PP (and thus phytoplankton growth rates) is very very small in the DCM. How can you explain that, since grazing rates should be similar?

P8, sections 3.2.1 and 3.2.2: I understand that the lack of data prevents a detailed and complete validation of the seasonal dynamics predicted by the model. However, is it really impossible to do some basic validation using for instance satellite data for Chl, historical data for nutrients averaged over a regional box which would be valid since the model setup is not representative of a specific station but rather of a broad region.

P8, lines 16-18: I don't understand that statement. I don't understand why the vertical resolution of the fluxes is not the same as the vertical resolution of the state variables. It should be clarified.

P 9, section 4.1.1: the study suggests that DIP accumulation might by explained by the lack of nitrogen fixation. However, it does not explain why there are small rates of nitrogen fixation at WGY.

P10, lines 21-26: The authors performed an additional sensitivity run in which they suppress TRI but not UCYN. In that case, the DCM is shallower and the model skill is improved. However, does that lead to a complete exhaustion in DIP at the surface? In that case, the conclusion of the paper would be quite different since it would mean that low rates of N fixation as observed in WGY do not explain the DIP accumulation. Results from that sensitivity test should be presented in the manuscript.

P11, lines 10-21: the results of the sensitivity experiment mentioned in that paragraph should be included in the study as they directly support one the main conclusions, i.e. the decoupling between the phosphacline and the nitracline explains the high N fixation rates at WMA.

P13, section 4.4: This section is a little bit too long and remains very descriptive.

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