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Interactive comment on "Contrasting juxtaposition of two paradigms for diazotrophy in an Earth System Model of intermediate complexity" *by* Ulrike Löptien and Heiner Dietze

Anonymous Referee #1

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A premise of this paper is that two version of a model with different assumptions about diazotrophy and which have similar patterns of nitrogen fixation in the current day, have very different responses in a future climate scenario. This is a useful comment to make in terms of modeling of climate change impacts. The fact that different assumptions can lead to similar patterns of diazotrophy has indeed been seen before (e.g. Landolfi et al 2015). However, that future changes lead to different outcomes has not been documented to my knowledge. Given this premise, I would like to be supportive of this paper. However, there are several aspects that I have problems with, or that I think are too simplistic. And in the final assessment I am not convinced they have proved the premise. I am not convinced that these issues can be resolved. As such I do not

recommend publication.

1) The two paradigm concept is far too simplistic, and I believe unrealistic. See reviews by Sohm et al (2011) and more recent by Zehr and Carpone (2020), where much of the discussion of controlling mechanisms is focused on iron/phosphate availability perspective. In particular, the importance of iron is neglected in these paradigms and likely to be a more important than either grazing or phosphorus demands (see e.g. Ward et al 2013; Schlosser et al 2014). Moreover, I am not convinced that the "selective grazing" is a paradigm used by many models as stipulated (further references would be needed to show this to be true "de facto standard", line 247 needs substantiated). Early models of diazotrophy were based on Trichodesmium, which indeed appears to have lower grazing pressure and thus earlier models may have incorporated this type of parameterization. But it is now known that there is a great variety of diazotrophs (see e.g. Zehr et al 2020) and many do not appear to be grazed less than other phytoplankton. So this "paradigm" appears highly flawed. In fact, a study cited in this paper, Wang et al (2019) show a case where parameterizing reduced grazing on diazotrophs led to an unrealistic distribution of diazotrophs. It appears that such results are also found in this study (line 205-206). So why even make this a "paradigm"? Similarly, I am not convinced that the P-demand paradigm is fully justified. The study by Landolfi et al (2015) appears to have a very different parameterization of phosphate acquisition. It would seem that at least an iron paradigm should have been included (instead). Line 244: "...exploring two paradigms that are proposed in the literature" is too strong a statement. These do not appear to be the major paradigms that have been put forth (see reviews suggested above). Given the diversity of diazotrophs, it is likely that many processes lead to nitrogen fixation patterns, and expecting any single paradigm to explain them is simplistic. And as such, the setup of the paper appears fatally flawed.

2) Line 95: would have been better to be clearer what you mean by iron being not being explicitly resolved. Does that mean iron concentration are imposed? (I note that

it is not a state variable). Given that iron is likely important in controlling diazotroph distributions, this suggests in itself that this is not the best model for exploring controls on diazotrophy

3) All simulations (REF, OLIGO, GRAZ) have the assumption that diazotrophs do not grow above a temperature threshold and that they are dis-advantaged in nitrate-replete water (though this latter parameter is one that is explored, but only within a narrow range). Could it be that these two assumption are responsible for the similarities between the simulations in the current day model ocean. That is: any other assumptions (as in OLIGO or GRAZ) are slave to these other very strong restrictions. And then that it is the expansion of warmer, lower nitrate conditions in future change simulation that allow the two simulations to diverge. Put another way, these other assumptions (temp, NO3 handicap) are stronger controllers of the nitrogen fixation. So a "bad" parameterization is constrained by the temperature/handicap assumptions in such a way that it doesn't show up until warming occurs. This does not totally detract from the premise of the paper, but it does suggest that a "bad" parameterization could lead to unrealistic future projections. Which is an important difference to the premise.

4) The above also leads to the question on how reasonable the temp/handicap parameterizations are? There are cold water diazotrophs – as suggested by Harding et al (2018), by diazotrophy in places such as the Baltic Sea, and as shown in the Wang et al (2019) estimates shown in Fig 1d? A modelling study (Monteiro et al 2011) has shown that temperature does not need to be invoked to explain diazotroph distribtions. By constraining diazotrophy by temperature you have forced it to be close to observations, but not necessarily for the right reasons. How necessary is the NO3-replete handicap? I feel as though these two parameterizations should be far more fully understood before taking on this type of "paradigm" project.

5) Using Wang et al (2019) for the skill assessment seems awkward since Wang et al (2019) is itself a model estimate. Though data constrained, I would suggest it is not a good benchmark. Deutch et al (2007) was also a "data constrained" estimate

and it is very different to that found in Wang et al (2019). Wang et al (2019) is far more believable and a better study, but this example does suggest that there remains significant level of uncertainty even in a data constrained model,

6) Why not show the future diazotroph/nitrogen fixation distributions? Does GRAZ become unrealistic? I felt that since this was the crux of the premise, this last part of the paper was very rushed through: paragraphs only and one very simple figure. There is a mention of Bay of Biscayne feature (line 240), but this is not shown and appears rather arbitrary.

Details: Line 47: By "supply" do you mean "concentrations"? I would agree that concentrations do not necessarily correlate – but do not think that studies have shown the "supply" doesn't correlate as it is so difficult to measure supply rates. Line 89: Do you mean "DIC" not "DIN" Line 191: "desert dwellers" does not seem appropriate term here Line 193/256: The use of the word "quota" does not seem right here. Do you mean "ratio" instead? Line 194: I do not understand what you mean here? How do they go below zero? Line 218: why do discuss only phosphate here. The changes to nitrate are also important to the issue under discussion.

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