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Interactive comment on "In depth characterization of diazotroph activity across the Western Tropical South Pacific hot spot of N_2 fixation" *by* Sophie Bonnet et al.

Anonymous Referee #3

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The manuscript presented by Bonnet et al. reports N2 fixation rate measurements and diazotroph abundances from the Western tropical South Pacific. Complementary single-cell measurements of the two most abundant diazotrophs reveal the biogeochemical importance of each of these organisms in this region. Since this manuscript reports a subset of data collected on the OUTPACE cruise, these measurements are analyzed in correlation to a comprehensive set of nutrients, including dissolved iron, and other biogeochemical parameters. The manuscript is well written and the conclusions of this manuscript are appropriately based on the presented data. This new data is a nice addition of N2 fixation rate measurements in relation to biogeochemical parameters and a contribution of individual organisms (not solely based on abundances)

that will ultimately help confine the extent and magnitude of N input by N2 fixation into the global ocean. I only have a few comments (please see below).

Abstract: Maybe the authors could add a little more discussion/conclusions to the abstract as it currently reads almost like results only.

p 3, l 5: I would use either ammonia (NH3) or ammonium (NH4+) p 3, l 6: Isn't this 'nif genes' rather than 'nifH genes'? p 3, l 7: anammox uses nitrite and ammonium as substrates, maybe those could be added to the fixed N? Section 2.2: As far as I understand, the authors used the time-zero samples rather than incubated controls as the natural abundance value in the N2 fixation rate calculation. In many cases, this is OK; however, I have also seen quite large changes in the natural abundance over time in incubated samples/bottles without the addition of stable isotope. These are usually the result of fractionation during the incubation time, e.g. due to the uptake of residual nitrate or remineralization of organic material. The fractionation effects can lead to higher or lower d15N values of the natural abundances. In the absence of incubated controls, the detection limits of N2 fixation might be a bit worse than if those values are available. I would therefore recommend that the authors add their detection limits to the manuscripts, such as the minimum change in d15N that was used as a cutoff for a significant 15N enrichments or reporting the actual d15N values measured in their incubations and the time-zero values. This would also be coherent with general criticisms brought up in the recent paper by Gradoville et al. (2017; DOI: 10.1002/lno.10542).

Section 2.4: Was the at% 15N in the N2 pool measured here as well?

Section 3.4.: Was primary production measured on this cruise? Based on the description in this section, it sounds to me as if N2 fixation somewhat scales with productivity and/or turnover of organic material.

p 15, l 5: I assume that the "%" dropped from the 9.7? If not, does that mean that more organic matter is exported than produced at a given point in time?

p 15, l 14: With respect to the structure of the sections, I would almost move the entire section 4.2 here, as the rest of the discussion nicely scales from a more detailed and organism-centric discussion to a more system-oriented discussion. This would also have the side effect that the end of your discussion is not quite as focused on so many references that are related to other OUTPACE data which are not actually presented here.

Table 1: Any idea on why the d15N value is so low for stn 11 (i.e. -7.05 ‰? Do you have any depth distribution of the d15N values for PN?

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