Anonymous Referee #2

General Comments

A: We thank the reviewer for the time and effort. We find all comments extremely helpful and are convinced that they will help us to improve our manuscript substantially.

R: The paper looks at two different formulations of Nitrogen fixation, which are then fitted to the nitrogen fixation observations to obtain the best solution. They show both formulations can adequately represent observations today but deviate when using the RCP8.5 future scenario. I like the inverse approach to parameterising the two N2-fixation formulations using observations. Interestingly, both formulations can represent today's limited N2-fixation data. However, to make the study more complete and justify publication, it needs expanding to address the following issues.

R: 1. It is not clear what observations are used to constrain N2-fixation formulations. It is stated that both models faithfully capture the other key BGC fields like NO3, PO4 and oxygen. However, you should show and quantify how well these fields are simulated by the best parameters of your two N2 fixation formulations. What are the differences? How about differences in DIC and air sea carbon fluxes, and volume of anoxic water? Do the differences provide any insight into the suitability of the 2 different formulations? No, can be the answer, but it would be helpful to show this more explicitly.

A: Agreed. We will add respective information to the revised version of the manuscript. We used estimates of nitrogen fixation to "tune" (poorly) known model parameters that are associated to the numerical representation of diazotrophy in the model. To first order this does have little effect on PO4, nitrogen and DIC because in our model, the stoichiometry (i.e., P:N:C) is identical for diazotrophs and ordinary phytoplankton, with the only exception being that diazotrophs are capable of "filling up" their intracellular N:P ratio up to Redfield even in the absence of NO3. Thus, NO3 is to a certain extend "produced" by diazotrophs (in contrast to ordinary phytoplankton). To this end, it seems straightforward to use global NO3 concentrations as a major constrain for the tuning of the diazotroph model formulations. The problem here is, however, that simulated anoxia and, in turn, denitrification rates are typically flawed as a consequence of an apparently endemic problem in the ocean circulation component of the current generation of global coupled ocean-circulation biogeochemical models (see Dietze & Löptien (2013), Getzlaff & Dietze (2013)). By fitting the diazotrophs to NO3 concentrations one risks to get the right answer for the wrong reason. We realize by the reviewer comment that this is

complex and will need a thorough discussion in the revised version of the manuscript.

2. Typically in applying an inverse approach one considers other observations that were not used to constrain the model to assess the solutions. Here the future projected response is used, but you should consider other potential sources of information. A couple of ideas are: 1) does/would N15 differ between the two models?; 2) do any of the other BGC fields, like the ones listed in 1, differ significantly in the two formulations?; 3) does the ocean carbon uptake differ?; 4) does the response to ocean variability differ (e.g. ocean variability from atmospheric forcing of the last 5 decades)?;What I'm looking for is some guidance on whether other features of the two N2 simu-lations could provide useful insight to access their suitability and direct where to target future observations. Looking at natural variability in the ocean is one way to provide insight into how the two formulations respond in a way that could be assessed against our current understanding and observations. You should add this to the paper. I would also say that relying on more N2 fixation data would not enable one to choose the most suitable N2 formulation now since the simulated N2 fixation fields look similar.At what point in the future do the differences become significant? Is it the pattern ort he total amount of N2 fixation that is most helpful in differentiating between the two formulations?

A; **All these suggestions are very constructive. They make sense to us and are pretty straightforward to address. We will add a respective discussion and analysis to the revised version of the manuscript.**

R: 3. In the simulated future projection, the study only shows the global N2 fixation response of the two formulations, but do other BGC fields show significant differences too? How does the spatial distribution of N2 fixation change? Does an increase in N2 fixation significantly change ocean carbon uptake, equatorial net primary production, volume of anoxic water? Both the change in the amount and distribution of N2 fixation can impact the other BGC fields and fluxes in important ways - does this occur? I'm looking for reasons for why I should care about the future N2 fixation response? I assume the projected differences in the N2 fixation have impacts on the ocean BGC behaviour - it would be great if you showed it.

A: One may indeed argue that, ultimately, the macronutrient PO4 (rather than the macronutrients NO3 and ammonium) is controlling the autotrophic growth - or at least that is how the majority of state-of-the-art models used in coupled ocean-circulation biogeochemical model configurations are constructed. In a nutshell, most models have the following pattern engrained: in the presence of PO4 (and light, iron ...), diazotrophs grow if there is no or only little NO3 - otherwise ordinary phytoplankton outcompetes the diazotrophs. Thus, one might indeed argue that P-based models are sufficient to address most

biogeochemical questions on a global scale: if there is PO4 (and light, iron ...) then autotrophic growth takes place, oxygen and organic matter are produced, organic matter is exported to depth where it's remineralization consumes oxygen etc. Among the reasons to consider NO3 nevertheless are: (1) academic curiosity over the question if the oceanic N-inventory is in balance (and if so on which timescales), brought up by Gruber and Sarmiento 1997, (2) the nitrogen cycle (including all sources and sinks) needs to be comprehensively understood if oceanic sources of the powerful greenhouse gas nitrous oxide are to be quantified, (3) management efforts to limit eutrophication in coastal region need to consider both, N and P, (4) blooms of nitrogen fixing cyanobacteria can be toxic which is an issue in some coastal areas where it can harm assets like tourism and fisheries, (4) a comprehensive understanding of diazotrophy is essential for non-constant Redfield ratio modelling which, in-turn, may well prove to be essential to reliable projections of biotic carbon uptake in a warming ocean (roughly speaking because the responses of C:P ratios in a warming world may differ between diazotrophs and ordinary phytoplankton).

However, we agree with the reviewer that we did not make our motivation entirely clear and also the manuscript would strongly benefit from an additional motivation. Following the reviewers, advice we started additional analysis. In accordance with our argumentation above we found that the respective projections are robust for many metrics. But there are exceptions. Among them a profound impact of the considered paradigms on the projected suboxic volume. We will include these new results into the revised version of the manuscript and thank the reviewer for the good suggestion.

<u>*A few detail comments*</u>

R: line 15, nitrogen is also abundant in the ocean too
A: True. We will clarify this.

R: line 19, not in the air but dissolvedin the ocean
A: Agreed. We will change that.

R: line 22, what input? state it is the added Bioavailable nitrogen
A: We will clarify this.

R: line 31, not clear what is vicious cycles is - expand
A: We will add a brief explanation.

R: line 133 - only fit N2-fixation? how well do you simulated other BGC fields and fluxes?

A: As the chosen parameters refer specifically to diazotrophs, we tuned the model performance indeed based on observational estimates of nitrogen fixation only (because other oceanic state variables might depend crucially on other model parameters which are not "tuned"). We agree that such a choice contains inevitably to a subjective element.

That said, we will discuss the respective model performances also with respect to other BGC fields and fluxes in the revised version of the manuscript. The reviewer is correct in pointing out that this is important information.

R: line 255 - observations show very low biomass of N2 fixers - is this believable? the two N2 formulations differ in the projected response of N2 fixation to global warming but could we use ocean variability over the past few decades to determine which one is more realistic?

A: Good point. We will look into this and add a respective discussion to the revised version of the manuscript.